

British Association for the Advancement of Science.

BELFAST, 1902.

ADDRESS

BY

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PRESIDENT.

THE members of an Association whose studies involve perpetual contemplation of settled law and ordered evolution, whose objects are to seek patiently for the truth of things and to extend the dominion of man over the forces of nature, are even more deeply pledged than other men to loyalty to the Crown and the Constitution which procure for them the essential conditions of calm security and social stability. I am confident that I express the sentiments of all now before me when I say that to our loyal respect for his high office we add a warmer feeling of loyalty and attachment to the person of our Gracious Sovereign. It is the peculiar felicity of the British Association that, since its foundation seventy-one years ago, it has always been easy and natural to cherish both these sentiments, which indeed can never be dissociated without peril. At this, our second meeting held under the present reign, these sentiments are realised all the more vividly, because, in common with the whole empire, we have recently passed through a period of acute apprehension, followed by the uplifting of a national deliverance. The splendid and imposing coronation ceremony which took place just a month ago was rendered doubly impressive both for the King and his people by the universal consciousness that it was also a service of thanksgiving for escape from imminent peril. In offering to His Majesty our most hearty congratulations upon his singularly rapid recovery from a dangerous illness, we rejoice to think that the nation has received gratifying evidence of the vigour of his constitution, and may, with confidence more assured than before, pray that he may have length of happy and prosperous days. No one in his wide dominions is more competent than the King to realise how much he owes, not only to the skill of his surgeons, but also to the equipment which has been placed in their hands as the combined result of scientific investigation in many and diverse directions. He has already displayed a profound and sagacious interest in the discovery of methods for dealing with some of the most intractable maladies that still baffle scientific penetration; nor can we doubt that this interest extends to other forms of scientific investigation, more directly connected with the amelioration

of the lot of the healthy than with the relief of the sick. Heredity imposes obligations and also confers aptitude for their discharge. If His Majesty's royal mother throughout her long and beneficent reign set him a splendid example of devotion to the burdensome labours of State which must necessarily absorb the chief part of his energies, his father no less clearly indicated the great part he may play in the encouragement of science. Intelligent appreciation of scientific work and needs is not less but more necessary in the highest quarters to-day than it was forty-three years ago, when His Royal Highness the Prince Consort brought the matter before this Association in the following memorable passage in his Presidential Address: 'We may be justified, however, in hoping that by the gradual diffusion of science and its increasing recognition as a principal part of our national education, the public in general, no less than the legislature and the State, will more and more recognise the claims of science to their attention; so that it may no longer require the begging box, but speak to the State like a favoured child to its parent, sure of his paternal solicitude for its welfare; that the State will recognise in science one of its elements of strength and prosperity, to protect which the clearest dictates of self-interest demand.' Had this advice been seriously taken to heart and acted upon by the rulers of the nation at the time, what splendid results would have accrued to this country! We should not now be painfully groping in the dark after a system of national education. We should not be wasting money, and time more valuable than money, in building imitations of foreign educational superstructures before having put in solid foundations. We should not be hurriedly and distractedly casting about for a system of tactics after confrontation with the disciplined and co-ordinated forces of industry and science led and directed by the rulers of powerful States. Forty-three years ago we should have started fair had the Prince Consort's views prevailed. As it is, we have lost ground which it will tax even this nation's splendid reserves of individual initiative to recover. Although in this country the king rules, but does not govern, the Constitution and the structure of English society assure to him a very potent and far-reaching influence upon those who do govern. It is hardly possible to overrate the benefits that may accrue from his intelligent and continuous interest in the great problem of transforming his people into a scientifically educated nation. From this point of view we may congratulate ourselves that the heir to the Crown, following his family traditions, has already deduced from his own observations in different parts of the empire some very sound and valuable conclusions as to the national needs at the present day.

Griffith—Gilbert—Cornu.

The saddest yet the most sacred duty falling to us on such an occasion as the present is to pay our tribute to the memory of old comrades and fellow-workers whom we shall meet no more. We miss to-day a figure

that has been familiar, conspicuous, and always congenial at the meetings of the British Association during the last forty years. Throughout the greater part of that period Mr. George Griffith discharged the onerous and often delicate duties of the assistant general secretary, not only with conscientious thoroughness and great ability, but also with urbanity, tact, and courtesy that endeared him to all. His years sat lightly upon him, and his undiminished alertness and vigour caused his sudden death to come upon us all with a shock of surprise as well as of pain and grief. The British Association owes him a debt of gratitude which must be so fully realised by every regular attender of our meetings that no poor words of mine are needed to quicken your sense of loss, or to add to the poignancy of your regret.

The British Association has to deplore the loss from among us of Sir Joseph Gilbert, a veteran who continued to the end of a long life to pursue his important and beneficent researches with untiring energy. The length of his services in the cause of science cannot be better indicated than by recalling the fact that he was one of the six past Presidents boasting fifty years' membership whose jubilee was celebrated by the Chemical Society in 1898. He was in fact an active member of that Society for over sixty years. Early in his career he devoted himself to a most important but at that time little cultivated field of research. He strove with conspicuous success to place the oldest of industries on a scientific basis, and to submit the complex conditions of agriculture to a systematic analysis. He studied the physiology of plant life in the open air, not with the object of penetrating the secrets of structure, but with the more directly utilitarian aim of establishing the conditions of successful and profitable cultivation. By a long series of experiments alike well conceived and laboriously carried out, he determined the effects of variation in soil, and its chemical treatment—in short, in all the unknown factors with which the farmer previously had to deal according to empirical and local rules, roughly deduced from undigested experience by uncritical and rudimentary processes of inference. Gilbert had the faith, the insight, and the courage to devote his life to an investigation so difficult, so unpromising, and so unlikely to bring the rich rewards attainable by equal diligence in other directions, as to offer no attraction to the majority of men. The tabulated results of the Rothamsted experiments remain as a benefaction to mankind and a monument of indomitable and disinterested perseverance.

It is impossible for me in this place to offer more than the barest indication of the great place in contemporary science that has been vacated by the lamented death of Professor Alfred Cornu, who so worthily upheld the best traditions of scientific France. He was gifted in a high degree with the intellectual lucidity, the mastery of form, and the perspicuous method which characterise the best exponents of French thought in all departments of study. After a brilliant career as a student, he was chosen at the early age of twenty-six to fill one of the enviable positions

more numerous in Paris than in London, the Professorship of Physics at the École Polytechnique. In that post, which he occupied to the end of his life, he found what is probably the ideal combination for a man of science—leisure and material equipment for original research, together with that close and stimulating contact with practical affairs afforded by his duties as teacher in a great school, almost ranking as a department of State. Cornu was admirable alike in the use he made of his opportunities and in his manner of discharging his duties. He was at once a great investigator and a great teacher. I shall not even attempt a summary, which at the best must be very imperfect, of his brilliant achievements in optics, the study of his predilection, in electricity, in acoustics, and in the field of physics generally. As a proof of the great estimation in which he was held, it is sufficient to remind you that he had filled the highest presidential offices in French scientific societies, and that he was a foreign member of our Royal Society and a recipient of its Rumford medal. In this country he had many friends, attracted no less by his personal and social qualities than by his commanding abilities. Some of those here present may remember his appearance a few years ago at the Royal Institution, and more recently his delivery of the Rede Lecture at Cambridge, when the University conferred upon him the honorary degree of Doctor of Science. His death has inflicted a heavy blow upon our generation, upon France, and upon the world.

The Progress of Belfast.

A great man has observed that the 'intelligent anticipation of events before they occur' is a factor of some importance in human affairs. One may suppose that intelligent anticipation had something to do with the choice of Belfast as the meeting-place of the British Association this year. Or, if it had not, then it must be admitted that circumstances have conspired, as they occasionally do, to render the actual selection peculiarly felicitous. Belfast has perennial claims, of a kind that cannot easily be surpassed, to be the scene of a great scientific gathering—claims founded upon its scientific traditions and upon the conspicuous energy and success with which its citizens have prosecuted in various directions the application of science to the purposes of life. It is but the other day that the whole nation deplored at the grave of Lord Dufferin the loss of one of the most distinguished and most versatile public servants of the age. That great statesman and near neighbour of Belfast was a typical expression of the qualities and the spirit which have made Belfast what it is, and have enabled Ireland, in spite of all drawbacks, to play a great part in the Empire. I look round on your thriving and progressive city giving evidence of an enormous aggregate of industrial efforts intelligently organised and directed for the building up of a sound social fabric. I find that your great industries are interlinked and interwoven with the whole economic framework of the Empire, and that you are silently and irresistibly compelled to harmonious co-operation by practical considerations

acting upon the whole community. It is here that I look for the real Ireland, the Ireland of the future. We cannot trace with precision the laws that govern the appearance of eminent men, but we may at least learn from history that they do not spring from every soil. They do not appear among decadent races or in ages of retrogression. They are the fine flower of the practical intellect of the nation working studiously and patiently in accordance with the great laws of conduct. In the manifold activities of Belfast we have a splendid manifestation of individual energy working necessarily, even if not altogether consciously, for the national good. In great Irishmen like Lord Dufferin and Lord Roberts, giving their best energies for the defence of the nation by diplomacy or by war, we have complementary evidence enough to reassure the most timid concerning the real direction of Irish energies and the vital nature of Irish solidarity with the rest of the Empire.

Belfast has played a prominent part in a transaction of a somewhat special and significant kind, which has proved not a little confusing and startling to the easy-going public. The significance of the shipping combination lies in the light it throws on the conditions and tendencies which make such things possible, if not even inevitable. It is an event forcibly illustrating the declaration of His Royal Highness the Prince of Wales, that the nation must 'wake up' if it hopes to face its growing responsibilities. Belfast may plead with some justice that it, at least, has never gone to sleep. In various directions an immense advance has been effected during the twenty-eight years that have elapsed since the last visit of the British Association. Belfast has become first a city and then a county, and now ranks as one of the eight largest cities in the United Kingdom. Its municipal area has been considerably extended, and its population has increased by something like 75 per cent. It has not only been extended, but improved and beautified in a manner which very few places can match, and which probably none can surpass. Fine new thoroughfares, adorned with admirable public institutions, have been run through areas once covered with crowded and squalid buildings. Compared with the early fifties, when iron shipbuilding was begun on a very modest scale, the customs collected at the port have increased tenfold. Since the introduction of the power-loom, about 1850, Belfast has distanced all rivals in the linen industry, which continues to flourish notwithstanding the fact that most of the raw material is now imported, instead of being produced, as in former times, in Ulster. Extensive improvements have been carried out in the port at a cost of several millions, and have been fully justified by a very great expansion of trade. These few bare facts suffice to indicate broadly the immense strides taken by Belfast in the last two decades. For an Association that exists for the advancement of science it is stimulating and encouraging to find itself in the midst of a vigorous community, successfully applying knowledge to the ultimate purpose of all human effort, the amelioration of the common lot by an ever-increasing mastery of the powers and resources of Nature.

Tyndall and Evolution.

The Presidential Address delivered by Tyndall in this city twenty-eight years ago will always rank as an epoch-making deliverance. Of all the men of the time, Tyndall was one of the best equipped for the presentation of a vast and complicated scientific subject to the mass of his fellow-men. Gifted with the powers of a many-sided original investigator, he had at the same time devoted much of his time to an earnest study of philosophy, and his literary and oratorical powers, coupled with a fine poetic instinct, were qualifications which placed him in the front rank of the scientific representatives of the later Victorian epoch, and constituted him an exceptionally endowed exponent of scientific thought. In the Belfast discourse Tyndall dealt with the changing aspects of the long unsettled horizon of human thought, at last illuminated by the sunrise of the doctrine of evolution. The consummate art with which he marshalled his scientific forces for the purpose of effecting conviction of the general truth of the doctrine has rarely been surpassed. The courage, the lucidity, the grasp of principles, the moral enthusiasm with which he treated his great theme, have powerfully aided in effecting a great intellectual conquest, and the victory assuredly ought to engender no regrets.

Tyndall's views as a strenuous supporter and believer in the theory of evolution were naturally essentially optimistic. He had no sympathy with the lugubrious pessimistic philosophy whose disciples are for ever intent on administering rebuke to scientific workers by reminding them that, however much knowledge man may have acquired, it is as nothing compared with the immensity of his ignorance. That truth is indeed never adequately realised except by the man of science, to whom it is brought home by repeated experience of the fact that his most promising excursions into the unknown are invariably terminated by barriers which, for the time at least, are insurmountable. He who has never made such excursions with patient labour may indeed prattle about the vastness of the unknown, but he does so without real sincerity or intimate conviction. His tacit, if not his avowed, contention is, that since we can never know all it is not worth while to seek to know more ; and that in the profundity of his ignorance he has the right to people the unexplored spaces with the phantoms of his vain imagining. The man of science, on the contrary, finds in the extent of his ignorance a perpetual incentive to further exertion, and in the mysteries that surround him a continual invitation, nay, more, an inexorable mandate. Tyndall's writings abundantly prove that he had faced the great problems of man's existence with that calm intellectual courage, the lack of which goes very far to explain the nervous dogmatism of nescience. Just because he had done this, because he had, as it were, mapped out the boundaries between what is knowable though not yet known and what must remain for ever unknowable to man, he did not hesitate to place implicit reliance on the progress of which man is

capable, through the exercise of patient and persistent research. In Tyndall's scheme of thought the chief dicta were the strict division of the world of knowledge from that of emotion, and the lifting of life by throwing overboard the malign residuum of dogmatism, fanaticism, and intolerance, thereby stimulating and nourishing a plastic vigour of intellect. His cry was 'Commotion before stagnation, the leap of the torrent before the stillness of the swamp.'

His successors have no longer any need to repeat those significant words, 'We claim and we shall wrest from theology the entire domain of cosmological theory.' The claim has been practically, though often unconsciously, conceded. Tyndall's dictum, 'Every system must be plastic to the extent that the growth of knowledge demands,' struck a note that was too often absent from the heated discussions of days that now seem so strangely remote. His honourable admission that, after all that had been achieved by the developmental theory, 'the whole process of evolution is the manifestation of a power absolutely inscrutable to the intellect of man,' shows how willingly he acknowledged the necessary limits of scientific inquiry. This reservation did not prevent him from expressing the conviction forced upon him by the pressure of intellectual necessity, after exhaustive consideration of the known relations of living things, that matter in itself must be regarded as containing the promise and potency of all terrestrial life. Bacon in his day said very much the same thing: 'He that will know the properties and proceedings of matter should comprehend in his understanding the sum of all things, which have been, which are, and which shall be, although no knowledge can extend so far as to singular and individual beings.' Tyndall's conclusion was at the time thought to be based on a too insecure projection into the unknown, and some even regarded such an expansion of the crude properties of matter as totally unwarranted. Yet Tyndall was certainly no materialist in the ordinary acceptance of the term. It is true his arguments, like all arguments, were capable of being distorted, especially when taken out of their context, and the address became in this way an easy prey for hostile criticism. The glowing rhetoric that gave charm to his discourse and the poetic similes that clothed the dry bones of his close-woven logic were attacked by a veritable broadside of critical artillery. At the present day these would be considered as only appropriate artistic embellishments, so great is the unconscious change wrought in our surroundings. It must be remembered that, while Tyndall discussed the evolutionary problem from many points of view, he took up the position of a practical disciple of Nature dealing with the known experimental and observational realities of physical inquiry. Thus he accepted as fundamental concepts the atomic theory, together with the capacity of the atom to be the vehicle or repository of energy, and the grand generalisation of the conservation of energy. Without the former, Tyndall doubted whether it would be possible to frame a theory of the material universe; and as to the latter he recognised its radical significance in that the ultimate

philosophical issues therein involved were as yet but dimly seen. That such generalisations are provisionally accepted does not mean that science is not alive to the possibility that what may now be regarded as fundamental may in future be superseded or absorbed by a wider generalisation. It is only the poverty of language and the necessity for compendious expression that oblige the man of science to resort to metaphor and to speak of the Laws of Nature. In reality, he does not pretend to formulate any laws for Nature, since to do so would be to assume a knowledge of the inscrutable cause from which alone such laws could emanate. When he speaks of a 'law of Nature' he simply indicates a sequence of events which, so far as his experience goes, is invariable, and which therefore enables him to predict, to a certain extent, what will happen in given circumstances. But, however seemingly bold may be the speculation in which he permits himself to indulge, he does not claim for his best hypothesis more than provisional validity. He does not forget that to-morrow may bring a new experience compelling him to recast the hypothesis of to-day. This plasticity of scientific thought, depending upon reverent recognition of the vastness of the unknown, is oddly made a matter of reproach by the very people who harp upon the limitations of human knowledge. Yet the essential condition of progress is that we should generalise to the best of our ability from the experience at command, treat our theory as provisionally true, endeavour to the best of our power to reconcile with it all the new facts we discover, and abandon or modify it when it ceases to afford a coherent explanation of new experience. That procedure is far as are the poles asunder from the presumptuous attempt to travel beyond the study of secondary causes. Any discussion as to whether matter or energy was the true reality would have appeared to Tyndall as a futile metaphysical disputation, which, being completely dissociated from verified experience, would lead to nothing. No explanation was attempted by him of the origin of the bodies we call elements, nor how some of such bodies came to be compounded into complex groupings and built up into special structures with which, so far as we know, the phenomena characteristic of life are invariably associated. The evolutionary doctrine leads us to the conclusion that life, such as we know it, has only been possible during a short period of the world's history, and seems equally destined to disappear in the remote future; but it postulates the existence of a material universe endowed with an infinity of powers and properties, the origin of which it does not pretend to account for. The enigma at both ends of the scale Tyndall admitted, and the futility of attempting to answer such questions he fully recognised. Nevertheless, Tyndall did not mean that the man of science should be debarred from speculating as to the possible nature of the simplest forms of matter or the mode in which life may have originated on this planet. Lord Kelvin, in his Presidential Address, put the position admirably when he said 'Science is bound by the everlasting law of honour to face fearlessly every problem that can fairly be presented to it. If a probable solution consistent with the ordinary course

of Nature can be found, we must not invoke an abnormal act of Creative Power'; and in illustration he forthwith proceeded to express his conviction that from time immemorial many worlds of life besides our own have existed, and that 'it is not an unscientific hypothesis that life originated on this earth through the moss-grown fragments from the ruins of another world.' In spite of the great progress made in science, it is curious to notice the occasional recrudescence of metaphysical dogma. For instance, there is a school which does not hesitate to revive ancient mystifications in order to show that matter and energy can be shattered by philosophical arguments, and have no objective reality. Science is at once more humble and more reverent. She confesses her ignorance of the ultimate nature of matter, of the ultimate nature of energy, and still more of the origin and ultimate synthesis of the two. She is content with her patient investigation of secondary causes, and glad to know that since Tyndall spoke in Belfast she has made great additions to the knowledge of general molecular mechanism, and especially of synthetic artifice in the domain of organic chemistry, though the more exhaustive acquaintance gained only forces us the more to acquiesce in acknowledging the inscrutable mystery of matter. Our conception of the power and potency of matter has grown in little more than a quarter of a century to much more imposing dimensions, and the outlook for the future assuredly suggests the increasing acceleration of our rate of progress. For the impetus he gave to scientific work and thought, and for his fine series of researches chiefly directed to what Newton called the more secret and noble works of Nature within the corpuscles, the world owes Tyndall a debt of gratitude. It is well that his memory should be held in perennial respect, especially in the land of his birth.

The Endowment of Education.

These are days of munificent benefactions to science and education, which however are greater and more numerous in other countries than in our own. Splendid as they are, it may be doubted, if we take into account the change in the value of money, the enormous increase of population, and the utility of science to the builders of colossal fortunes, whether they bear comparison with the efforts of earlier days. But the habit of endowing science was so long in practical abeyance that every evidence of its resumption is matter for sincere congratulation. Mr. Cecil Rhodes has dedicated a very large sum of money to the advancement of education, though the means he has chosen are perhaps not the most effective. It must be remembered that his aims were political as much as educational. He had the noble and worthy ambition to promote enduring friendship between the great English-speaking communities of the world, and knowing the strength of college ties he conceived that this end might be greatly furthered by bringing together at an English university the men who would presumably have much to do in later life with the influencing of opinion, or even with the direction of policy. It has

been held by some a striking tribute to Oxford that a man but little given to academic pursuits or modes of thought should think it a matter of high importance to bring men from our colonies or even from Germany, to submit to the formative influences of that ancient seat of learning. But this is perhaps reading Mr. Rhodes backwards. He showed his affectionate recollection of his college days by his gift to Oriel. But, apart from the main idea of fostering good relations between those who will presumably be influential in England, in the colonies, and in the United States, Mr Rhodes was probably influenced also by the hope that the influx of strangers would help to broaden Oxford notions and to procure revision of conventional arrangements.

Dr. Andrew Carnegie's endowment of Scottish universities, as modified by him in deference to expert advice, is a more direct benefit to the higher education. For while Mr. Rhodes has only enabled young men to get what Oxford has to give, Dr. Carnegie has also enabled his trustees powerfully to augment and improve the teaching equipment of the universities themselves. At the same time he has provided as far as possible for the enduring usefulness of his money. His trustees form a permanent body external to the universities, which, while possessing no power of direct control, must always, as holder of the purse-strings, be in a position to offer independent and weighty criticisms. More recently Dr. Carnegie has devoted an equal sum of ten million dollars to the foundation of a Carnegie Institution in Washington. Here again he has been guided by the same ideas. He has neither founded a university nor handed over the money to any existing university. He has created a permanent trust charged with the duty of watching educational efforts and helping them from the outside according to the best judgment that can be formed in the circumstances of the moment. Its aims are to be—to promote original research ; to discover the exceptional man in every department of study, whether inside or outside of the schools, and to enable him to make his special study his life-work ; to increase facilities for higher education ; to aid and stimulate the universities and other educational institutions ; to assist students who may prefer to study at Washington ; and to ensure prompt publication of scientific discoveries. The general purpose of the founder is to secure, if possible, for the United States leadership in the domain of discovery and the utilisation of new forces for the benefit of man. Nothing will more powerfully further this end than attention to the injunction to lay hold of the exceptional man whenever and wherever he may be found, and, having got him, to enable him to carry on the work for which he seems specially designed. That means, I imagine, a scouring of the old world, as well as of the new, for the best men in every department of study—in fact, an assiduous collecting of brains similar to the collecting of rare books and works of art which Americans are now carrying on in so lavish a manner. As in diplomacy and war, so in science, we owe our reputation, and no small part of our prosperity, to exceptional men ; and that we do not enjoy these things in fuller measure we owe to our

lack of an army of well-trained ordinary men capable of utilising their ideas. Our exceptional men have too often worked in obscurity, without recognition from a public too imperfectly instructed to guess at their greatness, without assistance from a State governed largely by dialecticians, and without help from academic authorities hidebound by the pedantries of medieval scholasticism. For such men we have to wait upon the will of Heaven. Even Dr. Carnegie will not always find them when they are wanted. But what can be done in that direction will be done by institutions like Dr. Carnegie's, and for the benefit of the nation that possesses them in greatest abundance and uses them most intelligently. When contemplating these splendid endowments of learning, it occurred to me that it would be interesting to find out exactly what some definite quantity of scientific achievement has cost in hard cash. In an article by Carl Snyder in the January number of the 'North American Review,' entitled 'America's Inferior Place in the Scientific World,' I found the statement that 'it would be hardly too much to say that during the hundred years of its existence the Royal Institution alone has done more for English science than all of the English universities put together. This is certainly true with regard to British industry, for it was here that the discoveries of Faraday were made.' I was emboldened by this estimate from a distant and impartial observer to do, what otherwise I might have shrunk from doing, and to take the Royal Institution—after all, the foundation of an American citizen, Count Rumford—as the basis of my inquiry. The work done at the Royal Institution during the past hundred years is a fairly definite quantity in the mind of every man really conversant with scientific affairs. I have obtained from the books accurate statistics of the total expenditure on experimental inquiry and public demonstrations for the whole of the nineteenth century. The items are :

Professors' Salaries—Physics and Chemistry .	£54,600
Laboratory Expenditure	24,430
Assistants' Salaries	21,590
Total for one hundred years	£100,620

In addition, the members and friends of the Institution have contributed to a fund for exceptional expenditure for Experimental Research the sum of 9,580*l*. It should also be mentioned that a Civil List pension of 300*l*. was granted to Faraday in 1853, and was continued during twenty-seven years of active work and five years of retirement. Thirty-two years in all, at 300*l*. a year, make a sum of 9,600*l*., representing the national donation, which, added to the amount of expenditure just stated, brings up the total cost of a century of scientific work in the laboratories of the Royal Institution, together with public demonstrations, to 119,800*l*., or an average of 1,200*l*. per annum. I think if you recall the names and achievements of Young, Davy, Faraday, and Tyndall, you will come to the

conclusion that the exceptional man is about the cheapest of natural products. It is a popular fallacy that the Royal Institution is handsomely endowed. On the contrary, it has often been in financial straits; and since its foundation by Count Rumford its only considerable bequests have been one from Thomas G. Hodgkins, an American citizen, for Experimental Research, and that of John Fuller for endowing with 95*l.* a year the chairs of Chemistry and Physiology. In this connection the Davy-Faraday Laboratory, founded by the liberality of Dr. Ludwig Mond, will naturally occur to many minds. But though affiliated to the Royal Institution, with, I hope, reciprocal indirect advantages, that Laboratory is financially independent and its endowments are devoted to its own special purpose, which is to provide opportunity to prosecute independent research for worthy and approved applicants of all nationalities. The main reliance of the Royal Institution has always been, and still remains, upon the contributions of its members, and upon corresponding sacrifices in the form of time and labour by its professors. It may be doubted whether we can reasonably count upon a succession of scientific men able and willing to make sacrifices which the conditions of modern life tend to render increasingly burdensome. Modern science is in fact in something of a dilemma. Devotion to abstract research upon small means is becoming always harder to maintain, while at the same time the number of wealthy independent searchers after truth and patrons of science of the style of Joule, Spottiswoode, and De la Rue is apparently becoming smaller. The installations required by the refinements of modern science are continually becoming more costly, so that upon all grounds it would appear that without endowments of the kind provided by Dr. Carnegie the outlook for disinterested research is rather dark. On the other hand, these endowments, unless carefully administered, might obviously tend to impair the single-minded devotion to the search after truth for its own sake, to which science has owed almost every memorable advance made in the past. The Carnegie Institute will dispose in a year of as much money as the members of the Royal Institution have expended in a century upon its purely scientific work. It will at least be interesting to note how far the output of high-class scientific work corresponds to the hundredfold application of money to its production. Nor will it be of less interest to the people of this country to observe the results obtained from that moiety of Dr. Carnegie's gift to Scotland which is to be applied to the promotion of scientific research.

Applied Chemistry, English and Foreign.

The Diplomatic and Consular reports published from time to time by the Foreign Office are usually too belated to be of much use to business men, but they sometimes contain information concerning what is done in foreign countries which affords food for reflection. One of these reports, issued a year ago, gives a very good account of the German arrangements

and provisions for scientific training, and of the enormous commercial demand for the services of men who have passed successfully through the universities and Technical High Schools, as well as of the wealth that has accrued to Germany through the systematic application of scientific proficiency to the ordinary business of life.

Taking these points in their order, I have thought it a matter of great interest to obtain a comparative view of chemical equipment in this country and in Germany, and I am indebted to Professor Henderson of Glasgow, who last year became the secretary of a committee of this Association of which Professor Armstrong is chairman, for statistics referring to this country, which enable a comparison to be broadly made. The author of the consular report estimates that in 1901 there were 4,500 trained chemists employed in German works, the number having risen to this point from 1,700 employed twenty-five years earlier. It is difficult to give perfectly accurate figures for this country, but a liberal estimate places the number of works chemists at 1,500, while at the very outside it cannot be put higher than somewhere between 1,500 and 2,000. In other words, we cannot show in the United Kingdom, notwithstanding the immense range of the chemical industries in which we once stood prominent, more than one-third of the professional staff employed in Germany. It may perhaps be thought or hoped that we make up in quality for our defect in quantity, but unfortunately this is not the case. On the contrary, the German chemists are, on the average, as superior in technical training and acquirements as they are numerically. Details are given in the report of the training of 633 chemists employed in German works. Of these, 69 per cent. hold the degree of Ph.D., about 10 per cent. hold the diploma of a Technical High School, and about 5 per cent. hold both qualifications. That is to say 84 per cent. have received a thoroughly systematic and complete chemical training, and 74 per cent. of these add the advantages of a university career. Compare with this the information furnished by 500 chemists in British works. Of these only 21 per cent. are graduates, while about 10 per cent. hold the diploma of a college. Putting the case as high as we can, and ignoring the more practical and thorough training of the German universities, which give their degrees for work done, and not for questions asked and answered on paper, we have only 31 per cent. of systematically trained chemists against 84 per cent. in German works. It ought to be mentioned that about 21 per cent. of the 500 are Fellows or Associates of the Institute of Chemistry, whatever that may amount to in practice, but of these a very large number have already been accounted for under the heads of graduates and holders of diplomas. These figures, which I suspect are much too favourable on the British side, unmistakably point to the prevalence among employers in this country of the antiquated adherence to rule of thumb, which is at the root of much of the backwardness we have to deplore. It hardly needs to be pointed out to such an audience as the present that chemists who are neither graduates of a university, nor holders of a diploma from a

technical college, may be competent to carry on existing processes according to traditional methods, but are very unlikely to effect substantial improvements, or to invent new and more efficient processes. I am very far from denying that here and there an individual may be found whose exceptional ability enables him to triumph over all defects of training. But in all educational matters it is the average man whom we have to consider, and the average ability which we have to develop. Now, to take the second point—the actual money value of the industries carried on in Germany by an army of workers both quantitatively and qualitatively so superior to our own. The Consular report estimates the whole value of German chemical industries at not less than fifty millions sterling per annum. These industries have sprung up within the last seventy years, and have received enormous expansion during the last thirty. They are, moreover, very largely founded upon basic discoveries made by English chemists, but never properly appreciated or scientifically developed in the land of their birth. I will place before you some figures showing the growth of a single firm engaged in a single one of these industries—the utilisation of coal tar for the production of drugs, perfumes, and colouring-matters of every conceivable shade. The firm of Friedrich Bayer & Co. employed in 1875, 119 workmen. The number has more than doubled itself every five years, and in May of this year that firm employed 5,000 workmen, 160 chemists, 260 engineers and mechanics, and 680 clerks. For many years past it has regularly paid 18 per cent. on the ordinary shares, which this year has risen to 20 per cent. ; and in addition, in common with other and even larger concerns in the same industry, has paid out of profits for immense extensions usually charged to capital account. There is one of these factories, the works and plant of which stand in the books at 1,500,000*l.*, while the money actually sunk in them approaches to 5,000,000*l.* In other words, the practical monopoly enjoyed by the German manufacturers enables them to exact huge profits from the rest of the world, and to establish a position which, financially as well as scientifically, is almost unassailable. I must repeat that the fundamental discoveries upon which this gigantic industry is built were made in this country, and were practically developed to a certain extent by their authors. But in spite of the abundance and cheapness of the raw material, and in spite of the evidence that it could be most remuneratively worked up, these men founded no school and had practically no successors. The colours they made were driven out of the field by newer and better colours made from their stuff by the development of their ideas, but these improved colours were made in Germany and not in England. Now what is the explanation of this extraordinary and disastrous phenomenon ? I give it in a word—want of education. We had the material in abundance when other nations had comparatively little. We had the capital, and we had the brains, for we originated the whole thing. But we did not possess the diffused education without which the ideas of men of genius cannot fructify beyond

the limited scope of an individual. I am aware that our patent laws are sometimes held responsible. Well, they are a contributory cause ; but it must be remembered that other nations with patent laws as protective as could be desired have not developed the colour industry. The patent laws have only contributed in a secondary degree, and if the patent laws have been bad the reason for their badness is again want of education. Make them as bad as you choose, and you only prove that the men who made them, and the public whom these men try to please, were misled by theories instead of being conversant with fact and logic. But the root of the mischief is not in the patent laws or in any legislation whatever. It is in the want of education among our so-called educated classes, and secondarily among the workmen on whom these depend. It is in the abundance of men of ordinary plodding ability, thoroughly trained and methodically directed, that Germany at present has so commanding an advantage. It is the failure of our schools to turn out, and of our manufacturers to demand, men of this kind, which explains our loss of some valuable industries and our precarious hold upon others. Let no one imagine for a moment that this deficiency can be remedied by any amount of that technical training which is now the fashionable nostrum. It is an excellent thing, no doubt, but it must rest upon a foundation of general training. Mental habits are formed for good or evil long before men go to the technical schools. We have to begin at the beginning : we have to train the population from the first to think correctly and logically, to deal at first hand with facts, and to evolve, each one for himself, the solution of a problem put before him, instead of learning by rote the solution given by somebody else. There are plenty of chemists turned out, even by our Universities, who would be of no use to Bayer & Co. They are chockfull of formulæ, they can recite theories, and they know textbooks by heart ; but put them to solve a new problem, freshly arisen in the laboratory, and you will find that their learning is all dead. It has not become a vital part of their mental equipment, and they are floored by the first emergence of the unexpected. The men who escape this mental barrenness are men who were somehow or other taught to think long before they went to the university. To my mind, the really appalling thing is not that the Germans have seized this or the other industry, or even that they may have seized upon a dozen industries. It is that the German population has reached a point of general training and specialised equipment which it will take us two generations of hard and intelligently directed educational work to attain. It is that Germany possesses a national weapon of precision which must give her an enormous initial advantage in any and every contest depending upon disciplined and methodised intellect.

History of Cold and the Absolute Zero.

It was Tyndall's good fortune to appear before you at a moment when a fruitful and comprehensive idea was vivifying the whole domain of scientific thought. At the present time no such broad generalisation presents itself for discussion, while on the other hand the number of specialised studies has enormously increased. Science is advancing in so broad a front by the efforts of so great an army of workers that it would be idle to attempt within the limits of an address to the most indulgent of audiences anything like a survey of chemistry alone. But I have thought it might be instructive, and perhaps not uninteresting, to trace briefly in broad outline the development of that branch of study with which my own labours have been recently more intimately connected—a study which I trust I am not too partial in thinking is as full of philosophical interest as of experimental difficulty. The nature of heat and cold must have engaged thinking men from the very earliest dawn of speculation upon the external world; but it will suffice for the present purpose if, disregarding ancient philosophers and even medieval alchemists, we take up the subject where it stood after the great revival of learning, and as it was regarded by the father of the inductive method. That this was an especially attractive subject to Bacon is evident from the frequency with which he recurs to it in his different works, always with lamentation over the inadequacy of the means at disposal for obtaining a considerable degree of cold. Thus in the chapter in the *Natural History*, '*Sylva Sylvarum*,' entitled '*Experiments in consort touching the production of cold*,' he says, '*The production of cold is a thing very worthy of the inquisition both for the use and the disclosure of causes. For heat and cold are nature's two hands whereby she chiefly worketh, and heat we have in readiness in respect of the fire, but for cold we must stay till it cometh or seek it in deep caves or high mountains, and when all is done we cannot obtain it in any great degree, for furnaces of fire are far hotter than a summer sun, but vaults and hills are not much colder than a winter's frost.*' The great Robert Boyle was the first experimentalist who followed up Bacon's suggestions. In 1682 Boyle read a paper to the Royal Society on '*New Experiments and Observations touching Cold, or an Experimental History of Cold*,' published two years later in a separate work. This is really a most complete history of everything known about cold up to that date, but its great merit is the inclusion of numerous experiments made by Boyle himself on frigorific mixtures, and the general effects of such upon matter. The agency chiefly used by Boyle in the conduct of his experiments was the glaciating mixture of snow or ice and salt. In the course of his experiments he made many important observations. Thus he observed that the salts which did not help the snow or ice to dissolve faster gave no effective freezing. He showed that water in becoming ice expands by about one-ninth of its volume, and bursts gun-barrels. He attempted to counteract the expansion and prevent freezing by completely

filling a strong iron ball with water before cooling ; anticipating that it might burst the bottle by the stupendous force of expansion, or that if it did not, then the ice produced might under the circumstances be heavier than water. He speculated in an ingenious way on the change of water into ice. Thus he says, 'If cold be but a privation of heat through the recess of that ethereal substance which agitated the little eel-like particles of the water and thereby made them compose a fluid body, it may easily be conceived that they should remain rigid in the postures in which the ethereal substance quitted them, and thereby compose an unfluid body like ice ; yet how these little eels should by that recess acquire as strong an endeavour outwards as if they were so many little springs and expand themselves with so stupendous a force, is that which does not so readily appear.' The greatest degree of adventitious cold Boyle was able to produce did not make air exposed to its action lose a full tenth of its own volume, so that, in his own words, the cold does not 'weaken the spring by anything near so considerable as one would expect.' After making this remarkable observation and commenting upon its unexpected nature, it is strange Boyle did not follow it up. He questions the existence of a body of its own nature supremely cold, by participating in which all other bodies obtain that quality, although the doctrine of a *primum frigidum* had been accepted by many sects of philosophers ; for, as he says, 'if a body being cold signify no more than its not having its sensible parts so much agitated as those of our sensorium, it suffices that the sun or the fire or some other agent, whatever it were, that agitated more vehemently its parts before, does either now cease to agitate them or agitates them but very remissly, so that till it be determined whether cold be a positive quality or but a privative it will be needless to contend what particular body ought to be esteemed the *primum frigidum*.' The whole elaborate investigation cost Boyle immense labour, and he confesses that he 'never handled any part of natural philosophy that was so troublesome and full of hardships.' He looked upon his results but as a 'beginning' in this field of inquiry, and for all the trouble and patience expended he consoled himself with the thought of 'men being oftentimes obliged to suffer as much wet and cold and dive as deep to fetch up sponges as to fetch up pearls.' After the masterly essay of Boyle, the attention of investigators was chiefly directed to improving thermometrical instruments. The old air thermometer of Galileo being inconvenient to use, the introduction of fluid thermometers greatly aided the inquiry into the action of heat and cold. For a time great difficulty was encountered in selecting proper fixed points on the scales of such instruments, and this stimulated men like Huygens, Newton, Hooke, and Amontons to suggest remedies and to conduct experiments. By the beginning of the eighteenth century the freezing-point and the boiling-point of water were agreed upon as fixed points, and the only apparent difficulties to be overcome were the selection of the fluid, accurate calibration of the capillary tube of the thermometer, and a general understanding as to scale divisions. It must be confessed

that great confusion and inaccuracy in temperature observations arose from the variety and crudeness of the instruments. This led Amontons in 1702-3 to contribute two papers to the French Academy which reveal great originality in the handling of the subject, and which, strange to say, are not generally known. The first discourse deals with some new properties of the air and the means of accurately ascertaining the temperature in any climate. He regarded heat as due to a movement of the particles of bodies, though he did not in any way specify the nature of the motion involved ; and as the general cause of all terrestrial motion, so that in its absence the earth would be without movement in its smallest parts. The new facts he records are observations on the spring or pressure of air brought about by the action of heat. He shows that different masses of air measured at the same initial spring or pressure, when heated to the boiling-point of water, acquire equal increments of spring or pressure, provided the volume of the gas be kept at its initial value. Further, he proves that if the pressure of the gas before heating be doubled or tripled, then the additional spring or pressure resulting from heating to the boiling-point of water is equally doubled or tripled. In other words, the ratio of the total spring of air at two definite and steady temperatures and at constant volume is a constant, independent of the mass or the initial pressure of the air in the thermometer. These results led to the increased perfection of the air thermometer as a standard instrument, Amontons' idea being to express the temperature at any locality in fractions of the degree of heat of boiling water. The great novelty of the instrument is that temperature is defined by the measurement of the length of a column of mercury. In passing, he remarks that we do not know the extreme of heat and cold, but that he has given the results of experiments which establish correspondences for those who wish to consider the subject. In the following year Amontons contributed to the Academy a further paper extending the scope of the inquiry. He there pointed out more explicitly that as the degrees of heat in his thermometer are registered by the height of a column of mercury, which the heat is able to sustain by the spring of the air, it follows that the extreme cold of the thermometer will be that which reduces the air to have no power of spring. This, he says, will be a much greater cold than what we call 'very cold,' because experiments have shown that if the spring of the air at boiling-point is 73 inches, the degree of heat which remains in the air when brought to the freezing-point of water is still very great, for it can still maintain the spring of $51\frac{1}{2}$ inches. The greatest climatic cold on the scale of units adopted by Amontons is marked 50, and the greatest summer heat 58, the value for boiling water being 73, and the zero being 52 units below the freezing-point. Thus Amontons was the first to recognise that the use of air as a thermometric substance led to the inference of the existence of a zero of temperature, and his scale is nothing else than the absolute one we are now so familiar with. It results from Amontons' experiments that the air would have no

spring left if it were cooled below the freezing-point of water to about $2\frac{1}{2}$ times the temperature range which separates the boiling-point and the freezing-point. In other words, if we adopt the usual centennial difference between these two points of temperature as 100 degrees, then the zero of Amontons' air thermometer is *minus* 240 degrees. This is a remarkable approximation to our modern value for the same point of *minus* 273 degrees. It has to be confessed that Amontons' valuable contributions to knowledge met with that fate which has so often for a time overtaken the work of too-advanced discoverers; in other words, it was simply ignored, or in any case not appreciated by the scientific world either of that time or half a century later. It is not till Lambert, in his work on 'Pyrometrie' published in 1779, repeated Amontons' experiments and endorsed his results that we find any further reference to the absolute scale or the zero of temperature. Lambert's observations were made with the greatest care and refinement, and resulted in correcting the value of the zero of the air scale to *minus* 270 degrees as compared with Amontons' *minus* 240 degrees. Lambert points out that the degree of temperature which is equal to zero is what one may call absolute cold, and that at this temperature the volume of the air would be practically nothing. In other words, the particles of the air would fall together and touch each other and become dense like water; and from this it may be inferred that the gaseous condition is caused by heat. Lambert says that Amontons' discoveries had found few adherents because they were too beautiful and advanced for the time in which he lived.

About this time a remarkable observation was made by Professor Braun at Moscow, who, during the severe winter of 1759, succeeded in freezing mercury by the use of a mixture of snow and nitric acid. When we remember that mercury was regarded as quite a peculiar substance possessed of the essential quality of fluidity, we can easily understand the universal interest created by the experiment of Braun. This was accentuated by the observations he made on the temperature given by the mercury thermometer, which appeared to record a temperature as low as *minus* 200° C. The experiments were soon repeated by Hutchins at Hudson's Bay, who conducted his work with the aid of suggestions given him by Cavendish and Black. The result of the new observations was to show that the freezing-point of mercury is only *minus* 40° C., the errors in former experiments having been due to the great contraction of the mercury in the thermometer in passing into the solid state. From this it followed that the enormous natural and artificial colds which had generally been believed in had no proved existence. Still the possible existence of a zero of temperature very different from that deduced from gas thermometry had the support of such distinguished names as those of Laplace and Lavoisier. In their great memoir on 'Heat,' after making what they consider reasonable hypotheses as to the relation between specific heat and total heat, they calculate values for the zero which range

from $1,500^{\circ}$ to $3,000^{\circ}$ below melting ice. On the whole, they regard the absolute zero as being in any case 600° below the freezing-point. Lavoisier, in his 'Elements of Chemistry' published in 1792, goes further in the direction of indefinitely lowering the zero of temperature when he says, 'We are still very far from being able to produce the degree of absolute cold, or total deprivation of heat, being unacquainted with any degree of coldness which we cannot suppose capable of still further augmentation; hence it follows we are incapable of causing the ultimate particles of bodies to approach each other as near as possible, and thus these particles do not touch each other in any state hitherto known.' Even as late as the beginning of the nineteenth century we find Dalton, in his new system of 'Chemical Philosophy,' giving ten calculations of this value, and adopting finally as the natural zero of temperature *minus* $3,000^{\circ}$ C.

In Black's lectures we find that he takes a very cautious view with regard to the zero of temperature, but as usual is admirably clear with regard to its exposition. Thus he says, 'We are ignorant of the lowest possible degree or beginning of heat. Some ingenious attempts have been made to estimate what it may be, but they have not proved satisfactory. Our knowledge of the degrees of heat may be compared to what we should have of a chain, the two ends of which were hidden from us and the middle only exposed to our view. We might put distinct marks on some of the links, and number the rest according as they are nearest to or further removed from the principal links; but not knowing the distance of any links from the end of the chain we could not compare them together with respect to their distance, or say that one link was twice as far from the end of the chain as another.' It is interesting to observe, however, that Black was evidently well acquainted with the work of Amontons, and strongly supports his inference as to the nature of air. Thus, in discussing the general cause of vaporisation, Black says that some philosophers have adopted the view 'that every palpable elastic fluid in nature is produced and preserved in this form by the action of heat. Mr. Amontons, an ingenious member of the late Royal Academy of Sciences, at Paris, was the first who proposed this idea with respect to the atmosphere. He supposed that it might be deprived of the whole of its elasticity and condensed and even frozen into a solid matter were it in our power to apply to it a sufficient cold; that it is a substance that differs from others by being incomparably more volatile, and which is therefore converted into vapour and preserved in that form by a weaker heat than any that ever happened or can obtain in this globe, and which therefore cannot appear under any other form than the one it now wears, so long as the constitution of the world remains the same as at present.' The views that Black attributes to Amontons have been generally associated with the name of Lavoisier, who practically admitted similar possibilities as to the nature of air; but it is not likely that in such matters Black would commit any mistake as to the real author of a particular idea, especially in his own department of knowledge. Black's own

special contribution to low-temperature studies was his explanation of the interaction of mixtures of ice with salts and acids by applying the doctrine of the latent heat of fluidity of ice to account for the frigorific effect. In a similar way Black explained the origin of the cold produced in Cullen's remarkable experiment of the evaporation of ether under the receiver of an air-pump by pointing out that the latent heat of vaporisation in this case necessitated such a result. Thus, by applying his own discoveries of latent heat, Black gave an intelligent explanation of the cause of all the low-temperature phenomena known in his day.

After the gaseous laws had been definitely formulated by Gay-Lussac and Dalton, the question of the absolute zero of temperature, as deduced from the properties of gases, was revived by Clement and Desormes. These distinguished investigators presented a paper on the subject to the French Academy in 1812, which, it appears, was rejected by that body. The authors subsequently elected to publish it in 1819. Relying on what we know now to have been a faulty hypothesis, they deduced from observations on the heating of air rushing into a vacuum the temperature of *minus* 267 degrees as that of the absolute zero. They further endeavoured to show, by extending to lower temperatures the volume or the pressure coefficients of gases given by Gay-Lussac, that at the same temperature of *minus* 267 degrees the gases would contract so as to possess no appreciable volume, or, alternatively, if the pressure was under consideration, it would become so small as to be non-existent. Although full reference is given to previous work bearing on the same subject, yet, curiously enough, no mention is made of the name of Amontons. It certainly gave remarkable support to Amontons' notion of the zero to find that simple gases like hydrogen and compound gases like ammonia, hydrochloric, carbonic, and sulphurous acids should all point to substantially the same value for this temperature. But the most curious fact about this research of Clement and Desormes is that Gay-Lussac was a bitter opponent of the validity of the inferences they drew either from his work or their own. The mode in which Gay-Lussac regarded the subject may be succinctly put as follows: A quick compression of air to one-fifth volume raises its temperature to 300 degrees, and if this could be made much greater and instantaneous the temperature might rise to 1,000 or 2,000 degrees. Conversely, if air under five atmospheres were suddenly dilated, it would absorb as much heat as it had evolved during compression, and its temperature would be lowered by 300 degrees. Therefore, if air were taken and compressed to fifty atmospheres or more, the cold produced by its sudden expansion would have no limit. In order to meet this position Clement and Desormes adopted the following reasoning: They pointed out that it had not been proved that Gay-Lussac was correct in his hypothesis, but that in any case it tacitly involves the assumption that a limited quantity of matter possesses an unlimited supply of heat. If this were the case, then heat would be unlike any other measurable thing or quality. It is, therefore, more

consistent with the course of nature to suppose that the amount of heat in a body is like the quantity of elastic fluid filling a vessel, which, while definite in original amount, one may make less and less by getting nearer to a complete exhaustion. Further, to realise the absolute zero in the one case is just as impossible as to realise the absolute vacuum in the other ; and as we do not doubt a zero of pressure, although it is unattainable, for the same reason we ought to accept the reality of the absolute zero. We know now that Gay-Lussac was wrong in supposing the increment of temperature arising from a given gaseous compression would produce a corresponding decrement from an identical expansion. After this time the zero of temperature was generally recognised as a fixed ideal point, but in order to show that it was hypothetical a distinction was drawn between the use of the expressions, zero of absolute temperature and the absolute zero.

The whole question took an entirely new form when Lord Kelvin, in 1848, after the mechanical equivalent of heat had been determined by Joule, drew attention to the great principles underlying Carnot's work on the 'Motive Power of Heat,' and applied them to an absolute method of temperature measurement, which is completely independent of the properties of any particular substance. The principle was that for a difference of one degree on this scale, between the temperatures of the source and refrigerator, a perfect engine should give the same amount of work in every part of the scale. Taking the same fixed points as for the Centigrade scale, and making 100 of the new degrees cover that range, it was found that the degrees not only within that range, but as far beyond as experimental data supplied the means of comparison, differed by only minute quantities from those of Regnault's air thermometer. The zero of the new scale had to be determined by the consideration that when the refrigerator was at the zero of temperature the perfect engine should give an amount of work equal to the full mechanical equivalent of the heat taken up. This led to a zero of 273 degrees below the temperature of freezing water, substantially the same as that deduced from a study of the gaseous state. It was a great advance to demonstrate by the application of the laws of thermodynamics not only that the zero of temperature is a reality, but that it must be located at 273 degrees below the freezing-point of water. As no one has attempted to impugn the solid foundation of theory and experiment on which Lord Kelvin based his thermodynamic scale, the existence of a definite zero of temperature must be acknowledged as a fundamental scientific fact.

Liquefaction of Gases and Continuity of State.

In these speculations, however, chemists were dealing theoretically with temperatures to which they could not make any but the most distant experimental approach. Cullen, the teacher of Black, had indeed shown how to lower temperature by the evaporation of volatile bodies,

such as ether, by the aid of the air-pump, and the later experiments of Leslie and Wollaston extended the same principle. Davy and Faraday made the most of the means at command in liquefying the more condensable gases, while at the same time Davy pointed out that they in turn might be utilised to procure greater cold by their rapid reconversion into the aeriform state. Still the chemist was sorely hampered by the want of some powerful and accessible agent for the production of temperatures much lower than had ever been attained. That want was supplied by Thilorier, who in 1835 produced liquid carbonic acid in large quantities, and further made the fortunate discovery that the liquid could be frozen into a snow by its own evaporation. Faraday was prompt to take advantage of this new and potent agent. Under exhaustion he lowered its boiling-point from *minus* 78° C. to *minus* 110° C., and by combining this low temperature with pressure all the gases were liquefied by the year 1844, with the exception of the three elementary gases—hydrogen, nitrogen, and oxygen, and three compound gases—carbonic oxide, marsh gas, and nitric oxide; Andrews some twenty-five years after the work of Faraday attempted to induce change of state in the uncondensed gases by using much higher pressures than Faraday employed. Combining the temperature of a solid carbonic acid bath with pressures of 300 atmospheres, Andrews found that none of these gases exhibited any appearance of liquefaction in such high states of condensation; but so far as change of volume by high compression went, Andrews confirmed the earlier work of Natterer by showing that the gases become proportionately less compressible with growing pressure. While such investigations were proceeding, Regnault and Magnus had completed their refined investigations on the laws of Boyle and Gay-Lussac. A very important series of experiments was made by Joule and Kelvin ‘On the Thermal Effects of Fluids in Motion’ about 1862, in which the thermometrical effects of passing gases under compression through porous plugs furnished important data for the study of the mutual action of the gas molecules. No one, however, had attempted to make a complete study of a liquefiable gas throughout wide ranges of temperature. This was accomplished by Andrews in 1869, and his Bakerian Lecture ‘On the Continuity of the Gaseous and Liquid States of Matter’ will always be regarded as an epoch-making investigation. During the course of this research Andrews observed that liquid carbonic acid raised to a temperature of 31° C. lost the sharp concave surface of demarcation between the liquid and the gas, the space being now occupied by a homogeneous fluid which exhibited, when the pressure was suddenly diminished or the temperature slightly lowered, a peculiar appearance of moving or flickering striæ, due to great local alterations of density. At temperatures above 31° C. the separation into two distinct kinds of matter could not be effected even when the pressure reached 400 atmospheres. This limiting temperature of the change of state from gas to liquid Andrews called the critical temperature. He showed that this temperature is constant, and differs with each

substance, and that it is always associated with a definite pressure peculiar to each body. Thus the two constants, critical temperature and pressure, which have been of the greatest importance in subsequent investigations, came to be defined, and a complete experimental proof was given that 'the gaseous and liquid states are only distinct stages of the same condition of matter and are capable of passing into one another by a process of continuous change.'

In 1873 an essay 'On the Continuity of the Gaseous and Liquid State,' full of new and suggestive ideas, was published by van der Waals, who, recognising the value of Clausius' new conception of the Virial in Dynamics, for a long-continued series of motions, either oscillatory or changing exceedingly slowly with time, applied it to the consideration of the molecular movements of the particles of the gaseous substance, and after much refined investigation, and the fullest experimental calculation available at the time, devised his well-known Equation of Continuity. Its paramount merit is that it is based entirely on a mechanical foundation, and is in no sense empiric; we may therefore look upon it as having a secure foundation in fact, but as being capable of extension and improvement. James Thomson, realising that the straight-line breach of continuous curvature in the Andrews isothermals was untenable to the physical mind, propounded his emendation of the Andrews curves—namely, that they were continuous and of S form. We also owe to James Thomson the conception and execution of a three-dimensional model of Andrews' results, which has been of the greatest service in exhibiting the three variables by means of a specific surface afterwards greatly extended and developed by Professor Willard Gibbs. The suggestive work of James Thomson undoubtedly was a valuable aid to van der Waals, for as soon as he reached the point where his equation had to show the continuity of the two states this was the first difficulty he had to encounter, and he succeeded in giving the explanation. He also gave a satisfactory reason for the existence of a minimum value of the product of volume and pressure in the Regnault isothermals. His isothermals, with James Thomson's completion of them, were now shown to be the results of the laws of dynamics. Andrews applied the new equation to the consideration of the coefficients of expansion with temperature and of pressure with temperature, showing that although they were nearly equal, nevertheless they were almost independent quantities. His investigation of the capillarity constant was masterly, and he added further to our knowledge of the magnitudes of the molecules of gases and of their mean free paths. Following up the experiments of Joule and Kelvin, he showed how their cooling coefficients could be deduced, and proved that they vanished at a temperature in each case which is a constant multiple of the specific critical temperature. The equation of continuity developed by van der Waals involved the use of three constants instead of one, as in the old law of Boyle and Charles, the latter being only utilised to express the relation of temperature, pressure, and volume, when the gas is far removed from its point of liquefaction. Of the two

new constants one represents the molecular pressure arising from the attraction between the molecules, the other four times the volume of the molecules. Given these constants of a gas, van der Waals showed that his equation not only fitted into the general characters of the isothermals, but also gave the values of the critical temperature, the critical pressure, and the critical volume. In the case of carbonic acid the theoretical results were found to be in remarkable agreement with the experimental values of Andrews. This gave chemists the means of ascertaining the critical constants, provided sufficiently accurate data derived from the study of a few properly distributed isothermals of the gaseous substance were available. Such important data came into the possession of chemists when Amagat published his valuable paper on 'The Isothermals of Hydrogen, Nitrogen, Oxygen, Ethylene, &c.,' in the year 1880. It now became possible to calculate the critical data with comparative accuracy for the so-called permanent gases oxygen and nitrogen, and this was done by Sarrau in 1882. In the meantime a great impulse had been given to a further attack upon the so-called permanent gases by the suggestive experiments made by Pictet and Cailletet. The static liquefaction of oxygen was effected by Wroblewski in 1883, and thereby the theoretical conclusions derived from van der Waals' equation were substantially confirmed. The liquefaction of oxygen and air was achieved through the use of liquid ethylene as a cooling agent, which enabled a temperature of *minus* 140 degrees to be maintained by its steady evaporation *in vacuo*. From this time liquid oxygen and air came to be regarded as the potential cooling agents for future research, commanding as they did a temperature of 200 degrees below melting ice. The theoretical side of the question received at the hands of van der Waals a second contribution, which was even more important than his original essay, and that was his novel and ingenious development of what he calls 'The Theory of Corresponding States.' He defined the corresponding states of two substances as those in which the ratios of the temperature, pressure, and volume to the critical temperature, pressure, and volume respectively were the same for the two substances, and in corresponding states he showed that the three pairs of ratios all coincided. From this a series of remarkable propositions were developed, some new, some proving previous laws that were hitherto only empiric, and some completing and correcting faulty though approximate laws. As examples, he succeeded in calculating the boiling-point of carbonic acid from observations on ether vapour, proved Kopp's law of molecular volumes, and showed that at corresponding temperatures the molecular latent heats of vaporisation are proportional to the absolute critical temperature, and that under the same conditions the coefficients of liquid expansion are inversely proportional to the absolute critical temperature, and that the coefficients of liquid compressibility are inversely proportional to the critical pressure. All these propositions and deductions are in the main correct, though further experimental investigation has shown minor discrepancies requiring

explanation. Various proposals have been made to supplement van der Waals' equation so as to bring it into line with experiments, some being entirely empiric, others theoretical. Clausius, Sarrau, Wroblewski, Batteli, and others attacked the question empirically, and in the main preserved the co-volume (depending on the total volume of the molecules) unaltered while trying to modify the constant of molecular attraction. Their success depended entirely on the fact that, instead of limiting the number of constants to three, some of them have increased them to as many as ten. On the other hand, a series of very remarkable theoretical investigations has been made by van der Waals himself, by Kammerlingh Onnes, Korteweg, Jaeger, Boltzmann, Dieterici, and Rienganum, and others, all directed in the main towards an admitted variation in the value of the co-volume while preserving the molecular attraction constant. The theoretical deductions of Tait lead to the conclusion that a substance below its critical point ought to have two different equations of the van der Waals type, one referring to the liquid and the other to the gaseous phase. One important fact was soon elicited—namely, that the law of correspondence demanded only that the equation should contain not more than three constants for each body. The simplest extension is that made by Reinganum, in which he increased the pressure for a given mean kinetic energy of the particles inversely in the ratio of the diminution of free volume, due to the molecules possessing linear extension. Berthelot has shown how a 'reduced' isothermal may be got by taking two other prominent points as units of measurement instead of the critical co-ordinates. The most suggestive advance in the improvement of the van der Waals equation has been made by a lady, Mme. Christine Meyer. The idea at the base of this new development may be understood from the following general statement: van der Waals brings the van der Waals surfaces for all substances into coincidence at the point where volume, pressure, and temperature are nothing, and then stretches or compresses all the surfaces parallel to the three axes of volume, pressure, and temperature, until their critical points coincide. But on this plan the surfaces do not quite coincide, because the points where the three variables are respectively nothing are not corresponding points. Mme. Meyer's plan is to bring all the critical points first into coincidence, and then to compress or extend all the representative surfaces parallel to the three axes of volume, pressure, and temperature, until the surfaces coincide. In this way, taking twenty-nine different substances, she completely verifies from experiment van der Waals' law of correspondence. The theory of van der Waals has been one of the greatest importance in directing experimental investigation, and in attacking the difficult problems of the liquefaction of the most permanent gases. One of its greatest triumphs has been the proof that the critical constants and the boiling-point of hydrogen theoretically deduced by Wroblewski from a study of the isothermals of the gas taken far above the temperature of liquefaction are remarkably near the experimental

values. We may safely infer, therefore, that if hereafter a gas be discovered in small quantity even four times more volatile than liquid hydrogen, yet by a study of its isothermals at low temperature we shall succeed in finding its most important liquid constants, although the isolation of the real liquid may for the time be impossible. It is perhaps not too much to say that as a prolific source of knowledge in the department dealing with the continuity of state in matter, it would be necessary to go back to Carnot's cycle to find a proposition of greater importance than the theory of van der Waals and his development of the law of corresponding states.

It will be apparent from what has just been said that, thanks to the labours of Andrews, van der Waals, and others, theory had again far outrun experiment. We could calculate the constants and predict some of the simple physical characteristics of liquid oxygen, hydrogen, or nitrogen with a high degree of confidence long before any one of the three had been obtained in the static liquid condition permitting of the experimental verification of the theory. This was the more tantalising, because, with whatever confidence the chemist may anticipate the substantial corroboration of his theory, he also anticipates with almost equal conviction that as he approaches more and more nearly to the zero of absolute temperature, he will encounter phenomena compelling modification, revision, and refinement of formulas which fairly covered the facts previously known. Just as nearly seventy years ago chemists were waiting for some means of getting a temperature of 100 degrees below melting ice, so ten years ago they were casting about for the means of going 100 degrees lower still. The difficulty, it need hardly be said, increases in a geometrical rather than in an arithmetical ratio. Its magnitude may be estimated from the fact that to produce liquid air in the atmosphere of an ordinary laboratory is a feat analogous to the production of liquid water starting from steam at a white heat, and working with all the implements and surroundings at the same high temperature. The problem was not so much how to produce intense cold as how to save it when produced from being immediately levelled up by the relatively superheated surroundings. Ordinary non-conducting packings were inadmissible because they are both cumbrous and opaque, while in working near the limits of our resources it is essential that the product should be visible and readily handled. It was while puzzling over this mechanical and manipulative difficulty in 1892 that it occurred to me that the principle of an arrangement used nearly twenty years before in some calorimetric experiments, which was based upon the work of Dulong and Petit on radiation, might be employed with advantage as well to protect cold substances from heat as hot ones from rapid cooling. I therefore tried the effect of keeping liquefied gases in vessels having a double wall, the annular space between being very highly exhausted. Experiments showed that liquid air evaporated at only one-fifth of the rate prevailing when it was placed in

a similar unexhausted vessel, owing to the convective transference of heat by the gas particles being enormously reduced by the high vacuum. But, in addition, these vessels lend themselves to an arrangement by which radiant heat can also be cut off. It was found that when the inner walls were coated with a bright deposit of silver the influx of heat was diminished to one-sixth the amount entering without the metallic coating. The total effect of the high vacuum and the silvering is to reduce the ingoing heat to about 3 per cent. The efficiency of such vessels depends upon getting as high a vacuum as possible, and cold is one of the best means of effecting the desired exhaustion. All that is necessary is to fill completely the space that has to be exhausted with an easily condensable vapour, and then to freeze it out in a receptacle attached to the primary vessel that can be sealed off. The advantage of this method is that no air-pump is required, and that theoretically there is no limit to the degree of exhaustion that can be obtained. The action is rapid, provided liquid air is the cooling agent, and vapours like mercury, water, or benzol are employed. It is obvious that when we have to deal with such an exceptionally volatile liquid as hydrogen, the vapour filling may be omitted because air itself is now an easily condensable vapour. In other words, liquid hydrogen, collected in such vessels with the annular space full of air, immediately solidifies the air and thereby surrounds itself with a high vacuum. In the same way, when it shall be possible to collect a liquid boiling on the absolute scale at about 5 degrees, as compared with the 20 degrees of hydrogen, then you might have the annular space filled with the latter gas to begin with, and yet get directly a very high vacuum, owing to the solidification of the hydrogen. Many combinations of vacuum vessels can be arranged, and the lower the temperature at which we have to operate the more useful they become. Vessels of this kind are now in general use, and in them liquid air has crossed the American continent. Of the various forms, that variety is of special importance which has a spiral tube joining the bottom part of the walls, so that any liquid gas may be drawn off from the interior of such a vessel. In the working of regenerative coils such a device becomes all-important, and such special vessels cannot be dispensed with for the liquefaction of hydrogen.

In the early experiments of Pictet and Cailletet, cooling was produced by the sudden expansion of the highly compressed gas preferably at a low temperature, the former using a jet that lasted for some time, the latter an instantaneous adiabatic expansion in a strong glass tube. Neither process was practicable as a mode of producing liquid gases, but both gave valuable indications of partial change into the liquid state by the production of a temporary mist. Linde, however, saw that the continuous use of a jet of highly compressed gas, combined with regenerative cooling, must lead to liquefaction on account of what is called the Kelvin-Joule effect; and he succeeded in making a machine, based on this principle, capable of producing liquid air for industrial purposes. These experimenters had proved that, owing to molecular attraction, compressed

gases passing through a porous plug or small aperture were lowered in temperature by an amount depending on the difference of pressure and inversely as the square of the absolute temperature. This means that for a steady difference of pressure the cooling is greater the lower the temperature. The only gas that did not show cooling under such conditions was hydrogen. Instead of being cooled it became actually hotter. The reason for this apparent anomaly in the Kelvin-Joule effect is that every gas has a thermometric point of inversion above which it is heated and below which it is cooled. This inversion point, according to van der Waals, is six and three-quarter times the critical point. The efficiency of the Linde process depends on working with highly compressed gas well below the inversion temperature, and in this respect this point may be said to take the place of the critical one, when in the ordinary way direct liquefaction is being effected by the use of specific liquid cooling agents. The success of both processes depends upon working within a certain temperature range, only the Linde method gives us a much wider range of temperature within which liquefaction can be effected. This is not the case if, instead of depending on getting cooling by the internal work done by the attraction of the gas molecules, we force the compressed gas to do external work as in the well-known air machines of Kirk and Coleman. Both these inventors have pointed out that there is no limit of temperature, short of liquefaction of the gas in use in the circuit, that such machines are not capable of giving. While it is theoretically clear that such machines ought to be capable of maintaining the lowest temperatures, and that with the least expenditure of power, it is a very different matter to overcome the practical difficulties of working such machines under the conditions. Coleman kept a machine delivering air at *minus* 83 degrees for hours, but he did not carry his experiments any further. Recently Monsieur Claude, of Paris, has, however, succeeded in working a machine of this type so efficiently that he has managed to produce one litre of liquid air per horse power expended per hour in the running of the engine. This output is twice as good as that given by the Linde machine, and there is no reason to doubt that the yield will be still further improved. It is clear, therefore, that in the immediate future the production of liquid air and hydrogen will be effected most economically by the use of machines producing cold by the expenditure of mechanical work.

Liquid Hydrogen and Helium.

To the physicist the copious production of liquid air by the methods described was of peculiar interest and value as affording the means of attacking the far more difficult problem of the liquefaction of hydrogen, and even as encouraging the hope that liquid hydrogen might in time be employed for the liquefaction of yet more volatile elements, apart from the importance which its liquefaction must hold in the process of the steady advance towards the absolute zero. Hydrogen is an element of especial

interest, because the study of its properties and chemical relations led great chemists like Faraday, Dumas, Daniell, Graham, and Andrews to entertain the view that if it could ever be brought into the state of liquid or solid it would reveal metallic characters. Looking to the special chemical relations of the combined hydrogen in water, alkaline oxides, acids, and salts, together with the behaviour of these substances on electrolysis, we are forced to conclude that hydrogen behaves as the analogue of a metal. After the beautiful discovery of Graham that palladium can absorb some hundreds of times its own volume of hydrogen, and still retain its lustre and general metallic character, the impression that hydrogen was probably a member of the metallic group became very general. The only chemist who adopted another view was my distinguished predecessor, Professor Odling. In his 'Manual of Chemistry,' published in 1861, he pointed out that hydrogen has chlorous as well as basic relations, and that they are as decided, important, and frequent as its other relations. From such considerations he arrived at the conclusion that hydrogen is essentially a neutral or intermediate body, and therefore we should not expect to find liquid or solid hydrogen possess the appearance of a metal. This extraordinary prevision, so characteristic of Odling, was proved to be correct some thirty-seven years after it was made. Another curious anticipation was made by Dumas in a letter addressed to Pictet, in which he says that the metal most analogous to hydrogen is magnesium, and that probably both elements have the same atomic volume, so that the density of hydrogen, for this reason, would be about the value elicited by subsequent experiments. Later on, in 1872, when Newlands began to arrange the elements in periodic groups, he regarded hydrogen as the lowest member of the chlorine family; but Mendeleef in his later classification placed hydrogen in the group of the alkaline metals; on the other hand, Dr. Johnstone Stoney classes hydrogen with the alkaline earth metals and magnesium. From this speculative divergency it is clear no definite conclusion could be reached regarding the physical properties of liquid or solid hydrogen, and the only way to arrive at the truth was to prosecute low-temperature research until success attended the efforts to produce its liquefaction. This result I definitively obtained in 1898. The case of liquid hydrogen is, in fact, an excellent illustration of the truth already referred to, that no theoretical forecast, however apparently justified by analogy, can be finally accepted as true until confirmed by actual experiment. Liquid hydrogen is a colourless transparent body of extraordinary intrinsic interest. It has a clearly defined surface, is easily seen, drops well, in spite of the fact that its surface tension is only the thirty-fifth part of that of water, or about one-fifth that of liquid air, and can be poured easily from vessel to vessel. The liquid does not conduct electricity, and, if anything, is slightly diamagnetic. Compared with an equal volume of liquid air, it requires only one-fifth the quantity of heat for vaporisation; on the other hand, its specific heat is ten times that of liquid air or five times

that of water. The coefficient of expansion of the fluid is remarkable, being about ten times that of gas; it is by far the lightest liquid known to exist, its density being only one-fourteenth that of water; the lightest liquid previously known was liquid marsh gas, which is six times heavier. The only solid which has so small density as to float upon its surface is a piece of pith wood. It is by far the coldest liquid known. At ordinary atmospheric pressure it boils at *minus* 252·5 degrees or 20·5 degrees absolute. The critical point of the liquid is about 29 degrees absolute, and the critical pressure not more than fifteen atmospheres. The vapour of the hydrogen arising from the liquid has nearly the density of air—that is, it is fourteen times that of the gas at the ordinary temperature. Reduction of the pressure by an air-pump brings down the temperature to *minus* 258 degrees, when the liquid becomes a solid resembling frozen foam, and this by further exhaustion is cooled to *minus* 260 degrees, or 13 degrees absolute, which is the lowest steady temperature that has been reached. The solid may also be got in the form of a clear transparent ice, melting at about 15 degrees absolute, under a pressure of 55 mm., possessing the unique density of one-eleventh that of water. Such cold involves the solidification of every gaseous substance but one that is at present definitely known to the chemist, and so liquid hydrogen introduces the investigator to a world of solid bodies. The contrast between this refrigerating substance and liquid air is most remarkable. On the removal of the loose plug of cotton-wool used to cover the mouth of the vacuum vessel in which it is stored, the action is followed by a miniature snowstorm of solid air, formed by the freezing of the atmosphere at the point where it comes into contact with the cold vapour rising from the liquid. This solid air falls into the vessel and accumulates as a white snow at the bottom of the liquid hydrogen. When the outside of an ordinary test-tube is cooled by immersion in the liquid, it is soon observed to fill up with solid air, and if the tube be now lifted out a double effect is visible, for liquid air is produced both in the inside and on the outside of the tube—in the one case by the melting of the solid, and in the other by condensation from the atmosphere. A tuft of cotton-wool soaked in the liquid and then held near the pole of a strong magnet is attracted, and it might be inferred therefrom that liquid hydrogen is a magnetic body. This, however, is not the case: the attraction is due neither to the cotton-wool nor to the hydrogen—which indeed evaporates almost as soon as the tuft is taken out of the liquid—but to the oxygen of the air, which is well known to be a magnetic body, frozen in the wool by the extreme cold.

The strong condensing powers of liquid hydrogen afford a simple means of producing vacua of very high tenuity. When one end of a sealed tube containing ordinary air is placed for a short time in the liquid, the contained air accumulates as a solid at the bottom, while the higher part is almost entirely deprived of particles of gas. So perfect is the vacuum thus formed, that the electric discharge can be made to pass only

with the greatest difficulty. Another important application of liquid air, liquid hydrogen, &c., is as analytic agents. Thus, if a gaseous mixture be cooled by means of liquid oxygen, only those constituents will be left in the gaseous state which are less condensable than oxygen. Similarly, if this gaseous residue be in its turn cooled in liquid hydrogen a still further separation will be effected, everything that is less volatile than hydrogen being condensed to a liquid or solid. By proceeding in this fashion it has been found possible to isolate helium from a mixture in which it is present to the extent of only one part in one thousand. By the evaporation of solid hydrogen under the air-pump we can reach within 13 or 14 degrees of the zero, but there or thereabouts our progress is barred. This gap of 13 degrees might seem at first sight insignificant in comparison with the hundreds that have already been conquered. But to win one degree low down the scale is quite a different matter from doing so at higher temperatures; in fact, to annihilate these few remaining degrees would be a far greater achievement than any so far accomplished in low-temperature research. For the difficulty is twofold, having to do partly with process and partly with material. The application of the methods used in the liquefaction of gases becomes continually harder and more troublesome as the working temperature is reduced; thus, to pass from liquid air to liquid hydrogen—a difference of 60 degrees—is, from a thermodynamic point of view, as difficult as to bridge the gap of 150 degrees that separates liquid chlorine and liquid air. By the use of a new liquid gas exceeding hydrogen in volatility to the same extent as hydrogen does nitrogen, the investigator might get to within five degrees of the zero; but even a second hypothetical substance, again exceeding the first one in volatility to an equal extent, would not suffice to bring him quite to the point of his ambition. That the zero will ever be reached by man is extremely improbable. A thermometer introduced into regions outside the uttermost confines of the earth's atmosphere might approach the absolute zero, provided that its parts were highly transparent to all kinds of radiation, otherwise it would be affected by the radiation of the sun, and would therefore become heated. But supposing all difficulties to be overcome, and the experimenter to be able to reach within a few degrees of the zero, it is by no means certain that he would find the near approach of the death of matter sometimes pictured. Any forecast of the phenomena that would be seen must be based on the assumption that there is continuity between the processes studied at attainable temperatures and those which take place at still lower ones. Is such an assumption justified? It is true that many changes in the properties of substances have been found to vary steadily with the degree of cold to which they are exposed. But it would be rash to take for granted that the changes which have been traced in explored regions continue to the same extent and in the same direction in those which are as yet unexplored. Of such a breakdown low-temperature research has already

yielded a direct proof at least in one case. A series of experiments with pure metals showed that their electrical resistance gradually decreases as they are cooled to lower and lower temperatures, in such ratio that it appeared probable that at the zero of absolute temperature they would have no resistance at all and would become perfect conductors of electricity. This was the inference that seemed justifiable by observations taken at depths of cold which can be obtained by means of liquid air and less powerful refrigerants. But with the advent of the more powerful refrigerant liquid hydrogen it became necessary to revise that conclusion. A discrepancy was first observed when a platinum resistance thermometer was used to ascertain the temperature of that liquid boiling under atmospheric and reduced pressure. All known liquids, when forced to evaporate quickly by being placed in the exhausted receiver of an air-pump, undergo a reduction in temperature, but when hydrogen was treated in this way it appeared to be an exception. The resistance thermometer showed no such reduction as was expected, and it became a question whether it was the hydrogen or the thermometer that was behaving abnormally. Ultimately, by the adoption of other thermometrical appliances, the temperature of the hydrogen was proved to be lowered by exhaustion as theory indicated. Hence it was the platinum thermometer which had broken down; in other words, the electrical resistance of the metal employed in its construction was not, at temperatures about *minus* 250° C., decreased by cold in the same proportion as at temperatures about *minus* 200°. This being the case, there is no longer any reason to suppose that at the absolute zero platinum would become a perfect conductor of electricity; and in view of the similarity between the behaviour of platinum and that of other pure metals in respect of temperature and conductivity, the presumption is that the same is true of them also. At any rate, the knowledge that in the case of at least one property of matter we have succeeded in attaining a depth of cold sufficient to bring about unexpected change in the law expressing the variation of that property with temperature, is sufficient to show the necessity for extreme caution in extending our inferences regarding the properties of matter near the zero of temperature. Lord Kelvin evidently anticipates the possibility of more remarkable electrical properties being met with in the metals near the zero. A theoretical investigation on the relation of 'electrions' and atoms has led him to suggest a hypothetical metal having the following remarkable properties: below 1 degree absolute it is a perfect insulator of electricity, at 2 degrees it shows noticeable conductivity, and at 6 degrees it possesses high conductivity. It may safely be predicted that liquid hydrogen will be the means by which many obscure problems of physics and chemistry will ultimately be solved, so that the liquefaction of the last of the old permanent gases is as pregnant now with future consequences of great scientific moment as was the liquefaction of chlorine in the early years of the last century.

The next step towards the absolute zero is to find another gas more volatile than hydrogen, and that we possess in the gas occurring in cleveite,

identified by Ramsay as helium, a gas which is widely distributed, like hydrogen, in the sun, stars, and nebulae. A specimen of this gas was subjected by Olszewski to liquid air temperatures, combined with compression and subsequent expansion, following the Cailletet method, and resulted in his being unable to discover any appearance of liquefaction, even in the form of mist. His experiments led him to infer that the boiling-point of the substance is probably below 9 degrees absolute. After Lord Rayleigh had found a new source of helium in the gases which are derived from the Bath springs, and liquid hydrogen became available as a cooling agent, a specimen of helium cooled in liquid hydrogen showed the formation of fluid, but this turned out to be owing to the presence of an unknown admixture of other gases. As a matter of fact, a year before the date of this experiment I had recorded indications of the presence of unknown gases in the spectrum of helium derived from this source. When subsequently such condensable constituents were removed, the purified helium showed no signs of liquefaction, even when compressed to 80 atmospheres, while the tube containing it was surrounded with solid hydrogen. Further, on suddenly expanding, no instantaneous mist appeared. Thus helium was definitely proved to be a much more volatile substance than hydrogen in either the liquid or solid condition. The inference to be drawn from the adiabatic expansion effected under the circumstances is that helium must have touched a temperature of from 9 to 10 degrees for a short time without showing any signs of liquefaction, and consequently that the critical point must be still lower. This would force us to anticipate that the boiling-point of the liquid will be about 5 degrees absolute, or liquid helium will be four times more volatile than liquid hydrogen, just as liquid hydrogen is four times more volatile than liquid air. Although the liquefaction of the gas is a problem for the future, this does not prevent us from safely anticipating some of the properties of the fluid body. It would be twice as dense as liquid hydrogen, with a critical pressure of only 4 or 5 atmospheres. The liquid would possess a very feeble surface-tension, and its compressibility and expansibility would be about four times that of liquid hydrogen, while the heat required to vaporise the molecule would be about one-fourth that of liquid hydrogen. Heating the liquid 1 degree above its boiling-point would raise the pressure by $1\frac{3}{4}$ atmospheres, which is more than four times the increment for liquid hydrogen. The liquid would be only seventeen times denser than its vapour, whereas liquid hydrogen is sixty-five times denser than the gas it gives off. Only some 3 or 4 degrees would separate the critical temperature from the boiling-point and the melting-point, whereas in liquid hydrogen the separation is respectively 10 and 15 degrees. As the liquid refractivities for oxygen, nitrogen, and hydrogen are closely proportional to the gaseous values, and as Lord Rayleigh has shown that helium has only one-fourth the refractivity of hydrogen, although it is twice as dense, we must infer that the refractivity of liquid helium would also be about one-

fourth that of liquid hydrogen. Now hydrogen has the smallest refractivity of any known liquid, and yet liquid helium will have only about one-fourth of this value—comparable, in fact, with liquid hydrogen just below its critical point. This means that the liquid will be quite exceptional in its optical properties, and very difficult to see. This may be the explanation of why no mist has been seen on its adiabatic expansion from the lowest temperatures. Taking all these remarkable properties of the liquid into consideration, one is afraid to predict that we are at present able to cope with the difficulties involved in its production and collection. Provided the critical point is, however, not below 8 degrees absolute, then from the knowledge of the conditions that are successful in producing a change of state in hydrogen through the use of liquid air, we may safely predict that helium can be liquefied by following similar methods. If, however, the critical point is as low as 6 degrees absolute, then it would be almost hopeless to anticipate success by adopting the process that works so well with hydrogen. The present anticipation is that the gas will succumb after being subjected to this process, only, instead of liquid air under exhaustion being used as the primary cooling agent, liquid hydrogen evaporating under similar circumstances must be employed. In this case the resulting liquid would require to be collected in a vacuum vessel, the outer walls of which are immersed in liquid hydrogen. The practical difficulties and the cost of the operation will be very great; but on the other hand, the descent to a temperature within 5 degrees of the zero would open out new vistas of scientific inquiry, which would add immensely to our knowledge of the properties of matter. To command in our laboratories a temperature which would be equivalent to that which a comet might reach at an infinite distance from the sun would indeed be a great triumph for science. If the present Royal Institution attack on helium should fail, then we must ultimately succeed by adopting a process based on the mechanical production of cold through the performance of external work. When a turbine can be worked by compressed helium, the whole of the mechanism and circuits being kept surrounded with liquid hydrogen, then we need hardly doubt that the liquefaction will be effected. In all probability gases other than helium will be discovered of greater volatility than hydrogen. It was at the British Association Meeting in 1896 that I made the first suggestion of the probable existence of an unknown element which would be found to fill up the gap between argon and helium, and this anticipation was soon taken up by others and ultimately confirmed. Later, in the Bakerian Lecture for 1901, I was led to infer that another member of the helium group might exist having the atomic weight about 2, and this would give us a gas still more volatile, with which the absolute zero might be still more nearly approached. It is to be hoped that some such element or elements may yet be isolated and identified as coronium or nebulium. If amongst the unknown gases possessing a very low critical point some have a high critical pressure, instead of a low one, which ordinary experience would lead us to antici-

pate, then such difficultly liquefiable gases would produce fluids having different physical properties from any of those with which we are acquainted. Again, gases may exist having smaller atomic weights and densities than hydrogen, yet all such gases must, according to our present views of the gaseous state, be capable of liquefaction before the zero of temperature is reached. The chemists of the future will find ample scope for investigation within the apparently limited range of temperature which separates solid hydrogen from the zero. Indeed, great as is the sentimental interest attached to the liquefaction of these refractory gases, the importance of the achievement lies rather in the fact that it opens out new fields of research and enormously widens the horizon of physical science, enabling the natural philosopher to study the properties and behaviour of matter under entirely novel conditions. This department of inquiry is as yet only in its infancy, but speedy and extensive developments may be looked for, since within recent years several special cryogenic laboratories have been established for the prosecution of such researches, and a liquid-air plant is becoming a common adjunct to the equipment of the ordinary laboratory.

The Upper Air and Auroras.

The present liquid ocean, neglecting everything for the moment but the water, was at a previous period of the earth's history part of the atmosphere, and its condensation has been brought about by the gradual cooling of the earth's surface. This resulting ocean is subjected to the pressure of the remaining uncondensed gases, and as these are slightly soluble they dissolve to some extent in the fluid. The gases in solution can be taken out by distillation or by exhausting the water, and if we compare their volume with the volume of the water as steam, we should find about 1 volume of air in 60,000 volumes of steam. This would then be about the rough proportion of the relatively permanent gas to condensable gas which existed in the case of the vaporised ocean. Now let us assume the surface of the earth gradually cooled to some 200 degrees below the freezing-point; then, after all the present ocean was frozen, and the climate became three times more intense than any arctic frost, a new ocean of liquid air would appear, covering the entire surface of the frozen globe about thirty-five feet deep. We may now apply the same reasoning to the liquid air ocean that we formerly did to the water one, and this would lead us to anticipate that it might contain in solution some gases that may be far less condensable than the chief constituents of the fluid. In order to separate them we must imitate the method of taking the gases out of water. Assume a sample of liquid air cooled to the low temperature that can be reached by its own evaporation, connected by a pipe to a condenser cooled in liquid hydrogen; then any volatile gases present in solution will distil over with the first portions of the air, and can be pumped off, being uncondensable

at the temperature of the condenser. In this way, a gas mixture, containing, of the known gases, free hydrogen, helium, and neon, has been separated from liquid air. It is interesting to note in passing that the relative volatilities of water and oxygen are in the same ratio as those of liquid air and hydrogen, so that the analogy between the ocean of water and that of liquid air has another suggestive parallel. The total uncondensable gas separated in this way amounts to about one fifty-thousandth of the volume of the air, which is about the same proportion as the air dissolved in water. That free hydrogen exists in air in small amount is conclusively proved, but the actual proportion found by the process is very much smaller than Gautier has estimated by the combustion method. The recent experiments of Lord Rayleigh show that Gautier, who estimated the hydrogen present as one five-thousandth, has in some way produced more hydrogen than he can manage to extract from pure air by a repetition of the same process. The spectroscopic examination of these gases throws new light upon the question of the aurora and the nature of the upper air. On passing electric discharges through the tubes containing the most volatile of the atmospheric gases, they glow with a bright orange light, which is especially marked at the negative pole. The spectroscope shows that this light consists, in the visible part of the spectrum, chiefly of a succession of strong rays in the red, orange, and yellow, attributed to hydrogen, helium, and neon. Besides these, a vast number of rays, generally less brilliant, are distributed through the whole length of the visible spectrum. The greater part of these rays are of, as yet, unknown origin. The violet and ultra-violet part of the spectrum rivals in strength that of the red and yellow rays. As these gases probably include some of the gases that pervade interplanetary space, search was made for the prominent nebular, coronal, and auroral lines. No definite lines agreeing with the nebular spectrum could be found, but many lines occurred closely coincident with the coronal and auroral spectrum. But before discussing the spectroscopic problem it will be necessary to consider the nature and condition of the upper air.

According to the old law of Dalton, supported by the modern dynamical theory of gases, each constituent of the atmosphere while acted upon by the force of gravity forms a separate atmosphere, completely independent, except as to temperature, of the others, and the relations between the common temperature and the pressure and altitude for each specific atmosphere can be definitely expressed. If we assume the altitude and temperature known, then the pressure can be ascertained for the same height in the case of each of the gaseous constituents, and in this way the percentage composition of the atmosphere at that place may be deduced. Suppose we start with a surface atmosphere having the composition of our air, only containing two ten-thousandths of hydrogen, then at thirty-seven miles, if a sample could be procured for analysis, we believe that it would be found to contain 12 per cent. of hydrogen and

only 10 per cent. of oxygen. The carbonic acid practically disappears; and by the time we reach forty-seven miles, where the temperature is *minus* 132 degrees, assuming a gradient of 3.2 degrees per mile, the nitrogen and oxygen have so thinned out that the only constituent of the upper air which is left is hydrogen. If the gradient of temperature were doubled, the elimination of the nitrogen and oxygen would take place by the time thirty-seven miles was reached, with a temperature of *minus* 220 degrees. The permanence of the composition of the air at the highest altitudes, as deduced from the basis of the dynamical theory of gases, has been discussed by Stoney, Bryan, and others. It would appear that there is a consensus of opinion that the rate at which gases like hydrogen and helium could escape from the earth's atmosphere would be excessively slow. Considering that to compensate any such loss the same gases are being supplied by actions taking place in the crust of the earth, we may safely regard them as necessarily permanent constituents of the upper air. The temperature at the elevations we have been discussing would not be sufficient to cause any liquefaction of the nitrogen and oxygen, the pressure being so low. If we assume the mean temperature as about the boiling-point of oxygen at atmospheric pressure, then a considerable amount of the carbonic acid must solidify as a mist, if the air from a lower level be cooled to this temperature; and the same result might take place with other gases of relatively small volatility which occur in air. This would explain the clouds that have been seen at an elevation of fifty miles, without assuming the possibility of water vapour being carried up so high. The temperature of the upper air must be above that on the vapour pressure curve corresponding to the barometric pressure at the locality, otherwise liquid condensation must take place. In other words, the temperature must be above the dew-point of air at that place. At higher elevations, on any reasonable assumption of temperature distribution, we inevitably reach a temperature where the air would condense, just as Fourier and Poisson supposed it would, unless the temperature is arrested in some way from approaching the zero. Both ultra-violet absorption and the prevalence of electric storms may have something to do with the maintenance of a higher mean temperature. The whole mass of the air above forty miles is not more than one seven-hundredth part of the total mass of the atmosphere, so that any rain or snow of liquid or solid air, if it did occur, would necessarily be of a very tenuous description. In any case, the dense gases tend to accumulate in the lower strata, and the lighter ones to predominate at the higher altitudes, always assuming that a steady state of equilibrium has been reached. It must be observed, however, that a sample of air taken at an elevation of nine miles has shown no difference in composition from that at the ground, whereas, according to our hypothesis, the oxygen ought to have been diminished to 17 per cent., and the carbonic acid should also have become much less. This can only be explained by assuming that a large intermixture of different layers of the atmosphere is still taking place at this.

elevation. This is confirmed by a study of the motions of clouds about six miles high, which reveals an average velocity of the air currents of some seventy miles an hour ; such violent winds must be the means of causing the intermingling of different atmospheric strata. Some clouds, however, during hot and thundery weather, have been seen to reach an elevation of seventeen miles, so that we have direct proof that on occasion the lower layers of atmosphere are carried to a great elevation. The existence of an atmosphere at more than a hundred miles above the surface of the earth is revealed to us by the appearance of meteors and fireballs, and when we can take photographs of the spectrum of such apparitions we shall learn a great deal about the composition of the upper air. In the meantime Pickering's solitary spectrum of a meteor reveals an atmosphere of hydrogen and helium, and so far this is corroborative of the doctrine we have been discussing. It has long been recognised that the aurora is the result of electric discharges within the limits of the earth's atmosphere, but it was difficult to understand why its spectrum should be so entirely different from anything which could be produced artificially by electric discharges through rarefied air at the surface of the earth. Writing in 1879, Rand Capron, after collecting all the recorded observations, was able to enumerate no more than nine auroral rays, of which but one could with any probability be identified with rays emitted by atmospheric air under an electric discharge. Vogel attributed this want of agreement between nature and experiment, in a vague way, to difference of temperature and pressure ; and Zollner thought the auroral spectrum to be one of a different order, in the sense in which the line and band spectra of nitrogen are said to be of different orders. Such statements were merely confessions of ignorance. But since that time observations of the spectra of auroras have been greatly multiplied, chiefly through the Swedish and Danish Polar Expeditions, and the length of spectrum recorded on the ultra-violet side has been greatly extended by the use of photography, so that, in a recent discussion of the results, M. Henri Stassano is able to enumerate upwards of one hundred auroral rays, of which the wave-length is more or less approximately known, some of them far in the ultra-violet. Of this large number of rays he is able to identify, within the probable limits of errors of observation, about two-thirds as rays, which Professor Liveing and myself have observed to be emitted by the most volatile gases of atmospheric air unliquefiable at the temperature of liquid hydrogen. Most of the remainder he ascribes to argon, and some he might, with more probability, have identified with krypton or xenon rays, if he had been aware of the publication of wave-lengths of the spectra of those gases, and the identification of one of the highest rays of krypton with that most characteristic of auroras. The rosy tint often seen in auroras, particularly in the streamers, appears to be due mainly to neon, of which the spectrum is remarkably rich in red and orange rays. One or two neon rays are amongst those most frequently observed, while the red ray of hydrogen and one red ray of krypton have been noticed only

once. The predominance of neon is not surprising, seeing that from its relatively greater proportion in air and its low density it must tend to concentrate at higher elevations. So large a number of probable identifications warrants the belief that we may yet be able to reproduce in our laboratories the auroral spectrum in its entirety. It is true that we have still to account for the appearance of some, and the absence of other, rays of the newly discovered gases, which in the way in which we stimulate them appear to be equally brilliant, and for the absence, with one doubtful exception, of all the rays of nitrogen. If we cannot give the reason of this, it is because we do not know the mechanism of luminescence—nor even whether the particles which carry the electricity are themselves luminous, or whether they only produce stresses causing other particles which encounter them to vibrate; yet we are certain that an electric discharge in a highly rarefied mixture of gases lights one element and not another, in a way which, to our ignorance, seems capricious. The Swedish North Polar Expedition concluded from a great number of trigonometrical measurements that the average above the ground of the base of the aurora was fifty kilometres (thirty-four miles) at Cape Thorsden, Spitzbergen; at this height the pressure of the nitrogen of the atmosphere would be only about one-tenth of a millimetre, and Moissan and Deslandres have found that in atmospheric air at pressures less than one millimetre the rays of nitrogen and oxygen fade and are replaced by those of argon and by five new rays which Stassano identifies with rays of the more volatile gases measured by us. Also Collie and Ramsay's observations on the distance to which electrical discharges of equal potential traverse different gases explosively throw much light on the question; for they find that, while for helium and neon this distance is from 250 to 300 mm., for argon it is $45\frac{1}{2}$ mm., for hydrogen it is 39 mm., and for air and oxygen still less. This indicates that a good deal depends on the very constitution of the gases themselves, and certainly helps us to understand why neon and argon, which exist in the atmosphere in larger proportions than helium, krypton, or xenon, should make their appearance in the spectrum of auroras almost to the exclusion of nitrogen and oxygen. How much depends not only on the constitution and it may be temperature of the gases, but also on the character of the electric discharge, is evident from the difference between the spectra at the cathode and anode in different gases, notably in nitrogen and argon, and not less remarkably in the more volatile compounds of the atmosphere. Paulsen thinks the auroral spectrum wholly due to cathodic rays. Without stopping to discuss that question, it is certain that changes in the character of the electric discharge produce definite changes in the spectra excited by them. It has long been known that in many spectra the rays which are inconspicuous with an uncondensed electric discharge become very pronounced when a Leyden jar is in the circuit. This used to be ascribed to a higher temperature in this condensed spark, though measurements of that

temperature have not borne out the explanation. Schuster and Hemsalech have shown that these changes of spectra are in part due to the oscillatory character of the condenser discharge which may be enhanced by self-induction, and the corresponding change of spectrum thereby made more pronounced. Lightning we should expect to resemble condensed discharge much more than aurora, but this is not borne out by the spectrum. Pickering's recent analysis of the spectrum of a flash obtained by photography shows, out of nineteen lines measured by him, only two which can be assigned with probability to nitrogen and oxygen, while three hydrogen rays most likely due to water are very conspicuous, and eleven may be reasonably ascribed to argon, krypton, and xenon, one to more volatile gas of the neon class, and the brightest ray of all is but a very little less refrangible than the characteristic auroral ray, and coincides with a strong ray of calcium, but also lies between, and close to, an argon and a neon ray, neither of them weak rays. There may be some doubt about the identification of the spectral rays of auroras because of the wide limits of the probable errors in measuring wave-lengths so faint as most of them are, but there is no such doubt about the wave-lengths of the rays in solar protuberances measured by Deslandres and Hale. Stassano found that these rays, forty-four in number, lying between the Fraunhofer line *F* and 3148 in the ultra-violet agree very closely with rays which Professor Liveing and myself measured in the spectra of the most volatile atmospheric gases. It will be remembered that one of the earliest suggestions as to the nature of solar prominences was that they were solar auroras. This supposition helped to explain the marvellous rapidity of their changes, and the apparent suspension of brilliant self-luminous clouds at enormous heights above the sun's surface. Now the identification of the rays of their spectra with those of the most volatile gases, which also furnish many of the auroral rays, certainly supports that suggestion. A stronger support, however, seems to be given to it by the results obtained at the total eclipse of May 1901, by the American expedition to Sumatra. In the 'Astrophysical Journal' for June last is a list of 339 lines in the spectrum of the corona photographed by Humphreys, during totality, with a very large concave grating. Of these no fewer than 209 do not differ from lines we have measured in the most volatile gases of the atmosphere, or in krypton or xenon, by more than one unit of wave-length on Armstrong's scale, a quantity within the limit of probable error. Of the remainder, a good many agree to a like degree with argon lines, a very few with oxygen lines, and still fewer with nitrogen lines; the characteristic green auroral ray, which is not in the range of Humphreys' photographs, also agrees within a small fraction of a unit of wave-length with one of the rays emitted by the most volatile atmospheric gas. Taking into account the Fraunhofer lines *H*, *K*, and *G*, usually ascribed to calcium, there remain only fifty-five lines of the 339 unaccounted for to the degree of probability indicated. Of these considerably more than half are very weak lines which have not depicted

themselves on more than one of the six films exposed, and extend but a very short distance into the sun's atmosphere. There are, however, seven which are stronger lines, and reach to a considerable height above the sun's rim, and all have depicted themselves on at least four of the six films. If there be no considerable error in the wave-lengths assigned (and such is not likely to be the case), these lines may perhaps be due to some volatile element which may yet be discovered in our atmosphere. However that may be, the very great number of close coincidences between the auroral rays and those which are emitted under electric excitement by gases of our atmosphere almost constrains us to believe, what is indeed most probable on other grounds, that the sun's coronal atmosphere is composed of the same substances as the earth's, and that it is rendered luminous in the same way—namely, by electric discharges. This conclusion has plainly an important bearing on the explanation which should be given of the outburst of new stars and of the extraordinary and rapid changes in their spectra. Moreover, leaving on one side the question whether gases ever become luminous by the direct action of heat, apart from such transfers of energy as occur in chemical change and electric disturbance, it demands a revision of the theories which attribute more permanent differences between the spectra of different stars to differences of temperature, and a fuller consideration of the question whether they cannot with better reason be explained by differences in the electric conditions which prevail in the stellar atmosphere.

If we turn to the question what is the cause of the electric discharges which are generally believed to occasion auroras, but of which little more has hitherto been known than that they are connected with sun-spots and solar eruptions, recent studies of electric discharges in high vacua, with which the names of Crookes, Röntgen, Lenard, and J. J. Thomson will always be associated, have opened the way for Arrhenius to suggest a definite and rational answer. He points out that the frequent disturbances which we know to occur in the sun must cause electric discharges in the sun's atmosphere far exceeding any that occur in that of the earth. These will be attended with an ionisation of the gases, and the negative ions will stream away through the outer atmosphere of the sun into the interplanetary space, becoming, as Wilson has shown, nuclei of aggregation of condensable vapours and cosmic dust. The liquid and solid particles thus formed will be of various sizes; the larger will gravitate back to the sun, while those with diameters less than one and a half thousandths of a millimetre, but nevertheless greater than a wave-length of light, will, in accordance with Clerk-Maxwell's electromagnetic theory, be driven away from the sun by the incidence of the solar rays upon them, with velocities which may become enormous, until they meet other celestial bodies, or increase their dimensions by picking up more cosmic dust or diminish them by evaporation. The earth will catch its share of such particles on the side which is turned towards the sun, and its upper atmosphere will thereby become negatively electrified until the

potential of the charge reaches such a point that a discharge occurs, which will be repeated as more charged particles reach the earth. This theory not only accounts for the auroral discharges, and the coincidence of their times of greatest frequency with those of the maxima of sunspots, but also for the minor maxima and minima. The vernal and autumnal maxima occur when the line through the earth and sun has its greatest inclination to the solar equator, so that the earth is more directly exposed to the region of maximum of sunspots, while the twenty-six days period corresponds closely with the period of rotation of that part of the solar surface where faculae are most abundant. J. J. Thomson has pointed out, as a consequence of the Richardson observations, that negative ions will be constantly streaming from the sun merely regarded as a hot body, but this is not inconsistent with the supposition that there will be an excess of this emission in eruptions, and from the regions of faculae. Arrhenius' theory accounts also, in a way which seems the most satisfactory hitherto enunciated, for the appearances presented by comets. The solid parts of these objects absorb the sun's rays, and as they approach the sun become heated on the side turned towards him until the volatile substances frozen in or upon them are evaporated and diffused in the gaseous state in surrounding space, where they get cooled to the temperature of liquefaction and aggregated in drops about the negative ions. The larger of these drops gravitate towards the sun and form clouds of the coma about the head, while the smaller are driven by the incidence of the sun's light upon them away from the sun and form the tail. The curvature of the tail depends, as Bredichin has shown, on the rate at which the particles are driven, which in turn depends on the size and specific gravity of the particles, and these will vary with the density of the vapour from which they are formed and the frequency of the negative ions which collect them. In any case Arrhenius' theory is a most suggestive one, not only with reference to auroras and comets, and the solar corona and chromosphere, but also as to the constitution of the photosphere itself.

Various Low-Temperature Researches.

We may now summarise some of the results which have already been attained by low-temperature studies. In the first place, the great majority of chemical interactions are entirely suspended, but an element of such exceptional powers of combination as fluorine is still active at the temperature of liquid air. Whether solid fluorine and liquid hydrogen would interact no one can at present say. Bodies naturally become denser, but even a highly expansive substance like ice does not appear to reach the density of water at the lowest temperature. This is confirmatory of the view that the particles of matter under such conditions are not packed in the closest possible way. The force of cohesion is greatly increased at low temperatures, as is shown by the additional stress required to rupture metallic wires. This fact is of interest in connection with two conflicting theories of matter. Lord Kelvin's view is that the forces that hold

together the particles of bodies may be accounted for without assuming any other agency than gravitation or any other law than the Newtonian. An opposite view is that the phenomena of the aggregation of molecules depend upon the molecular vibration as a physical cause. Hence, at the zero of absolute temperature, this vibrating energy being in complete abeyance, the phenomena of cohesion should cease to exist, and matter generally be reduced to an incoherent heap of cosmic dust. This second view receives no support from experiment.

The photographic action of light is diminished at the temperature of liquid air to about 20 per cent. of its ordinary efficiency, and at the still lower temperature of liquid hydrogen only about 10 per cent. of the original sensitivity remains. At the temperature of liquid air or liquid hydrogen a large range of organic bodies and many inorganic ones acquire under exposure to violet light the property of phosphorescence. Such bodies glow faintly so long as they are kept cold, but become exceedingly brilliant during the period when the temperature is rising. Even solid air is a phosphorescent body. All the alkaline earth sulphides which phosphoresce brilliantly at the ordinary temperature lose this property when cooled, to be revived on heating; but such bodies in the first instance may be stimulated through the absorption of light at the lowest temperatures. Radio-active bodies, on the other hand, like radium, which are naturally self-luminous, maintain this luminosity unimpaired at the very lowest temperatures, and are still capable of inducing phosphorescence in bodies like the platino-cyanides. Some crystals become for a time self-luminous when cooled in liquid air or hydrogen, owing to the induced electric stimulation causing discharges between the crystal molecules. This phenomenon is very pronounced with nitrate of uranium and some platino-cyanides.

In conjunction with Professor Fleming a long series of experiments was made on the electric and magnetic properties of bodies at low temperatures. The subjects that have been under investigation may be classified as follows: The Thermo-Electric Powers of Pure Metals; The Magnetic Properties of Iron and Steel; Dielectric Constants; The Magnetic and Electric Constants of Liquid Oxygen; Magnetic Susceptibility.

The investigations have shown that electric conductivity in pure metals varies almost inversely as the absolute temperature down to *minus* 200 degrees, but that this law is greatly affected by the presence of the most minute amount of impurity. Hence the results amount to a proof that electric resistance in pure metals is closely dependent upon the molecular or atomic motion which gives rise to temperature, and that the process by which the energy constituting what is called an electric current is dissipated essentially depends upon non-homogeneity of structure and upon the absolute temperature of the material. It might be inferred that at the zero of absolute temperature resistance would vanish altogether, and all pure metals become perfect conductors of electricity. This con-

clusion, however, has been rendered very doubtful by subsequent observations made at still lower temperatures, which appear to point to an ultimate finite resistance. Thus the temperature at which copper was assumed to have no resistance was *minus* 223 degrees, but that metal has been cooled to *minus* 253 degrees without getting rid of all resistance. The reduction in resistance of some of the metals at the boiling-point of hydrogen is very remarkable. Thus copper has only 1 per cent., gold and platinum 3 per cent., and silver 4 per cent. of the resistance they possessed at zero C., but iron still retains 12 per cent. of its initial resistance. In the case of alloys and impure metals, cold brings about a much smaller decrease in resistivity, and in the case of carbon and insulators like gutta-percha, glass, ebonite, &c., their resistivity steadily increases. The enormous increase in resistance of bismuth when transversely magnetised and cooled was also discovered in the course of these experiments. The study of dielectric constants at low temperatures has resulted in the discovery of some interesting facts. A fundamental deduction from Maxwell's theory is that the square of the refractive index of a body should be the same number as its dielectric constant. So far, however, from this being the case generally, the exceptions are far more numerous than the coincidences. It has been shown in the case of many substances, such as ice and glass, that an increase in the frequency of the alternating electromotive force results in a reduction of the dielectric constant to a value more consistent with Maxwell's law. By experiments upon many substances it is shown that even a moderate increase of frequency brings the large dielectric constant to values quite near to that required by Maxwell's law. It was thus shown that low temperature has the same effect as high frequency in annulling the abnormal dielectric values. The exact measurement of the dielectric constant of liquid oxygen as well as its magnetic permeability, combined with the optical determination of the refractive index, showed that liquid oxygen strictly obeys Maxwell's electro-optic law even at very low electric frequencies. In magnetic work the result of greatest value is the proof that magnetic susceptibility varies inversely as the absolute temperature. This shows that the magnetisation of paramagnetic bodies is an affair of orientation of molecules, and it suggests that at the absolute zero all the feebly paramagnetic bodies will be strongly magnetic. The diamagnetism of bismuth was found to be increased at low temperatures. The magnetic moment of a steel magnet is temporarily increased by cooling in liquid air, but the increase seems to have reached a limit, because on further cooling to the temperature of liquid hydrogen hardly any further change was observed. The study of the thermo-electric relations of the metals at low temperatures resulted in a great extension of the well-known Tait Thermo-Electric Diagram. Tait found that the thermo-electric power of the metals could be expressed by a linear function of the absolute temperature, but at the extreme range of temperature now under consideration this law was found not to hold generally; and further, it appeared that many abrupt

electric changes take place, which originate probably from specific molecular changes occurring in the metal. The thermo-electric neutral points of certain metals, such as lead and gold, which are located about or below the boiling-point of hydrogen, have been found to be a convenient means of defining specific temperatures in this exceptional part of the scale.

The effect of cold upon the life of living organisms is a matter of great intrinsic interest, as well as of wide theoretical importance. Experiment indicates that moderately high temperatures are much more fatal, at least to the lower forms of life, than are exceedingly low ones. Professor McKendrick froze for an hour at a temperature of 182° C. samples of meat, milk, &c., in sealed tubes; when these were opened after being kept at blood heat for a few days, their contents were found to be quite putrid. More recently some more elaborate tests were carried out at the Jenner Institute of Preventive Medicine on a series of typical bacteria. These were exposed to the temperature of liquid air for twenty hours, but their vitality was not affected, their functional activities remained unimpaired, and the cultures which they yielded were normal in every respect. The same result was obtained when liquid hydrogen was substituted for air. A similar persistence of life in seeds has been demonstrated even at the lowest temperatures; they were frozen for over a hundred hours in liquid air, at the instance of Messrs. Brown and Escombe, with no other result than to affect their protoplasm with a certain inertness, from which it recovered with warmth. Subsequently commercial samples of barley, pea, vegetable-marrow, and mustard seeds were literally steeped for six hours in liquid hydrogen at the Royal Institution, yet when they were sown by Sir W. T. Thiselton Dyer at Kew in the ordinary way, the proportion in which germination occurred was no less than in the other batches of the same seeds which had suffered no abnormal treatment. Bacteria are minute vegetable cells, the standard of measurement for which is the 'mikron.' Yet it has been found possible to completely triturate these microscopic cells, when the operation is carried out at the temperature of liquid air, the cells then being frozen into hard breakable masses. The typhoid organism has been treated in this way, and the cell plasma obtained for the purpose of studying its toxic and immunising properties. It would hardly have been anticipated that liquid air should find such immediate application in biological research. A research by Professor Macfadyen, just concluded, has shown that many varieties of micro-organisms can be exposed to the temperature of liquid air for a period of six months without any appreciable loss of vitality, although at such a temperature the ordinary chemical processes of the cell must cease. At such a temperature the cells cannot be said to be either alive or dead, in the ordinary acceptation of these words. It is a new and hitherto unobtainable condition of living matter—a third state. A final instance of the application of the above methods may be given. Certain species of bacteria during the course of their vital processes are capable of emitting

light. If, however, the cells be broken up at the temperature of liquid air, and the crushed contents brought to the ordinary temperature, the luminosity function is found to have disappeared. This points to the luminosity not being due to the action of a ferment—a 'Luciferase'—but as being essentially bound up with the vital processes of the cells, and dependent for its production on the intact organisation of the cell. These attempts to study by frigorific methods the physiology of the cell have already yielded valuable and encouraging results, and it is to be hoped that this line of investigation will continue to be vigorously prosecuted at the Jenner Institute.

And now, to conclude an address which must have sorely taxed your patience, I may remind you that I commenced by referring to the plaint of Elizabethan science, that cold was not a natural available product. In the course of a long struggle with nature, man, by the application of intelligent and steady industry, has acquired a control over this agency which enables him to produce it at will, and with almost any degree of intensity, short of a limit defined by the very nature of things. But the success in working what appears, at first sight, to be a quarry of research that would soon suffer exhaustion, has only brought him to the threshold of new labyrinths, the entanglements of which frustrate, with a seemingly invulnerable complexity, the hopes of further progress. In a legitimate sense all genuine scientific workers feel that they are 'the inheritors of unfulfilled renown.' The battlefields of science are the centres of a perpetual warfare, in which there is no hope of final victory, although partial conquest is ever triumphantly encouraging the continuance of the disciplined and strenuous attack on the seemingly impregnable fortress of Nature. To serve in the scientific army, to have shown some initiative, and to be rewarded by the consciousness that in the eyes of his comrades he bears the accredited accolade of successful endeavour, is enough to satisfy the legitimate ambition of every earnest student of Nature. The real warranty that the march of progress in the future will be as glorious as in the past lies in the perpetual reinforcement of the scientific ranks by recruits animated by such a spirit, and proud to obtain such a reward.

British Association for the Advancement of

BELFAST, 1902.

ADDRESS

TO THE

MATHEMATICAL AND PHYSICAL SECTION

BY

PROFESSOR JOHN PURSER, M.A., LL.D., M.R.I.A.,

PRESIDENT OF THE SECTION.

IN opening our proceedings to-day allow me at the outset to express my deep sense of the honour the Association has conferred upon me in asking me to preside over this Section.

My predecessors in this Chair have usually given you a survey of some department of Mathematics or Physics, tracing what had been already accomplished in that department and indicating the nature of the problems which still awaited solution.

May I crave your indulgence if I deviate from this course and, following the suggestion of some of my friends, take the opportunity of the Association meeting on Irish soil to give you a slight historical sketch of our Irish School of Mathematics and Physics?

In attempting such a review, for the sake of brevity as well as for other reasons, I shall confine it to the work of those who are no longer with us, and I would not carry it further back than the beginning of last century. This seems a natural starting point, as there was at that time a very marked revival of the study of science in the University of Dublin, a revival largely due to the influence of Provost Bartholomew Lloyd.

Lloyd won his Fellowship in Trinity College a few years before the century opened, and subsequently filled in succession the Chairs of Mathematics and Natural Philosophy. In both departments he imported a radical change into the methods of teaching. By his treatises on Analytical Geometry and on Mechanical Philosophy he introduced the study of what was then called the French Mathematics, in other words the more advanced Analytic Methods, which were in use on the Continent. In 1831 he was appointed Provost of the College, and his tenure of the office, though brief, was signalised by many important improvements and new developments effected in the University teaching.

Dr. Bartholomew Lloyd was President of one of the earliest Meetings of this Association, that held in Dublin in 1835.

His son, Dr. Humphrey Lloyd, had a course which was a singularly close parallel to his father's.

He won his Fellowship in 1824, and succeeded his father in the Chair of Natural Philosophy. He also was afterwards appointed Provost, and he too presided over another Dublin Meeting of this Association, that held in 1857. He also, in this again following in his father's steps, wrote important works on different branches of Physics; 'Light and Vision,' a systematic treatise on plane as distinct from physical optics, 'Lectures on the Wave Theory of Light,' and lastly a treatise on 'Magnetism.'

It is, perhaps, in connection with this latter subject that his most important work was done. He made in association with Sabine an elaborate series of observations on terrestrial magnetism in twenty-four stations in various parts of Ireland, and when subsequently, at the instance of your Association and of the Royal Society, the Government established magnetic observatories in different parts of the world, it was Lloyd who was entrusted with the task of drawing up the manual of instructions for the observers and of receiving their reports.

In the interval between the two Lloyds another name claims attention. Dr. Romney Robinson occupied during an exceptionally long life a much honoured and influential position amongst men of science. It was in this city he received his early education, for when young Robinson was only nine years of age his father had occasion to move to Belfast, and he placed his son under Dr. Bruce, a well-known schoolmaster of those days. Robinson was afterwards sent to Trinity College, and after a distinguished course was elected to a Fellowship in 1814. For some years he lectured in college as Deputy Professor of Natural Philosophy. He relinquished his Fellowship on obtaining a College living, and a few years later was appointed Astronomer in charge of the Armagh Observatory. The results of his observations were considered so valuable as to be used by the German astronomer Argeländer in determining the proper motions of stars. The range, however, of his published papers was by no means confined to Astronomy, but extended to the most varied subjects, Heat, Electricity, Magnetism, Turbines, Air-pumps, Fog-signals, and others. He is best known to the general public as the inventor of the Cup Anemometer. He was chosen to preside over the Birmingham Meeting of this Association in 1849.

Robinson was intimately associated with Lord Rosse and keenly interested in the experiments which culminated in the construction of the great reflector in Parsonstown. This naturally leads us to speak of Lord Rosse himself. Few scientific achievements took a greater hold upon the public mind than the successful completion of his great telescope. Only those who have read in Lord Rosse's own papers the description of the many difficulties he had to contend with in forging and polishing that wonderful speculum, harder than steel yet more brittle than glass, can adequately appreciate the patience and resource with which those difficulties were successively overcome.

Of the results obtained with this instrument the most notable were in the observation of the Nebulæ, a department where its unsurpassed power of light-concentration came fully into play. No doubt at the time public attention was most excited by the resolution of a number of hitherto supposed nebulae into star clusters, leading to the premature conclusion in the minds of those less instructed that all the nebulae might ultimately be so resolved. To us, however, a far greater interest attaches to the observation of the structure of what we now know to be genuine nebulae, especially the great discovery that these had in many cases a peculiar spiral form. All previous telescopes had failed to detect this spiral character; but the drawings taken by Lord Rosse and his assistants put this feature beyond question, and these have been fully confirmed in recent years, when more accurate delineations were obtained by photography. I need not dwell upon the significance of this form, indicating, as it does, a rotatory movement in these mighty masses and fitting in with, if not actually confirming, Laplace's Nebular Hypothesis.

Sir William Rowan Hamilton was undoubtedly the most striking figure in the annals of the Dublin School of Mathematics. *In limine* we must make good our right to call him an Irishman, for his greatest admirer and disciple, Professor Tait, has claimed him for a countryman of his own, asserting that Hamilton's grandfather was a Scotchman who migrated to Dublin with his two young sons. That this was a complete misconception has been abundantly proved by the careful investigations of his friend and biographer, Dr. R. P. Graves, who shows conclusively that the only known strain of Scotch blood in Hamilton came through his grandmother, who was the daughter of a minister of the Scottish Kirk.

It is interesting to find how early Hamilton's remarkable mental powers began

to show themselves. Dr. Graves has given us a letter from his mother in which she writes to her sister of the marvellous precocity of her little four-year-old boy, telling how 'he reads Latin, Greek, and Hebrew.'

His mental development did not belie these early indications, for at the age of thirteen, thanks to the teaching and care of his uncle, who was a most extraordinary linguist, he had not only acquired a considerable knowledge of the classics and the modern European languages, but also attained some proficiency in Arabic, Sanscrit, and Persian. His mathematical studies, on the other hand, appear to have been carried on without help from anyone, and it is noteworthy that he does not seem to have used common text-books, but to have gone direct to the great original authors; *e.g.*, he read his algebra in Newton's '*Arithmetica Universalis*'; while at the age of fifteen he set himself to read the '*Principia*,' and two years later began a systematic study of Laplace's '*Mécanique Céleste*.' His own estimate of his powers may be gathered from a characteristic letter to his sister written just after he had entered Trinity College:—

'One thing only have I to regret in the direction of my studies, that they should be diverted—or rather rudely forced—by the College course from their natural bent and favourite channel. That bent, you know, is science—science in its most exalted heights, in its most secret recesses. It has so captivated me, so seized on, I may say, my affections that my attention to classical studies is an effort and an irksome one; and I own that, before I entered College, I did not hope that in them I would rise above mediocrity. My success surprised me, but it has also given me a spur by holding out a prospect that even in the less agreeable part of my business I may hope still to succeed.'

This letter is interesting as indicating on Hamilton's part a consciousness wherein lay his real strength and vocation. Not that his interest in literature ever abated. To the last he loved to try his hand at poetical composition, frequently inserting in his letters to his friends sonnets of his own.

He knew Wordsworth intimately, and the poet to whom he sent some of his productions gives him the following candid advice:—

'It would be insincere not to say that something of a style more terse and a harmony more accurately balanced must be acquired before the bodily form of your verses will be quite worthy of their living souls. You are perfectly aware of this, though perhaps not in an equal degree with myself; nor is it desirable you should be, for it might tempt you to labour which would divert you from subjects of infinitely greater importance.'

Hamilton was first in his College classes in every subject and at every examination, and it was fully expected that he would carry off both the medals in Mathematics and Classics at his Degree when the following circumstances suddenly changed all his plans. Dr. Brinkley, the Professor of Astronomy in the University, was appointed to a Bishopric, and Hamilton, though still an undergraduate, was invited to offer himself for the vacant Chair. Sir George Airy and more than one of the Fellows of Trinity were also candidates, but Hamilton was unanimously elected.

His career as an original author dates from this time, for immediately after his appointment he communicated to the Royal Irish Academy the first of three remarkable papers on '*Systems of Rays*.'

Two striking features may be observed in these papers, as indeed in all his scientific memoirs: the generality and comprehensiveness with which he states his object at the outset and the confidence with which he follows the bold and original lines of treatment which he lays down for himself, and closely connected with this, the determination not to be baffled by any laboriousness of calculations which the application of his method may involve him in. In his first paper he begins by examining what happens to a system of rays of light emanating from a point and subjected to any number of reflections at curved surfaces. He establishes the theorem that such a system will be cut orthogonally by a system of surfaces, the length of the path measured from the original source to any of

these surfaces being the same for all the rays. The proof he gives of this theorem is so simple that it now seems almost axiomatic; but it is curious that Malus, who had made the laws of Light his special study, though he suspected that the theorem ought to hold, yet found himself unable to establish it.

Hamilton, now considering the length of the path to any point as a function of the coordinates of that point, and denoting this function by V , proves that V satisfies a simple partial differential equation of the first order and proceeds to show the important part the function V plays in the theory.

He goes on to prove generally that if we are dealing, not with right lines, that is, with paths, for which as between any two points $\int ds$ is a minimum,

but with curved paths for which $\int \mu ds$ is a minimum (where μ is a function of the coordinates), and a system of such paths be drawn through a given point, O , the system of surfaces $V = \text{const.}$ will still cut all the paths at right angles. If we adopt the emission theory of Light, and we take for μ the velocity of Light, V becomes 'the Action,' and the minimum property which the paths satisfy is the principle of 'Least Action.' If, on the other hand, we adopt the undulatory theory, and we take for μ the reciprocal of the velocity, the minimum property becomes the principle of 'Least Time.' Thus Hamilton shows that, by altering the significance of μ , his method applies to either theory.

Introducing the further conception that μ depends, not only on the coordinates of the point, but also on the direction-angles of the ray, he is able to apply his reasoning to rays passing through a crystal. He gives by his method a new and interesting proof of the equation of Fresnel's wave-surface, and arrives at the conclusion, hitherto unnoticed by mathematicians, that this wave-surface possesses four conical cusps and also four special tangent planes, each of which touches the surface, not in one point only, but in an infinite system of points lying in a circle. The physical significance of these theorems is what is known as Conical Refraction.

Having drawn this inference from his mathematical analysis, Hamilton wrote to his friend Dr. Lloyd and asked him to verify it by actual observation, and accordingly Hamilton's paper in the 'Transactions' of the Academy is accompanied by another from Lloyd describing the beautiful arrangements by which he had succeeded in verifying this remarkable phenomenon in both its varieties.

This striking instance of scientific prediction naturally made a great sensation at the time, appealing, as it did, to a much larger public than the few select mathematicians who were capable of mastering the elaborate treatise on 'Systems of Rays.'

The experimental skill that was required to obtain these results may be realised from the circumstance that as I have been told the French physicists found themselves unable to repeat the experiment till Lloyd himself went over to Paris with his instruments and showed them the way.

Hamilton was so well satisfied with the success of his new method in dealing with the problems presented by the propagation of Light that full of enthusiasm he proceeded to apply a generalised form of the same method in the investigations of the motion of any material system, and a paper of his was read before the Royal Society in 1834 with the following title: 'On a general method in Dynamics by which the Study of the Motions of all free systems of attracting or repelling points is reduced to the Search and Differentiation of one Central Relation, or Characteristic Function.'

To show the importance attached by the most competent judges to Hamilton's work in this field of Theoretical Dynamics, we cannot do better than quote the words of his great German contemporary Jacobi, who afterwards himself added to the new theory such valuable developments.

Jacobi writes as follows:—'If a free system of material points is acted on by no other forces than such as arise from their mutual attraction or repulsion, the differential equations of their motion can be represented in a simple manner by means of the partial differential coefficients of a single function of the coordinates. Lagrange, who first made this important observation, at the same time

showed that this form of the differential equations possesses great importance for Analytical Mechanics. The marked attention, therefore, of mathematicians could not fail to be aroused when Herr Hamilton, Professor of Astronomy in Dublin, indicated in the "Philosophical Transactions" that in the Mechanical problem referred to all the integral equations of motion might be represented in just as simple a manner by means of the Partial Differential Coefficients of a single function. This is undoubtedly the most considerable extension which Analytical Mechanics has received since Lagrange.'

It will be of interest to the Section to recall the fact that Hamilton and Jacobi met each other for the first and I fancy the only time at a Meeting of this Association, held in Manchester in 1842, at which meeting Jacobi, addressing this Section, called Hamilton 'le Lagrange de votre pays.'

The last third of Hamilton's life was mainly devoted to the development of his Quaternions Calculus. As early as 1828 his Class Fellow, J. T. Graves, who had been working at the theory of the use of imaginary quantities in Mathematics, wrote an essay on Imaginary Logarithms which he wished to get printed by the Royal Society. There appears to have been some hesitation amongst the leading mathematicians in the Society, notably, Herschel and Peacock, about publishing Graves' paper, as they felt dubious about the accuracy of his reasoning. Hamilton heard of this and wrote earnestly to Herschel defending his friend's conclusions, and it seems as if his generous desire to help his friend first set his own mind working in this direction.

For years his busy brain in the midst of all his other work kept pondering over this question of the interpretation of the imaginary, and he has left us in his 'Lectures on Quaternions' an elaborate account of the many systems he devised.

It was only in 1843, fifteen years later, that he first invented the celebrated laws of combination of the quadrantal versors of the Quaternion Calculus. Argand, Cauchy, and others had proposed for space of two dimensions the theory now known as that of the Complex Variable. For them $x + iy$ meant the vector to the point xy , and the product of two vectors meant a new vector of the same form, the only law required being that i operating upon i was always equivalent to -1 .

Many attempts had been made to form on similar lines a Calculus which should apply to space of three dimensions; but so far all such attempts had proved unsuccessful, the laws by which the new symbols acted upon one another leading to results hopelessly involved. It was here that Hamilton's wonderful faculty of scientific imagination came into play. He proposed that a vector should be denoted by $ix + jy + kz$. As in the theory of the complex variable in two dimensions the result of any number of successive operations always preserved the fundamental type $a + ib$, so it was desirable that the result of the successive operations of his vectors should issue in an equally simple fundamental type. This end he found he could attain if he discarded the commutative principle which hitherto had barred his own progress and that of others, yet preserving the distributive and associative principles, and finally one happy evening he arrived at the beautifully simple laws by which the symbols of this Calculus act upon each other; that not only $i^2 = j^2 = k^2 = -1$, but also that $ij = -ji = k$, $jk = -kj = i$, $ki = -ik = j$.

Though it was thus—as the product, that is, of two vectors—that the Quaternion first presented itself to Hamilton, he of course saw that it immediately followed that it might be regarded as the ratio of two vectors, in other words the operation which turned one vector into another. In fact in the more synthetic exposition which is contained in 'The Elements' he makes this latter the starting definition of the Quaternion.

It is noteworthy that this the more complete and systematic presentation of the subject by its illustrious author may be said to owe its origin to the keen interest my predecessor, Professor Tait, took in the new Calculus, of which, as you know, he ever afterwards remained the most ardent champion. This interest led him to seek from Dr. Andrews an introduction to Hamilton, and the encouragement came to Hamilton at an opportune moment, for he wrote:—

'It was useful to me to have my attention recalled to the whole subject of the Quaternions, which I had been almost trying to forget, partly under the impression that nobody cared or would soon care about them. The result seems likely to be that I shall go on to write some such "Manual," but necessarily a very short one.'

The 'Manual' thus foreshadowed became the voluminous treatise 'The Elements of Quaternions.'

Those interested in the future of Quaternions will have welcomed the new edition of this work brought out by the present occupant of Hamilton's Chair, Professor Charles Joly, who has himself also added some remarkable developments to one branch of the subject, the Theory of the Linear Vector Equation.

Hamilton's Quaternions may be viewed in two lights, as a development of the logic and philosophy of symbols in their relation to space of three dimensions and also as an instrument of research in Geometry and Physics. In the former aspect the Quaternions will ever remain a splendid monument of the imagination and genius of its inventor. In the latter point of view, that is, when we come to regard it as a working calculus, it would be premature as yet to fix the place it will ultimately occupy.

A few years after Hamilton had entered upon his scientific career James MacCullagh won his Fellowship in Trinity College. After an interval of three years he was appointed Professor of Mathematics, and eight years later succeeded Dr. Lloyd in the Chair of Natural Philosophy. It would be difficult to overestimate the stimulating effect of MacCullagh's lectures as Professor upon the Mathematical School. Many of those whose names stand out afterwards—such men as Jellett, Michael and William Roberts, Haughton, Townsend, and our present honoured Provost—were MacCullagh's pupils. To the present day the tradition still lingers in Trinity College of the impression MacCullagh made upon the minds of those with whom he came in contact.

When, passing from his influence as a teacher, we come to examine his own original work we find that this naturally divides itself into two departments, the first embracing Geometry and that part of the field of Mathematical Physics which most resembles Geometry, that in which the fundamental principles are entirely agreed upon; the second his work in Physical Optics, where he has to imagine new principles which, mathematically developed, should correlate the empirical laws hitherto obtained and be capable of verification by experiment.

Of the first class we have his studies in 'Surfaces of the Second Degree.' The most striking result he here obtained was the discovery of the modular generation of the quadric, thus extending to surfaces the focus-and-directrix property of the conic in plano. We are also indebted to him for some very elegant theorems in the theory of confocal quadrics, a subject to which he devoted much attention. He likewise gave a course of lectures containing a masterly discussion and geometrical presentment of the motion of a rigid body round a fixed point not acted on by external forces.

At the very outset of his career as an original author he seems to have been attracted by the theory of Light. To understand the ardour with which MacCullagh and his contemporaries devoted their mathematical powers to Physical Optics, we must endeavour to recall the circumstances of the time. The celebrated memoirs of Fresnel had recently appeared. In these he had proved, following Young, that the ethereal vibrations which constitute Light must be in the plane of the wave-front; that a beam of polarised light was simply a system of parallel waves in which these transverse vibrations were all in one direction. He had applied the theory of the ellipsoid to prove that there were three directions in a crystal in which the restitution-force coincided with the direction of the vibrations; that in the plane of every wave there are two directions along which, if a particle vibrate, the component of the restitution-force resolved in the plane of the wave will be along the direction of displacement. He had also from these principles deduced the equation of his famous wave-surface.

How much the work of Fresnel filled the imagination of scientific men in

those days may be seen from the enthusiastic language which the sober-minded Dr. Humphrey Lloyd allows himself to use about him in his valuable report on Physical Optics, which he wrote for this Association in 1884.

In passing I would say that the name of Fresnel reminds us of the loss Science, and especially this Section, has sustained since we last met in the death of that illustrious French physicist who devoted his life with such ardour and success to the same field of research—Alfred Cornu. Those of us who had the privilege of being present will recall with a sad pleasure the beautiful address he gave us in Cambridge on the Wave Theory of Light on the occasion of Sir George Stokes' jubilee.

Fresnel in his analysis had assumed that when the molecules of the ether are disturbed by the passage of a wave the force of restitution acting upon a molecule depends upon that molecule's absolute displacement. Cauchy and Neumann and, in England, Green, improved on Fresnel's reasoning, making this force depend not on the absolute but on the relative displacement; all these physicists, however, worked on the lines of endeavouring to form an explanation of the propagation of the waves of Light, by treating them as the waves in an elastic medium, akin in its properties to a solid medium in which the stresses depend on the deformation of the elements.

MacCullagh agreed with these others in making the forces of restitution depend on the relative displacements as expressed through a certain function V , which represented the potential energy of the medium. In the further development of the theory he, however, diverges from them and adopts a line of his own. Struck by the significance of the fact, to which he seems to have been the first to direct attention, that the vector whose components are

$$\frac{1}{2}\left(\frac{dv}{dz}-\frac{dw}{dy}\right), \frac{1}{2}\left(\frac{dw}{dx}-\frac{du}{dz}\right), \frac{1}{2}\left(\frac{du}{dy}-\frac{dv}{dx}\right)$$

which we now, of course, know as the vector of molecular rotational displacement, was, so to speak, a physical vector, independent of the choice of our axes of coordinates, he was led to the idea of choosing for the form of V that of a homogeneous quadric in these three components. It must be admitted that the reasoning by which he attempts to prove the necessity of this assumption is eminently unsatisfactory, and that the assumption itself lay open to an apparently fatal objection urged later by Stokes, that of neglecting to secure the equilibrium of the element of the medium quoad moments.

Having, however, adopted this form of V , MacCullagh proceeds (making the assumption that while the elasticity of the medium varied the density was everywhere the same), by processes of remarkable elegance and simplicity, to develop the laws of wave propagation in a crystal, thus verifying the wave-surface of Fresnel, while at the same time he found himself able to satisfy completely the requirements at the limits. He could also point to experience, *e.g.*, the experiments of Brewster and Seebeck, as justifying the simple and beautiful laws which he had succeeded in obtaining.

Nevertheless the force of Stokes' objection was felt to be so strong that one who reviewed the subject, say thirty years ago, would have regarded MacCullagh's work in Optics as presenting indeed opportunities for beautiful mathematical developments, but lacking sound physical basis.

The publication, however, of the epoch-making treatise of Maxwell on Electricity and Magnetism entirely changed the aspect of the question, and in particular threw a new light on MacCullagh's assumption. FitzGerald, in 1879, pointed out that the Potential Energy, which in Maxwell's theory was equivalent to the electro-static energy, really was a quadratic function of three variables, which answered to the components of MacCullagh's molecular rotation, and accordingly led to the same differential equations of the motion as MacCullagh had deduced.

Subsequently Larmor, in his remarkable investigation of the Dynamical Theory of the Electric and Luminiferous Ether, deliberately reconsiders MacCullagh's position, finds in fact in his equations the starting point of his own theory. He points

out the real significance of MacCullagh's function V ; that it corresponds to a stress-strain system, but one of a very novel type; one in which the stresses depend entirely on the rotational displacements of the molecules, and are otherwise absolutely unaffected by the ordinary deformation-strains. He further shows that the difficulty under which MacCullagh's theory laboured, that it did not provide for the rotatory equilibrium of the element, could be removed if we allowed ourselves to assume the existence of a hidden torque acting on each element.

As I understand the advocates of this theory, they maintain that an important step has been made, even though in the present state of our knowledge we may not be able to account for the existence of this hidden torque. They point out, however, that such a torque is at least not inconceivable, whether its explanation be sought in concealed kinetic phenomena, as in Lord Kelvin's material gyrostatically constituted medium, or in quasi-magnetic forces supposed to reside in the ethereal elements.

Should this theory of a rotationally elastic ether obtain final acceptance, it will of course be a matter of congratulation to MacCullagh's countrymen to find that his labours, in this, perhaps the most important field of his researches, have not been thrown away; that they represent no mere play of elegant mathematical analysis, but a real step in the progress of physical science.

A few years after MacCullagh, two other well-known men, whose names for half a century were associated with the Mathematical School in Dublin, were selected Fellows—Andrew Searle Hart, afterwards Sir Andrew Hart, and Charles Graves, subsequently Bishop of Limerick. They won their Fellowships in two successive years, and both lived to an advanced age.

Hart had a great reputation as a geometer. His examination papers were specially noted for the number of original problems they contained. As specimens of his work we may instance the following. Extending Feuerbach's theorem for the nine-point circle, Hart showed that the circles which touch three given circles can be distributed into sets of four all touched by the same circle. He also showed that Poncelet's beautiful porism for coaxial circles in plano held for the surface of an ellipsoid, if we replace the rectilinear polygons by geodetic polygons and the coaxial circles by lines of curvature.

Graves became Professor of Mathematics on MacCullagh's resigning the Chair in 1843. He was largely influenced by the writings of Chasles, of whose two memoirs on Cones and Spherical Conics he published a translation. In this were incorporated valuable original additions of his own, amongst others the remarkable theorem that if two spherical ellipses are confocal the sum of the tangents drawn to the inner from any point of the outer exceeds the intercepted arc between the points of contact by a constant length, a theorem which of course includes the corresponding proposition for confocals in plano. Graves was one of the first to apply the method of the Separation of Symbols to Differential Equations, and gave an elegant demonstration by this method of Jacobi's celebrated test for distinguishing between maxima and minima in the Calculus of Variations.

On the death of MacCullagh it was determined to strengthen the Natural Philosophy department by the establishment of a second Professorship in that subject, and Jellett, one of the ablest of MacCullagh's pupils, was appointed to the new Chair.

His first published work was his 'Calculus of Variations,' which at the time it was written constituted the only systematic English treatise on the subject. It is marked by that peculiar acuteness and power of fastening on essential points, whether for criticism or exposition, which was the author's leading characteristic. Apart from the excellent account he gives of the researches of Continental mathematicians, I would notice especially his most interesting chapters on the conditions of integrability and many valuable geometrical theorems on surfaces hence resulting. In discussing his more properly original work we may arrange it in three divisions: 1st, his papers on Elasticity; 2nd, that on the properties of Inextensible Surfaces; 3rd, those on the application of polarised light to the new subject of Chemical Equilibrium.

In taking up the problem of an elastic medium and the propagation of waves

in such medium, Jellett follows the example of MacCullagh, who had made this subject one of special interest to the Dublin school. In these memoirs he draws attention to a remarkable difference in the mode of regarding the molecular constitution of the medium, a difference corresponding to what is now known as the distinction between the Rari-constant and Multi-constant theories. We may, Jellett points out, regard the action between two molecules as only conditioned by the relative position of these molecules, or as dependent also on the position of the neighbouring molecules. The first is termed by Jellett the hypothesis of independent action, and this he shows to lie at the basis of Cauchy's theory, whereas the theory of Green, the English elastician, essentially involves the second hypothesis which Jellett calls 'modified action.' He established in the same papers the important theorem that if a Work function exists the three directions of vibration, corresponding to a plane-wave, are rectangular, and *vice versa*.

In his memoir on Inextensible Surfaces various interesting questions are discussed. He proves that in the case of a synclastic surface if a closed curve on the surface be held fixed, the entire surface will be immovable; that on the other hand on an anticlastic surface it is possible to draw a curve which may be held fixed without involving the immovability of the surface, the conditions being that the curve will be that formed by the successive elements of the inflexional tangents. The mathematical theory of such curves had been already studied, but Jellett seems to have been the first to signalise their importance in the theory of deformation, and, on account of the property referred to, he proposed to call them Curves of Flexure. It is interesting to remark that Maxwell was attracted by the same subject of Inextensible Surfaces, and in one of his earliest papers confirms by an entirely different method several of Jellett's conclusions.

At the close of Jellett's paper a remarkable proposition is laid down, apparently for the first time, that a closed oval surface cannot be inextensibly deformed; in other words, that if such a surface be perfectly inextensible it is also perfectly rigid. I think we must admit that the proof of this striking theorem offered by Jellett is by no means satisfactory. Subsequent attempts by others to establish this proposition can hardly be said to be more successful. But the fact that it can be rigorously proved true for a sphere or more generally for any ellipsoid seems to indicate that we have here to do with a real and important theorem, but one which needs, as is so often the case, to have the limits of its application more clearly defined.

Many experimental physicists will know Jellett best by the beautiful and delicate instrument he invented, 'The Double-plane Analyser,' an instrument which he devised in order to secure the more exact determination of the rotation of the plane of polarisation than could be obtained by the polariscopes hitherto in use. Jellett was actuated here by the consideration that he saw in this phenomenon of the rotation of the plane of polarisation a means of attacking the interesting problem of chemical equilibrium. Chemical equilibrium he defines thus: 'Two or more substances may be said to be in chemical equilibrium, if they can be brought into chemical presence of each other (as in a solution) without the formation of any new compound or change in the amount of any of the former compounds which have thus been brought together.' In a mixed solution of sundry bases and acids where all the possible salts are soluble, what are the proportions in which the acids are distributed amongst the bases? Such was Jellett's question, and in answering it he arrives by a remarkable train of quasi-mathematical reasoning at certain laws governing this distribution, and proceeds to establish the truth of these laws by observation with his new polariscope.

He also discusses in the same papers two alternative theories which we can hold of chemical combination, the 'statical' and the 'dynamical,' and shows from the consideration of the number of equations which subsist that the 'dynamical theory' is alone admissible.

When the Association met in Belfast twenty-eight years ago Dr. Jellett occupied this Chair, and at the close of his Address, in which he took for his subject certain fresh applications of Mathematical Analysis to Physical Science, he touched upon these very researches in which he was at the time engaged.

All old Trinity men would think this enumeration incomplete if it did not refer to the wonderfully active animating presence of Samuel Haughton. He also directed his energies in the first instance to the subject of Elasticity, on which he wrote several important memoirs, endeavouring to formulate a system of laws by which he might be able to explain the propagation of Light. But apparently discouraged by the extreme difficulty of the problem his versatile brain turned soon to quite other branches of science—to Physical Geology, then to Physiology and Medical Science, and in fact in his later work he passes out of the cognisance of Section A.

Of the pure mathematicians trained under MacCullagh two of the most eminent were the twin brothers Michael and William Roberts. Strikingly alike in their personal appearance they were in my student days two of the best known figures in the Courts of Trinity.

In his geometrical work Michael Roberts pursued the fruitful lines of research started by Chasles and followed up by MacCullagh in the study of quadric surfaces, and it fell to his lot to discover some most remarkable theorems on the relations of the geodetics on the surface to the lines of curvature; theorems indeed to which the author would have been justified in applying words which Gauss used of a great theorem of his own:

‘Theoremata quæ ni fallimur ad elegantissima referenda esse videntur.’

Joachimsthal had shown that the first integral of the equation of the geodetics on an ellipsoid could be thrown into the well-known form $PD = \text{constant}$. Michael Roberts now showed that the geodetics, which issue in all directions from an umbilic, pass through the opposite umbilic where they meet again by paths of equal length; that the lines of curvature considered with respect to two interior umbilics possess properties closely analogous to those of the plane conic with respect to its foci; that if such umbilics A and B be joined by geodetics to any point P on a given line of curvature they make equal angles with such line, and consequently that as P moves along the line of curvature, either $PA + PB$ or $PA - PB$ remains constant, so that if the ends of a string be fastened at the two umbilics and a style move over the surface of the ellipsoid, keeping the string stretched, the style will describe a line of curvature. Another remarkable analogue he proved was the following: that as in a plane conic if a point P on the curve be joined to the foci A and B,

$$\begin{aligned} \tan \frac{1}{2}(PAB) \tan \frac{1}{2}(PBA) &= \text{const.} \\ \text{or } \tan \frac{1}{2}(PAB) / \tan \frac{1}{2}(PBA) &= \text{const.} \end{aligned}$$

so precisely the same relation holds for a line of curvature on the quadric, replacing the foci by the umbilics and the right lines by geodetics.

Sir Andrew Hart made a valuable contribution to the subject by investigating the relation between the angles which an umbilicar geodetic makes with the principal plane when it leaves the umbilic and when it returns to it again after going the circuit of the surface. He proved that if ω and ω' be these angles, $\tan \frac{1}{2}\omega'$ can be expressed by means of complete elliptic integrals independent of ω . This is interesting, as it shows that such a geodetic is not a finite closed curve, but that it crosses itself over and over again at the umbilics, the successive values of $\tan \frac{1}{2}\omega$ forming a geometric series.

To Michael Roberts is also due much important work in the department of pure analysis—notably, in modern Algebra his method of deriving Covariants, and the investigation of their relations by means of their sources, and in the theory of Abelian integrals his construction (following the method of Jacobi) of a Trigonometry of the hyperelliptic functions.

His brother William Roberts is perhaps best known for some of the investigations he carried out by means of elliptic coordinates. For example, he applied

them to Fresnel's wave-surface, and showed that the two sheets of the surface can be expressed in the simple forms

$$\lambda^2 + \nu^2 = a^2 + b^2 - c^2 \quad \text{and} \quad \lambda^2 + \mu^2 = a^2 + b^2 - c^2.$$

By following the same method he succeeded also in adding an interesting new triple system of orthogonal surfaces to those already known.

Richard Townsend was another of the Fellows of Trinity of MacCullagh's school. He was known to us in College in my day as the great expositor of the new geometry of Anharmonics and Involution. He wrote many valuable original papers, but it was as a lecturer he was most remarkable. I never met a teacher so enthusiastic nor one who seemed to enjoy teaching more thoroughly.

He inspired his pupils with much of his own ardour, and it is greatly owing to Townsend's influence that the old name Trinity had for the study of Geometry was so well kept up in his day.

He published in the latter part of his life an extensive treatise on Modern Geometry, which did good service in presenting the subject in the light of an organised system and not as a collection of isolated problems.

In this connection I must not omit to mention one of our most original Irish geometers of recent days, Dr. John Casey. Where Casey learnt his Mathematics is indeed a marvel. Up to middle life he was engaged in the engrossing labour of a schoolmaster in Kilkenny under the National Board of Education. It was not till he was nearly forty that by the advice of Townsend, to whom he used to send up some of his ingenious geometrical solutions, he moved up to Dublin and entered Trinity College. Of his original papers his best known are those on Bicircular Quartics and Cycloides.

In elementary Geometry we owe to him a very elegant extension of Ptolemy's famous theorem that for four points, A, B, C, D , on a circle $AC \cdot BD = AB \cdot CD + AD \cdot BC$. Casey shows that the same equation is true if we replace the four points by four circles touching a common circle and the lines joining the points by the common tangents to the circles. He acquired so high a repute both as a teacher and as a writer that he was offered and accepted the post of Professor of Mathematics in the Catholic University.

It is not yet two years since George FitzGerald was taken from us. The many loving tributes to his memory which appeared in the scientific journals after his death reveal to us how deep and widespread his loss was felt to be, but it is in Ireland this loss is most serious. As long as he lived and worked, our country could claim to own one of the foremost members of that select band who are endeavouring to wrest from Nature her inmost secrets.

You know how sedulous an attendant he was of the Meetings of this Section, and Trinity College never sent you a representative of whom she had more reason to be proud, for he has done more than any of her sons for many years to maintain the reputation of her scientific school. This he has brought about, not by his writings only, able and original as these were, but also by the encouragement and stimulus he gave the younger men he gathered round him, and the self-forgetful readiness with which he gave all the help he could to those who in any measure shared his own genuine love for science.

You will all rejoice that we are now in possession of a volume containing a complete collection of FitzGerald's scientific papers. I am sure he himself could not have wished for a better chronicler of his life and labour than his intimate friend Dr. Larmor, more especially as Dr. Larmor's own far-reaching speculations on the great mystery of the Ether qualify him in a very peculiar manner to appreciate the work of his fellow-physicist. The admirable analysis of that work in the opening pages of this volume renders any further account of it on my part completely unnecessary.

A few months before FitzGerald's death there passed away one of his most distinguished pupils, Thomas Preston. Though cut off so young he had already done much work, and of a quality which raised high expectations of his future. His treatises on Light and on Heat are to be noted, not merely for the excellent account they give of the recent additions to the subjects treated, but for the

thoughtful and philosophic spirit in which the whole is presented. It was, however, his experimental researches which most excited attention, more particularly those on the action on Light of a strong electro-magnetic field and the fine experiments in which he extended beyond any observations hitherto made the analysis of the Zeeman effect.

Of two others I have yet to speak, and these were emphatically representatives of this city and of the College in whose Halls we are meeting to-day—Thomas Andrews and James Thomson. It would be difficult to describe adequately all the phases of so manifold an activity as that of Dr. Andrews. As one long associated with him as a colleague I would bear testimony to one side of his life-work—the potent influence he exercised in this College in its earlier years as a skilful pilot guiding the ship till it was well out of port. His high ideal of the function it should discharge in the education of the country and the practical zeal and ability which he ever brought to bear on the administration of our affairs contributed in no small measure to place the College in the assured position it occupies to-day.

On his great physical and chemical investigations it is happily the less necessary for me to touch, as they have been so fully brought before you by our President in his opening Address; and as regards the most important of these researches, those on the continuity of the Liquid and Gaseous states, no one assuredly could have more fitly expounded them than one who has himself pressed forward with such splendid success in the paths which Andrews opened up.

I have always considered that Andrews, through the long course of these later researches, was most fortunate in having near at hand such a friend as James Thomson; not that he was a collaborator—for Andrews did all this work unaided—but that Thomson gave him throughout that best of all encouragement which consists in enlightened appreciation of the importance of the results he was obtaining and of their inner meaning and significance.

Of Thomson himself what shall I say? Of all the scientific men I have come across he perhaps most fulfilled the idea of a philosopher, his ever-working brain ever seeking out causes, ever pondering on the why and the wherefore of the unexplained.

One of his earliest investigations is perhaps the best known, that in which, basing his reasoning on Carnot's principle, he demonstrates the effect of pressure in lowering the freezing-point of water, and in which he gave at the same time a numerical estimate of this effect.

This discovery was of great practical import, for, small as the effect was, it enabled him to explain fully the rationale of the plasticity of ice.

Forbes had already shown that the motion of glaciers depended upon a plastic or viscous quality in the ice. It remained for Thomson, by the aid of his newly discovered principle, to go a step further and account for this plasticity.

It is interesting to note that the questions which led to some of his most valuable investigations seem to have been started by the filial task he took upon himself of re-editing his father's educational text-books. It was, for example, the revision of a chapter in his father's Geography which I believe led him to examine more thoroughly into Hadley's theory of the Trade winds, and to make the following important addition to that theory. He showed that while in the tropical latitudes, say of our northern hemisphere, two currents would satisfy all the conditions, *i.e.*, the Trade wind blowing from N.E. to S.W. in the lower regions of the atmosphere, and the return current in the upper regions, on the other hand that in the temperate latitudes there must be three currents at different elevations; that the uppermost and the lowest of these have a movement towards the pole, but in the middle regions of the atmosphere between these there must be a large return current from the Pole, and that the prevailing motions of all three currents would be from west to east.

Thomson was particularly successful in his treatment of this and other questions of fluid motion. He was not familiar with the technique of the higher mathematics, and on this very account was not tempted, as so many mathematical experts are, to assume impossible conditions in order to bring the problems within

reach of their algebraic analysis; but for all that his mind was eminently of a mathematical cast. He is never vague or loose in his reasoning, and he had a wonderfully tenacious grasp of physical principles. The result was that he has succeeded in finding out the key to some of the most curious phenomena in the motions of fluids.

I may give as a typical instance of his line of reasoning his beautiful explanation of the action of the water of a river flowing round a bend. He saw clearly that from true dynamical principles the flow of the water must be most rapid near the inner bank, and the question which presented itself to his mind was why then the inner bank was not worn away. The answer he showed to consist in the friction of the bed checking the velocity of the lowest stratum of the water. The effect of this he proves to be that an under-current is produced in this stratum across the bed of the river from the outer towards the inner bank, a current which does two things: it carries sand and detritus and deposits them on the inner bank; and, since the water in this current has to rise vertically to the surface when it reaches this bank, it thus protects it from the scour.

In a review of Thomson's work we should emphasise his constant endeavour, whether in Mathematics or Physics, to attain clear conceptions of fundamental principles. This showed itself in the various innovations in nomenclature he introduced. Many of the new words he coined, 'radian,' 'numeric,' 'torque,' 'interface,' 'clinure,' 'posure,' &c., are great helps both in thinking and teaching.

The same determination at any cost of hard thinking to arrive at clearness in regard to fundamental principles is strikingly evidenced by one of his later papers, that on the 'Law of Inertia and the Principle of Chronometry,' which is a most searching discussion of the true significance of Newton's first and second laws of motion.

I must now close this review. I shall be glad if I have succeeded, however imperfectly, in giving you some impression of our Irish schools of Mathematics and Physics, of the workers and of the sources from which they drew their inspiration. There surely never was a time when the problems presented to the mathematician by Physical Science were more interesting; never a time when Science for its onward progress stood more in need of those gifted ones who combine clearness of thought with imagination and hopeful courage. Let us hope that amongst these in this new century, others of our countrymen may be found not unworthy to have their names inscribed in the roll which contains those of Hamilton and MacCullagh, of Andrews and Thomson.

British Association for the Advancement of Science.

BELFAST, 1902.

ADDRESS TO THE CHEMICAL SECTION

BY

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PRESIDENT OF THE SECTION.

The Atomic Theory without Hypothesis.

IN opening the Chemical Section of the British Association in this city and in the halls of the Queen's College my first words must be those of reverence for the memory of Thomas Andrews, for so many years the Professor of Chemistry in this College, whose investigations into the properties of gases—above all, those which resulted in the recognition and determination of the critical pressure and temperature of carbonic anhydride—have become a part of the foundation of the Kinetic Theory of Gases. At the Meeting of the British Association here in 1852 Andrews was President of this Section, and again at the Meeting in Edinburgh in 1871.

Since the Meeting last year another distinguished chemist, formerly professor in one of the Queen's Colleges, Maxwell Simpson, has also passed away. He, too, acted as President of this Section, namely, at the Meeting in Dublin in 1878. The work by which Simpson's name will ever be recalled is more especially that upon the synthesis of polybasic organic acids.

One other name must not be left unmentioned in this Address: it is that of a long-time Fellow of the Chemical Society who has been intimately connected with the British Association—I mean that of George Griffith, the genial and most effective Assistant General Secretary of the Association for so many years, who died four months ago. He had visited Belfast in the spring and made the preliminary arrangements with the Local Committee for this Meeting. He joined the Chemical Society in 1859—just one year before I did—and remained a Fellow until his death.

It is now almost a century ago since John Dalton made known to the world his theory of the nature of chemical combination by the publication of a table of atomic weights. He had been occupying himself for some years with the study of the physical properties and atomic constitution of gases before he was led to extend the notion of the atom to chemical phenomena, and thus to form that conception which was to become celebrated as the atomic theory. In his laboratory note-books, preserved from 1802 onwards, the publication and analysis of which we owe to Sir Henry Roscoe and Dr. Harden, no reference is made to the theory till 1803, but we may well believe with Henry that it was already in Dalton's mind just a hundred years ago. But however that may have been, it seems fitting in a year so closely approaching the centennial of its publication as the present that the occupier of this Chair should address his audience on a subject of such general interest and importance as the atomic theory, if indeed there remains anything to be said on a subject which has so long and so fully engaged attention.

I dare not assert that I have found anything actually novel to bring before you with regard to the atomic theory, but I may say that there has certainly long seemed to me to exist the need to treat it as being a true theory instead of as an hypothesis, and to teach it and discuss it accordingly.

In thus setting forth what appears to me to be the proper form of the atomic theory, I shall have, at the risk of overtaking your patience, to restate and examine most of the fundamental and familiar principles of our science in order to illustrate and justify the view I take. Not only this, but in order as directly and briefly as possible to meet the objection that whatever the atomic theory may be it cannot be introduced to the student of chemical philosophy in another form than that now in use, I shall sometimes have to adopt, in order to show what can be done, a didactic method which, in most other circumstances, would be quite inexcusable before so distinguished an assembly.

The atomic theory of chemistry stands unsurpassed for the way in which it has fulfilled the purpose of every great theory, that of giving intellectual mastery of the phenomena of which it treats. But in the form in which it was enunciated, and still is universally expressed and accepted, it has the defect of resting upon a metaphysical basis, namely, upon the ancient hypothesis that bodies are not continuous in texture, but consist of discrete, ultra-minute particles whose properties, if known, would account for those of the bodies themselves. Hence it has happened that, despite the light it throws upon the relations of chemical phenomena and the simple means it affords of expressing these relations, this theory has always been regarded with misgiving, and failed to achieve that explicit recognition which its abounding merit calls for. Indeed, the desire has been expressed to see the time when something on a more solid foundation shall have taken its place.

Now, it is not my intention to discuss the merits or demerits of the atomic hypothesis, which can indeed no longer be treated as a merely metaphysical speculation. What I would do to-day is to impress upon you that, in spite of all that has been said and written about the atomic hypothesis in connection with chemistry, the atomic theory propounded by Dalton and adopted, implicitly at least, by all chemists, is not founded upon the metaphysical conception of material discontinuity, and is not explained or illuminated by it. For if that should be the case there will no longer exist any grounds for hesitation in accepting the theory quite explicitly, and then the anomalous condition of things will be removed of a theory being in universal use without its truth being freely and openly admitted. For the sake of clearness, it is convenient to restrict the term 'atomic hypothesis' to the old metaphysical view of the discontinuity of matter whilst applying the term 'atomic theory' to the current elaborated form of the Daltonian theory; this distinction is adhered to in the present Address.

In the peroration to his admirable discourse upon atomic weights or masses delivered before the Chemical Society in 1892 as the Stas Memorial Lecture Professor Mallet, F.R.S., said: 'By the chemist at his balance the arm of reason is directed into those regions of almost inconceivable minuteness, which lie as far beyond the reach of the most powerful microscope as that carries us beyond the reach of the naked eye, quite as impressively as that same arm is stretched forth by the astronomer at his divided circle to reach and to weigh the mighty planets that shine in the remotest regions of our solar system.' On two occasions I have heard the same comparison between the chemist and the astronomer made by Lord Kelvin when he was in the company of chemists; and undoubtedly both these high authorities have only then expressed the general view as to the nature of the domain of the chemist. Yet I venture to question whether there is anything in the ways and work of the chemist to support such a view and give point to Mallet and Kelvin's comparison. If, indeed, chemistry is a science which rests upon the atomic hypothesis and, therefore, would cease to exist in the form into which it has developed, should matter prove to be continuous and not discrete, nothing can be said against the view that it is a science of the minute. But I am sure there can be no one ready to maintain that, if the hypothesis of the atomic constitution of substances were an unfounded one, the atomic theory would have been a discovery of no great importance; and Dalton himself, instead of being the founder

of the chemistry of to-day, have been little more than the discoverer of the law of multiple proportions. If that cannot be maintained, what, then, becomes of this conception of chemistry as dealing with the minute? So far as comparison can be made between the operations of the astronomer and the chemist, it is the former and not the latter who, as a matter of fact, deals with the almost infinitely minute. For if, indeed, the chemist often works upon comparatively small amounts of substances, and, consequently, with very sensitive balances, that is, as we all know, only for reasons of economy of time, materials, and apparatus; otherwise he works on the largest possible scale, with the object of attaining to the highest degree of accuracy and perfection. The astronomer, on the other hand, has, perforce, to deal with the smallest visible things in nature, the nearest approach there is to geometrical points, those fixed points of light in the heavens which are only known through scientific investigation to be other than what they seem to be. It is, therefore, only as interpreted by the atomic hypothesis that chemistry can be said to deal with the minute.

When the atomic theory is expounded in the usual way it is commonly and correctly stated that, on the assumption that substances consist of minute indivisible particles having weights or masses bearing the ratios of the combining numbers assigned to them, the laws of chemical combination by weight necessarily follow, and are thereby explained. But then the converse is not true—that because chemical combination obeys the well-known laws, substances consist of discrete particles. Nor does the assumption of the truth of the atomic hypothesis afford any real explanation of the facts expressed by the laws of chemical combination, or more comprehensively by the atomic theory, when that theory is given in non-hypothetical terms. It is just as difficult to see why the atoms should possess the weights on chemical grounds assigned to them, as to see why substances interact in the proportions that they do; that they do so is, in either case, an ultimate fact, for which no explanation has presented itself. The atomic hypothesis masks this ignorance and deadens inquisitiveness. Notwithstanding all this, which is incontrovertible, it is certainly a common opinion that in chemistry we investigate the minute and intimate constitution of things.

But if, after all, chemistry does not deal with the minute, or, rather, if it has no concern with the magnitude of single bodies or their molecules; if the atomic hypothesis is not the foundation of, or necessary to, the atomic theory, then it is certainly most desirable and important that the theory of chemistry, which, with all its modern developments, I take to be indisputably the atomic theory of Dalton, should be held and expounded without any reference to the physical constitution of matter, in so far as that remains unknown. The opinion that chemical theory should be developed without reference to the atomic hypothesis has indeed all along been held by many eminent chemists; but then the dilemma appears to have presented itself to them, that either the atomic hypothesis must be granted, or the atomic theory must be dispensed with, since it falls with the hypothesis. That dilemma I do not recognise, and the practice of chemists shows beyond doubt that it is always ignored. Investigators use the theory, whether they admit it or not; teachers of the science find it indispensable to their task, however much they may deprecate, and rightly so, unreserved acceptance of the atomic hypothesis as true.

Refusing to commit themselves to belief in the hypothesis, chemists have thought from the first to escape the adoption of the atomic theory by putting Dalton's discovery into something like these words: Numbers, called proportional or combining numbers, can be assigned to the chemical elements—one to each—which will express all the ratios of the weights or masses in which substances interact and combine together. Perhaps the atomic theory is here successfully set aside by expressing what is an actuality as an unaccounted-for possibility. But then those who use any such mode of expressing the facts, without reference to the theory, never fail also to adopt the doctrine of equivalents, and thus, by this double act, implicitly give in their adherence to the theory.

Divested of all reference to the physical constitution of matter, the atomic theory is that the quantities of substances which interact in single chemical changes are equal to one another,—as truly equal in one way as equal masses are in another,—

and, therefore, that chemical interaction is a measure of quantity of unlike substances, distinct from and independent of dynamical or mass measurement.

Dalton, indeed, did not express himself in any such terms, his mind being fully possessed with the ancient and current belief upon which he framed his theory that substances are made up of minute, discrete particles. But it is clear enough that his theory was that of the existence of another order of equality between substances than that of weight. Up to his time, the weight or mass of every ultimate particle of any substance whatever appears to have been assumed to be the same, the atoms being alike in every way. That assumption is still made by many thinkers, chemists among them; we meet it, for example, in the different forms of the hypothesis that the elements are all, in some way, physically compounded of a universal and only true element, as in Prout's hypothesis. Dalton saw things differently, and recognised that, on the assumption of substances being constituted of particles which never subdivide, weight or mass cannot be the same for every such particle, except in the case of those of any one simple substance. Therefore, having given some numbers showing what he believed to be the respective weights of the atoms of several simple substances, taking that of hydrogen as of unit-weight, he proceeded at once to invent symbols for these atoms to indicate, not only their distinctness in kind, but above all things their indivisibility and their equality, properties which the use of their atomic numbers would have inadvertently concealed or even apparently denied, and could never have expressed or connoted.

It was only in this immediate invention and use of chemical symbols that Dalton's conception found clear expression; and again it is by the universal adoption of such symbols that chemists have shown their real acceptance of the atomic theory, even while displaying, not infrequently, their scepticism as to its truth. The replacement by Berzelius of Dalton's marked circles for atomic symbols by letters which should recall the names of the substances was in a way a great improvement, but it has had the serious consequence of causing chemical symbols to be usually first brought under notice merely as serviceable abbreviations for the names of the elements, and only then described as representing their atomic quantities. Now, evidently, what the character used as symbol shall be is, theoretically considered, but a petty detail; the vital point is what the character symbolises, and that is the atom. It does not symbolise the name; it only indicates that and recalls it. It may be said indeed to represent the atomic number, since it stands in place of it; but it is made to do so only in order that we may for the time forget this number and have in mind the integral character of the atom. It is not the 4006 parts of sodium hydroxide and 8097 parts of hydrobromic acid, or approximately twice as much of the latter as of the former; it is not these gravimetrically expressed interacting quantities that we are to think of when the formulæ NaOH and HBr are before us, as we too often strive to do; it is not these, from a chemical point of view, meaningless numbers of parts, but quantities which are equal in the sense of chemistry, that are expressed as such by these symbolic formulæ. The real purpose of chemical formulation is not to abbreviate or replace language, but to facilitate, if not ensure, abstraction from and non-contemplation of gravimetric numbers.

I have just passed from atomic symbols to the formulæ of molecules; but this was not without warrant. In the form in which I have enunciated the atomic theory, it relates to the chemical interaction of substances, whether compound or simple, and the equality of the quantities concerned is the equality of molecules, since these are the quantities of substances entering into or coming out from single chemical interactions. Were it not, therefore, for fear of confounding it with the mechanical theory of that name, the atomic theory should be called the molecular theory of chemistry. It might, indeed, have happened to be so called by its author, for Dalton has told us that he had in mind both atom and molecule as names for his chemically ultimate particles, and chose the former because it carried with it the notion of indivisibility. He extended also, as we do, the use of the term 'atom' to chemically compound substances, since their combining quantities are chemically indivisible.

Next, I would point out that in the atomic theory the notions of indivisibility and equality are inseparably involved. The indivisibility of atom and molecule is not absolute or ultimate, and Dalton distinctly guarded himself against being understood to claim for the atom more than chemical indivisibility, and chemists of to-day assert no more than this. This indivisibility being conditioned by the equality of molecules, the importance of emphasising it rests only upon the danger, when it is overlooked, of losing sight also of the chemical equality through the gravimetric inequality receiving numerical expression, and thereby conveying the notion of divisibility, though only gravimetrically. The idea of indivisibility in connection with the atom or molecule is intrinsically quite subordinate to that of equality; for equality, being unity or oneness brought into relation with itself, the conception of it carries with it and includes that of indivisibility. Any rational hypothesis as to substances consisting of ultimate particles will include the notion of their being indivisible particles; and the import of the hypothesis in chemical theory must lie, therefore, not in this indivisibility, but in the nature of the equality of the particles. By his atomic theory Dalton asserted that where the substances are different this equality is chemical instead of gravimetric.

Molecules are equal in the sense that they are quantities of their substances which are interdependent and coordinate in any and every single chemical change in which they take part together. It is a form of equality for which no close parallel can be found; but as to that it should be remembered that this equality relates to the phenomena of the transformations of substances into each other, which, though they form so large a part of the phenomena of the universe, are fundamentally distinct in nature from the rest of the behaviour of bodies throughout which the substance remains what it was. In some agreement with it there is that of mechanical pressures when these balance or neutralise each other, and therefore are opposite and mutually destructive though equal. But such pressures when exerted in the same direction are also equal in their effect on any body in their path, whereas in chemical interactions the effects of molecules or equal quantities of two unlike substances are only equal in the sense that each is that quantity which interacts with the same quantity of some third substance, which itself proves to be also a chemically equal quantity to them. For the products of the interaction in the one case are in part at least not the same as those in the other, though all prove chemically equal in further interactions.

To give an example: the molecule of ammonia is equal to that of aldehyde in that it combines with it and with it disappears, or ceases to exist as such. For the same reason it is equal to the molecule of hydrocyanic acid, and molecules of aldehyde and hydrocyanic acid equal to each other, because they, too, combine and disappear as such in doing so. But the molecule of ammonia again equals that of aldehyde in effecting transformation of hydrocyanic acid and its own self into something else. And lastly, chemically equal or molecular are the products of these combinations; aldehyde ammonia, ammonium cyanide, and aldehyde-cyanhydrine, not only among themselves, but also with the quantities of ammonia, aldehyde, and hydrocyanic acid from which they come and into which they return in other chemical changes. But with all this quantitative equality in transforming power, the substances produced are unlike and, each to each, peculiar to one of the three acts of chemical combination; and on this account exception may be taken to the treatment of molecules as equal chemical quantities. Yet the equality of molecules here asserted is but an extension of what is meant by the equivalence of certain atoms and radicals, since the atom and the radical are, nowadays, conceptions entirely dependent upon and derived from that of the molecule (apart, of course, from the atomic hypothesis); and this universally allowed equivalence admittedly does not extend to the identity of the products of the replacing activity of the atoms and radicals.

Quantitative equality and equivalency, it is true, have not the same meaning, equivalence being used to denote qualified equality, equality in certain specified ways, of quantities not equal in all other ways and possibly in no other. Quantities

of different substances cannot, strictly speaking, ever be equal, and can only be styled so in the sense of being equivalent; for were they equal in every way the substances would obviously be the same. But this fact, if it ever strikes one, is ignored by universal custom, and quantities of substances, however unlike—feathers, air, water, salt, and what not—are taken to be all equal, even by chemists as by the world at large, if only they have the same weight, notwithstanding the incongruities of the substances. I proceed now to show the baselessness of this conviction, but only to bring out more strongly the claim of chemical activity to equal rights with weight or mass in determining what are equal quantities of substances, for I am aware that here I have nothing to tell you that you do not already know. Weight being only the gravitational measure of mass, which itself is independent of it, quantities of substances are held to be equal when their masses are equal. Now, mass is quantity of matter. But what then is meant by matter? The answer must be either that it is a general term for any and all substances, or else that it is the common basis of all substances, which presents itself in all the different forms which are known to us as such, by virtue of a corresponding variety in its intestinal motions. I gladly pass over the latter answer without discussing it, on the ground that it introduces the subject of the intimate constitution of substances, which it is my set purpose to keep independent of in this discourse. I will only say of it that it would probably be the answer of many physicists and chemists, and yet that it gives such a limitation to the nature of matter as makes the common expression 'constitution of matter' devoid of all meaning. That expression means, and can only mean, the constitution of substances in common; and this brings me to the first answer, that matter is the term standing for all substances in common. Now, one thing which all substances possess in common is the property of resisting pressures; pressures not only of moving bodies, but of the motions of the ether and electrons. Measured or quantified, resistance becomes mass, all that can be signified by this term being the quantity of the resistance or inertia a substance exhibits when tested. It is the measure of a property of the substance, that is all; and there is no other way of quantifying a substance than through some one of its properties. No quantities of different substances can, as such, be commensurable throughout; and when compared and measured through some common property, such as the possession of mass, the equivalence or pseudo-equality found by this means is not the same as that found when some other common property is taken as the means of measurement. But experience has shown that though there are several rational and comprehensive ways of instituting, through some common property, comparisons between quantities of different substances, they all, with the exception of that of weighing, agree more or less exactly in pointing to the same order of equivalence, that of chemical activity; for with this are colligated those of gaseous volume and the other well-known physical activities, which give nearly the same quantities as it gives of different substances as being molecularly equivalent. There are, therefore, essentially only two measures of quantitative equivalency or pseudo-equality between substances, the dynamical and the chemical or molecular, the one wholly independent of and the other wholly dependent upon the particular nature of the substances compared. The former is the measure of dynamical phenomena, those of changes of bodies, due to their impacts and pressures, which may lead to their deformation and disruption, but do not involve transformations of the substances of the bodies into others; the latter is the measure of chemical phenomena, those of changes of bodies induced by such of their interactions as do involve transformations of the substances of the interacting bodies into other substances. Since it is already settled for us by custom that quantities of different substances are to be called equal when or because they are equivalent gravimetrically, and as it is not to be supposed that we shall ever give up calling 16 kilos. of oxygen, of salt, of chalk, and of every other substance, however unlike, equal quantities of them from the gravimetric point of view, we have no choice but also to call molecular quantities of these substances equal from the chemical point of view if the claim to coordination in equality of chemical with gravimetric equivalency is to be asserted and maintained.

The contention that chemical equality must be regarded as of as clearly defined a nature as gravimetric equality becomes the more weighty when it is reflected that our very definite views concerning gravimetric equality are due solely to the law of conservation of mass, the evidence for and against which, I may remind you, is just now to be discussed by Lord Rayleigh before the Physical Section. The mass of one pound of sodium remains unchanged when the metal is converted into salt, washing soda, or borax; if this were not the case, gravimetric equality would be just as definite as it is now but physicists would have to argue for its general recognition in much the same way as I am doing now for the recognition of chemical equality.

In further justification of this claim of chemical equality to coordinate rank with dynamical equality in the quantification of substances it may be well to take the fact into consideration that the determination of the former is independent of that of the latter. Overlooking the difficulties of the task, let there be at hand or always procurable unlimited numbers of parcels of the different substances to be experimented upon, each of which, by other means than weighing, such as spatial measurement, can be known to be equal to, or greater or less than, other parcels of the same substances. Suppose, now, that after many trials, one of a number of equal portions of sodium hydroxide has been found to be the quantity just necessary to interact with one of a number of portions of hydrochloric acid also equal among themselves. The products of the interaction will be some water and some salt. We can now have placed before us a parcel of sodium hydroxide equal to that previously used, another of hydrochloric acid also equal to that used, and the water and the salt obtained, and then have before us chemically equal quantities of four substances. Let now, by spatial measurement, a number of parcels of water be portioned out, all equal to that of the water obtained, and a number of parcels of salt equal to that of the salt obtained. By a series of trials we find a quantity of silver nitrate just sufficient to interact with the sodium chloride, and having, by supposition, taken this quantity of silver nitrate from a lot of other parcels equal to it, we find that one of these is just sufficient to interact with one of the portions of hydrochloric acid equal to that used in producing one of the portions of salt. Further, we find that the salt and the hydrochloric acid each produce a substance which is the same, namely, silver chloride, and in the same quantity as the other. Along with it in the case of the salt, is sodium nitrate, and in the case of the hydrochloric acid, nitric acid. We can then find that this quantity of nitric acid is just enough to interact with one of those of sodium hydroxide, and thereby produce quantities of sodium nitrate and water, respectively equal to those obtained in the other interactions. If now we conjoin with these experiments others in which hydrogen, sodium, and silver are each caused to combine with chlorine, and others in which hydrochloric acid, silver chloride, and sodium chloride are electrolysed into these elementary substances, evidence is obtained of such facts of chemical composition and decomposition and of double decomposition (or what happens when compounds interact) as those upon which the science of chemistry is framed.

In teaching chemistry the point is kept too much in the background, if not altogether out of sight, that the chemical equality of quantities of different substances is independent of all other relations of equality between them, and that, therefore, its validity is not affected by the fact of its terms agreeing with some and not with other terms of equalities determined in other ways. Instead of bringing out this point the molecule of water is given out as being, primarily and prominently, that quantity which has eighteen units of mass and which measures two unit volumes. Both statements happen in the nature of things to be true, but neither of them describes the molecule. Let it be clearly understood from illustrative examples what is meant by 'chemically equal,' and there is hardly more to be said as to what constitutes a molecule of water than that it is the quantity of it chemically equal to that of some other substance presenting itself for comparison. 'Molecule' is a term of relation: it stands for an equal quantity, not for any particular quantity; but as such it is as easy to understand and as indefinable as an equal volume or an equal weight of a substance.

It is then only as colligated equalities, established by experiment, that gaseous volumes, osmotic pressures, and other properties of substances come into consideration, first as enforcing the truth of the conception of the indicated quantities as equal, and then as the means of molecular measurement without resort to chemical change. But of the purposes served by the colligative properties, that of giving molecular measurements without recourse to the evidence afforded by chemical change is well known to be of the very widest application. To determine chemically the molecular equalities of substances, single chemical changes of suitable character, changes which are cases of double decomposition, have to be looked for; and to know these with the desirable degree of certainty calls for a much larger acquaintance with the chemical behaviour of the substances than can usually be gained at the early stage of work when the knowledge of the molecule is of the utmost assistance in the further investigation of the nature of the substances. Consequently, it is nearly always through recourse to physical methods that the molecule is first ascertained, and then through the molecule the certainty acquired that some particular interaction is a single one, thus reversing the normal order of things, which undoubtedly is that the molecule in chemistry, however it may have been first determined, is recognised as such by being what it is in chemical change.

I shall have been wholly misunderstood by you if you suppose that I would make light of the importance of the balance in chemical operations, or of the value of its indications in chemical investigations. Once the weights of molecular or atomic quantities have been ascertained the balance becomes the most accurate and generally the most easily applied instrument for apportioning substances in these quantities. Chemical interaction, to be employed in this way and without the aid of the balance, is practically useless, for the reason that it involves the destruction of the quantities it measures. Out of this dependence on the balance arises the exceeding importance of accurate tables of atomic weights, from which molecular weights are derived by addition; but the place for these tables is not on the walls of the lecture-theatre, but in the laboratory pocket-book and, perhaps, in the balance-room. Besides the use of the balance and of atomic weight tables for getting and calculating out molecules of different substances at pleasure, there is the indispensable service they perform in enabling chemical analysis to be carried out and applied to the solution of the problems offered by chemical change. The primary problem of every science is to find some element of sameness in the diversity of its phenomena, in order that they may be compared, a problem which was solved for chemistry to a large extent by Dalton, and ceased to exist when the distinction had been made between molecule and atom. But this having been solved, there comes the other problem, namely, to find definite, that is, quantitative differences in the midst of the uniformities, and these for the chemist are differences of mass or weight. Through that redistribution of mass which attends chemical interactions it has been possible to trace out to some extent the nature of the transformation of substances and develop the science on the lines of chemical composition and chemical constitution. Thus, then, the balance has become and will continue to be the necessary instrument of chemical research; but again I would remind you that it records its facts in units which are not ours, and of which we avail ourselves only as the means to an end. Sodium chloride is chemically composed, not of 3545 equal parts of chlorine with 2305 of the same equal parts of sodium, but of equal quantities of these simple substances.

The theory of chemical molecules or equalities and their relations to the equalities between the weights and gaseous volumes of different substances were brought to light not by Richter's law of chemical combining proportions, and not by Avogadro's hypothesis as to there being equal numbers of particles in the same volume of different gases, but in the first place by Dalton's atomic theory and Gay-Lussac's law of simply related gaseous volumes in chemical change; and then, much more fully in the middle of the last century, through the brilliant work of Gerhardt, Williamson, Laurent, Odling, Wurtz, and others, in the purely chemical field. Dalton gave us the conception of the molecule, though confused

with that of the atom, as the unit of measure of chemical activity in place of the gravimetric unit; the work of the chemists of the last mid-century gave us a fuller conception of the molecule, along with the notion of chemical change as being substitution in the molecule effected by what became known as double decomposition. Up to that time chemistry had been treated only as the science of compounding and decomposing or reducing. Sodium added to oxygen gives soda, sulphur added to oxygen gives sulphuric anhydride, soda added to the anhydride gives sodium sulphate, ethylene added to chlorine gives dichlorethane, water subtracted from alcohol leaves ether, and so forth. All this is strictly true in a limited way, but then it is not chemistry; and the addition precedes and does not constitute the chemical union. In the sodium sulphate we perceive no soda, no anhydride, no sodium, sulphur, or oxygen. That is to say, there is evidence of the addition and subtraction of mass and some other such evidence; but, for the rest, evidence of addition there is none. Were it otherwise there could be no chemistry. It is true that one of the great things accomplished by chemistry has been that of establishing the law of the conservation of mass, without which to rely upon the chemist would be unable to carry on his experimental investigations. But that is only because, like the steady point to the seismologist, it is there unchangeable when all else is changing. Since it is the law of no change, it cannot serve to explain what is change. Far from being the science of the composition of substances, chemistry might be defined as being the science of the non-composition of substances where that composition might have been looked for from the antecedents. If salt is verily a compound of sodium and chlorine, and can be broken up into these, why have the fragments not the marks on them of that whole of which they formed a part? It is true that 5850 parts of salt become 3545 parts of chlorine and 2305 parts of sodium, nothing being gained or lost in weight; but to account for that there is no need of chemistry, a science which takes cognisance of the phenomena of change, and not of those of unchanged properties. The use of the word 'composition' in chemistry cannot be discarded now, and all that is necessary to make it unobjectionable is to see that the term is always qualified by the prefix 'chemical' when there is a possibility of mistake about its significance, and that that significance is carefully explained, if not defined and fully illustrated, before it is given over to the beginner.

The facts of a chemical nature about common salt which cause the statement to be made that it is a chemical compound of chlorine and sodium are such as these. Salt can be wholly changed into sodium and chlorine; these substances brought together change into salt and nothing else; salt and sodium, each under conditions appropriate to it, change into the same substance, called also a sodium compound, such as sodium hydroxide; salt and chlorine, each in its own way, change into the same chlorine compound, such as hydrochloric acid; neither sodium nor chlorine, one apart from the other or the other's chemical compounds, ever changes into salt; salt is, directly or indirectly, producible in the chemical interaction of a sodium compound with a chlorine compound; the properties of salt are much less like those of either sodium or chlorine than like those of some other substances; in sensible and other physical properties the chemically compound substance, salt, is as simple as or simpler than either of the chemically simple substances, sodium and chlorine; lastly, the laws of combining proportion by weight are obeyed in all the chemical changes in which salt takes part.

With exclusive reference to such facts as these, the chemical composition of a substance will, I think, be found to be satisfactorily defined, as its having the power, capacity, or property of being wholly producible from and wholly convertible into, directly or indirectly, those substances of which it is said to be composed. A simple substance differs from one that is compound only in not possessing the power of being by itself convertible into two others, or of being produced alone from any two others. Simple substances are not less varied or less complex in their physical properties than compound substances, while their chemical constitution is often more problematic than that of many which are compound. The term 'simple,' therefore, is as misleading in the language of chemistry as 'compound,' unless defined and qualified in use by the word 'chemically.'

The ground really occupied by chemical composition in theoretical chemistry is now greatly limited; for with the full acceptance of the idea of the molecule and of the atom as a derivative of it, its place has been taken by chemical constitution to an extent hardly realised. The useful and practically necessary expression of the results of the quantitative analysis of a new substance gravimetrically is all that can strictly receive the name of its chemical composition. When the term is applied more widely it is used for what are really the simpler forms of chemical constitution. It was otherwise before the conception of the molecule had become current and the atom had become a derived function of the molecule. Chemical composition as expressed by Dalton in atoms is indeed that and nothing else. Carbonic anhydride is composed, according to him, of two atoms of oxygen to one of carbon, as against carbonic oxide which is composed of one; marsh gas of two atoms of hydrogen to one of carbon, as against olefiant gas composed of one. But then it was only numerical necessity which led him to adopt such a mode of expressing the facts. The same necessity, it is true, affects us also in the matter of carbon dioxide, of water, and of ammonia, but how little it does so is shown by the many cases in which the empirical or simple composition is expressed in multiples. The atomic chemical composition of ethylene is two of hydrogen to one of carbon, and that of benzene one of hydrogen to one of carbon. When we say, as we always do, that the one substance is 'composed' of four atoms of hydrogen to two of carbon, and the other of six of hydrogen to six of carbon, we give what is information concerning the constitution of these substances. Call it the composition of the molecule as we may, it is evident that by composition we can here mean only constitution. As with polymerism, so with isomerism, and in a more marked way. Mercurous sulphate and mercuric oxysulphite, quite distinct salts, have yet the same composition.

In the great reformation wrought by the chemists to whom I have referred, but by Gerhardt in particular, the new light set up in chemistry was the notion of what came to be called 'double decomposition' in chemical change. The phrase is not, perhaps, happily constructed, but it has the merit of needing some explanation of its meaning before it can be understood, and troubles, therefore, through a too simple apprehension of the sense of the word 'composition' are hardly to be feared. Its introduction into chemistry marked the ascendancy of the idea of the molecule as the factor in chemical change whose interactions with other molecules were to be considered, instead of those additions which, as chemical phenomena, never take place. It led also to new conceptions of the nature of the atom and the compound radical as being the quantitative and qualitative expressions of the powers possessed by substances to change into others, and to the conception of the valency of atoms and radicals as expressing the nature of the connection of successive chemical changes. The zeal with which it was attempted to force all chemical changes into the form of double decomposition interfered, perhaps, with the full recognition of its importance; but the fact remains that, with hardly an exception, all that is stated concerning the nature of those chemical changes in which two or three substances become one, or one becomes two or more, is based upon notions derived from the study of double decomposition.

The fundamental value of double decomposition consists in its displaying threads running through chemical transformations which can be followed up. When two substances change into two others, and only then, there can be found, in most cases relations of resemblance, both physical and chemical, between the before and after of a chemical change. Instead of the striking unlikenesses shown by the substances formed by quasi-addition to those from which they are formed, there are here met with the similarities of the outcoming to the interacting substances, and the similarities between the products of different interactions in which the acting substances are similar. Chemists had been for very long familiar with acids, bases, salts, without becoming deeply impressed with the significance of the resemblances which these class-names imply, and also with the facts that acids beget acids, bases bases, and salts salts, or in more general terms, that substances in interaction produce others like them, and that differences between the products and the agents in one change are distinctly repeated in a similar change in which

other substances are concerned, points now given expression to by such terms as 'chemical constitution,' 'homologous' and 'analogous series,' 'Kopp's law,' &c.

What is so important to consider in the study of double decomposition is that the fact, that the sum of the masses of the two products of the change is the sum of the masses of the two interacting substances, presents itself no longer as being merely the evidence of the massing together of substances into a compound; for there is in double decomposition to be considered that redistribution of mass which, on the one hand, is found to correspond to and be part of a general though not sharply defined redistribution of physical and chemical properties; and, on the other hand, to be obviously irreducible to that interchange of those simpler substances which in many cases are produced in the simple decomposition of the acting substances.

The physical properties of substances, or rather their sensible qualities, are of too uncertain a character for their redistribution to be safely traced. But it generally does result, amongst inorganic substances, at least, that colour is transmitted, the saline, acid, bitter, or other taste of one of the active substances will appear, with more or less distinctness, in one of the products, a relatively volatile and a relatively fixed substance together will yield a similar pair of products, a dense and a light substance will yield a dense and a light substance, and so on. The chemical properties, however, are quite definitely redistributed to a large extent, a fact sufficiently illustrated by saying that an iron salt yields an iron salt, and a sulphate yields a sulphate.

But this is not a redistribution in which simpler substances, or indeed any other substances than those interacting, play a part; as soon becomes evident on attempting to establish the contrary by an appeal to the facts. While silver acetate and silver sulphate resemble each other and also silver nitrate as silver salts, they do not resemble silver itself; and though silver nitrate resembles sodium nitrate as nitrate, there is not even a substance known which is related to these salts as silver is related to silver salts. It might be objected to this that there may yet become known such a substance, which in its ultimate decomposition would give one molecule of nitrogen to three molecules of oxygen. If instead of nitrate were given acetate or cyanide, there would be found in the substances acetic peroxide and cyanogen, it might be said, the analogues of the as yet unknown substance of the nitrate. But the point I would make is that nitrate, sulphate, &c., are names with well-defined meanings independent of the fact that the corresponding substances are not known; for it follows without argument that also the terms silver, iron, chloride, &c., should be equally independent in meaning of the existence of the substances silver, sodium, chlorine, &c. It is a familiar historical fact that cesium, helium, and fluorine were chemical names long before the substances cesium, helium, and fluorine became known. We might well be convinced, therefore, without going further, that constitutional names, names which convey the facts of likenesses preserved in chemical change, cannot be indicators of the presence of the substances for which they may be also used. For, that being the case, we have no grounds for assuming that silver nitrate in interaction with sodium sulphate decomposes into the substance silver, which then combines to form silver sulphate. But fuller proof than any appearance of likeness or unlikeness can give is afforded by facts which became known and appreciated in connection with the chemical molecule. Typical of them all is the fact that in none of its interactions does chlormethane yield a hydrocarbon simpler than methane or than itself. Under those conditions in which it might have been expected to give a substance which would be methyl, it produces ethane, a substance which chlorine converts into another substance, having instead of one-third only one-sixth less hydrogen in its composition. Similar results have been obtained in all cases where the point can be determined—that is, where the simpler substance looked for would still be a compound substance, and such simpler derivatives are looked for no longer. The monohydride of oxygen or sulphur, the dihydride of nitrogen or phosphorus or arsenic, the mononitride of carbon, the organic compounds, methyl, phenyl, acetyl, are not only unknown, but are held to be non-existent substances, though their chemical compounds, the hydroxides, amides,

cyanides, and the rest, are both numerous and well defined. Whatever other view we shall have to take of the constitution of Gomborg's remarkable 'triphenyl-methyl,' it will certainly not be that it is identical with the radical of the triphenylchloromethane from which it is derived, unless we are prepared to allow that carbon is sometimes tervalent. Ethylene the substance differs from ethylene the radical in having its two carbons differently related; but it is difficult to see how to make a similar distinction in the case of Gomborg's substance.

In those other cases in which the point is not strictly determinable, only because the resulting substances are the simple substances themselves, it required but the recognition of molecular quantities to make it evident that these cases run parallel with the others. For, in all changes which can be satisfactorily followed out, the resulting or entering quantity of the simple substance is twice as great as that which can have come from, or gone to form the molecule of either of the compound substances. But if, so far as can be traced, a simple substance comes only half from one molecule of any of its compounds, none of these compounds can contain or be composed of simple substances. All simple substances, therefore, as well as all compound substances, enter into and come out from chemical changes as dual in all of them in origin and disappearance. Their colligative properties have been appealed to in order to confirm this observation, but with conflicting results, sometimes confirmatory of the chemical evidence, sometimes contradictory of it, and sometimes too complex for confident chemical interpretation.

I refer here more especially to Avogadro's proposition, which is in effect that equal volumes of gases are chemically equal or molecular. As in the case of Dalton's atomic theory, there is to be distinguished in this proposition what Avogadro really put forward as new from what he took for granted. Admitting, as was to him a matter of course, that gases have in equal volumes equal numbers of particles, he asserted that in the case of elementary substances these particles are not the atomic particles, but, as in the case of compound substances, particles compounded of these, which interact with the particles of other gases as chemically equal each to each. If now this proposition is divested of all hypothesis, all reference to the mechanical structure of gases, it becomes the law that equal volumes of gases at the same temperature and pressure, whether simple or compound, are almost exactly chemically equal quantities, and once in possession of this law we find nothing becomes clearer by assuming that equal volumes of different gases contain the same number of chemically equal particles. This law is, obviously, an advance upon Gay-Lussac's law similar to that of the chemical molecular theory upon the atomic theory of Dalton. Unfortunately, however, it does not hold good in the case of not a few simple substances, and it seems impossible from the chemical point of view, and consistently with the molecular theory, to admit that, because the gas-volume has only half the expected mass, the chemical molecule of sodium or mercury is not bipartite like that of hydrogen or oxygen, and chemically equal to either.

The dual constitution or chemically compound nature of the simple substances as thus established by the part they take in chemical interactions furnishes further evidence of the untenability of the belief that the molecule is chemically composed of two substances, or their substitutes, simpler than itself, when we consider that, were this true, there would be chemical union between two things perfectly alike, two portions of the same thing. This difficulty was, I believe, first raised by Berzelius, and has never been met. Physically, the matter is simple enough, if motion in the opposite direction is not counted as a difference between two masses. But this would be a non-elective union, whilst chemical union is elective.

The difficulty, insurmountable when made, does not arise when the fact is recognised that every chemically single substance, whether simple or compound, is, as a substance, one and without parts, and can never, therefore, be built up of or broken down into parts different from itself. One substance (as two molecules) or two substances change into two others or into two molecules of one, in an interaction which is instant, uninterrupted, and irresolvable into stages, where the interaction is single in character. But just as a body can be mentally analysed (as in the investigations of dynamics) into mass and motion, which apart are un-

known, and as these again can each be conceived of as further divided, resolved, condensed, and otherwise qualified as centres of mass, compounded motions, and so forth, so the chemist is enabled mentally to find quantitatively defined this, that, and the other mark of the many chemical interactions which have or may have gone to bring it into existence, and will or may again have place in the possible forms of its dissolution into others. The two methyls in the constitution of ethane, about which we are quite certain, are not two things held together till some interaction sunders them in the chemical dissolution of ethane, but the double mark of similarity between it and other methyl compounds in their chemical interactions. We cannot say that only one part of the ethane is methyl, or hydrogen, or carbon, but that part of its nature, of its constitution, is its behaviour as a methyl compound, or, again, as an ethyl compound; or, more comprehensively but less specifically, part of its constitution is its behaviour as a hydrocarbon, as a hydrogen and as a carbon compound. But these are different aspects of it, different relations of it, not differing parts of the one homogeneous substance.

With the laudable object of combating the prevalent notion that matter is something which is the basis or essence of a body, something acting as the medium of the manifestation of its forms of energy, a distinguished and most lucid writer on chemistry has, adequately perhaps for that object, represented a body as a compound of the various forms of energy subsisting together and cohering in certain proportions within the volume of the body. But this presentation of a subject as a cohesion or association of forms of energy is on the same footing as the presentation of ethane as consisting of two methyls bonded together, or two portions of carbon with six of hydrogen. It is compounding what cannot be had apart, what cannot be even conceived of as separate, so far as bodies are concerned. The analysis of bodies into manifestations of different properties are only mental operations. A moving body, a hot body, a green body, an explosive body, becomes by legitimate abstraction a phenomenon of motion, of heat, of colour, or of light, or a chemical phenomenon as our needs require; but the body is there all the while, and its undivided and continuous existence is indispensable to the phenomenon. The body can be hotter or colder, but not that only,—not that without other differences; red-hot iron is throughout a very different thing from cold iron, and ice differs widely from steam in most of its properties. A substance is no more composed of its properties or energies than it is composed of its so-called elements. It manifests its presence in a thousand and one ways more or less distinguishable; its properties are so to manifest itself. But no divisibility of itself while it remains itself can be thought of, no differentiation can be suggested, no nucleus with its superinduced properties can be traced.

It ought, therefore, to be possible to express all the particulars of chemical constitution without making any assumption as to substances having parts or structure. Of chemical constitution itself, I doubt whether there is to be found a definition which is not couched in language having reference to the minute mechanical structure of substances, notwithstanding the fact that all knowledge of their chemical constitution has come to us through observation of the properties of the substances themselves, and more particularly their relations in cases of double decomposition. Bearing in mind that all terms are relative, I think the chemical constitution of a substance may be defined as the resemblances shown by it in its chemical changes to other substances, often better known than it and taken as types, these resemblances being indicated and described usually by means of special nomenclature and notation. As this nomenclature and notation have been developed out of those designed to express chemical composition, it is well to point out that the notion of chemical constitution is independent of that of the latter, though clothed to some extent in its language and symbols.

The notions of radical and atom are so intimately related as to be often used indifferently, the one for the other. The radical, ethylene, is always an atom of ethylene, the radical nitrogen always an atom of nitrogen. Radical and atom are in fact the qualitative and quantitative aspects of the same thing. They are thus exactly parallel with substance and molecule. We can think of unquantified substance, and perhaps of unquantified radical, but in chemistry we

never really want such conceptions; one of the many definitions of science is the quantification of phenomena, and in every chemical phenomenon the substances concerned are quantified as molecules. The quantification of radicals expressed by the atom is fundamentally the same in principle as that of substances, namely, that of chemical equality in interaction; but it may be better to say that it is dependent upon the quantification of substances as molecules.

In the interaction of double decomposition each substance by contact and union with the other develops and manifests a dual character by becoming distributed as the two new substances, with the consequence that each of these has certain properties the same as those of the one, and certain others the same as those of the second interacting substance. What is common in this way to one of the interacting and one of the resulting substances is a radical of these substances, of which there are evidently four in every double decomposition. These radicals of a single interaction are defined as whatever two parts of the powers of a substance to yield the simple substances of its chemical composition are, in certain interactions, continued separately from each other in the two new substances. But the pair of radicals developed in the various double decompositions of a substance being by no means always the same, one of the radicals of one pair must include in its composition part or all of one of those of another pair. Acetic acid has for one pair of radicals methyl and carboxyl, and for another pair acetyl and hydroxyl. Of these, carboxyl includes hydroxyl, and acetyl includes methyl. Again, acetic acid yields the hydrogen and acetate radicals in one interaction, and hydroxyl and acetyl in another, so that in these cases the acetate radical includes acetyl and the hydroxyl includes the radical hydrogen. Now, what is common to carboxyl and acetyl and what is common to the acetate radical and hydroxyl are also treated as radicals, the one being known as carbonyl and the other as the radical oxygen. These are examples of what may be distinguished from the others as the polyvalent radicals. They are radicals of radicals, and therefore also radicals of substances. They may be defined as the common part of two or more other radicals. A single definition of all radicals can be given, but it is not instructive. A radical is any single power or any interdependent association of the powers of a substance to produce simple substances which continue in any product or series of successive products of its chemical change.

Before I leave the subject of the radical I wish to repeat that it is only when it is interacting that a substance shows a dual character or division, as it were, into parts or radicals, and that the duality it then shows is determined as much by the nature of the other substance as by its own. A substance is neither actually nor conceptually the sum of its radicals. The very fact of the difference of these in different interactions should be proof of this; though it only leads to its being taken to be at least the sum of its ultimate or simple radicals. If, however, it is not the sum of its proximate radicals, it is hard to see how it can be imagined to be that of the ultimate ones. In relation to its radicals, a substance must be held to present itself as any one of these for the purpose of investigation, and at the standpoint from which it is considered. It is then to the mind that particular radical, though also something else; just as snow is white and cold, yet also something else, for the moment unconsidered. Nor can the two products of an interaction be looked upon as themselves the sum in properties of the interacting substances. To a limited extent and imperfectly, we can attach to a given radical certain of the properties common to its compounds; but it needs no greater insight than we have already, to recognise that a substance cannot be what it is in one way, without being in that way greatly affected by what it is in another. This is now a recognised but not sufficiently considered point, and I therefore welcome those publications of Professor Vorländer, of Halle (who now honours this Section with his presence), in which he has been vigorously calling attention to the extent to which the properties of a substance, acid, basic, stable, and what not, depend as much as, if not more, upon the interrelations of the radicals than upon the radicals themselves.

One other thing I have to say about the radical, which is as to the spelling of

the word. I plead for a return to the ending of the word radical with 'al,' now interdicted in the 'Journal of the Chemical Society.' It seems appropriate to call the powers of a substance to behave chemically as it does, the roots or radical parts of its chemical nature, but it does not seem appropriate to call them radicles or rootlets. Americans and all other nationalities but our own use the original spelling.

I have put off too long, perhaps, all reference to the properties of very dilute aqueous solutions of salts, but I wished first to discuss the nature of the radical. The osmotic pressure and other dependent points which are particular in the behaviour of such solutions are in full accordance with the assumption that an electrolyte by dissolution in much water becomes a pair or a binary system of two interdiffused quasi-substances called 'ions.' These ions must differ from isolated substances in bearing equal and opposite quantities of electricity; in being each unknown apart from its fellow; and in having a composition not to be found in actual substances, though identical possibly with that which a radical would have were it a substance. The ions can be indeed separated from each other, but not to continue as themselves, since in the act of separating they form ordinary substances, either by uniting with other ions, or by two molecules of ion becoming one molecule of substance. In the former way of separation the ions of two salts interact on mixing their solutions; in the other way, the ions become substances when their solution is placed in a galvanic circuit. In this mode of separation—by electrolysis, that is—the substances corresponding with the two ions, or else secondary products of their change, are produced, the one substance at the kathode and the other at the anode, while the solution away from the electrodes, but between them, remains for the time unaltered in composition. Along with this there occurs in many cases a phenomenon first recorded by Daniell, and afterwards investigated by Hittorf with such beautiful results. This consists in a greater fall taking place in the concentration of the salt solution close to one electrode than in the concentration of that close to the other, as though the ions were hydrate compounds, and that the one ion was a higher hydrate than the other. Until we know more of the nature of the ions themselves this phenomenon is most conveniently quantified on the hypothesis that the ions travel as molecular particles, but the discussion of this hypothesis is beside my present purpose.

The phenomena of ionisation or, in other words, the particular properties of dilute solutions of salts, belong evidently to a change unlike all other chemical changes. It is a polarised chemical change, in which the equivalent and complementary products of the interaction appear apart and at remote surfaces of the mass of decomposing salt solution. Two points which call for notice in connection with my present subject are that an ion is one of a pair of quantities commensurate with the quantity of the salt itself that is or would be in interaction; and that it is molecular in character and therefore to be regarded as a relative and wholly variable quantity.

Dalton's atoms were both the atoms and the molecules of present-day chemistry, but much more the latter than the former. Although the chemical atom can now be no more than a dependency of the molecule, it is commonly set up as the starting-point in chemical theory, and as having an independent existence as a quantity of the substance, while the molecule is represented as being a conjugation of atoms. But there cannot be two standards in reference to the same thing, and in molecular chemistry the atom must give way. As I have already had occasion to point out, the atom is of the radical, the molecule is of the substance.

The four radicals of a double decomposition are equal and chemically complementary. These chemically equal quantities of such radicals are atoms. The quantities of all other radicals are also atoms, but only those of proximate radicals, those of a single interaction, are equal. Similarly, the quantities of the four substances of a single interaction are all equal and are molecules, but the quantities of substances are not equal in other interactions. These others are treated as the simultaneous occurrence of two or more single interactions, which they can always

be represented and sometimes demonstrated to be. Calcium hydroxide and hydrogen sulphide give calcium hydrosulphide and water by two single interactions together, which in this case can be easily distinguished, since the calcium hydroxide will also interact with only half as much hydrogen sulphide to form the insoluble crystalline calcium hydroxyhydrosulphide and half as much water as before; this calcium salt will then interact with as much more hydrogen sulphide as went to form it, and produce the very soluble crystalline calcium hydrosulphide. Or the calcium hydrosulphide and as much calcium hydroxide as yielded it will readily interact to form twice as much as the first-obtained quantity of calcium hydroxyhydrosulphide. Thirdly, the calcium hydrosulphide and half as much water as was formed with it from calcium hydroxide readily interact to produce calcium hydroxyhydrosulphide, and half as much hydrogen sulphide as was needed to form the hydrosulphide. Therefore, and on other grounds, we say and know that one molecule of calcium hydroxide and two molecules of hydrogen sulphide give one molecule of calcium hydrosulphide and two molecules of water. This is, of course, only the law of multiple proportions introduced into chemical interactions. The expression 'two or more molecules of a substance' has a meaning only as indicating the number of simultaneous or successive single interactions which have led to the conversion of certain substances into others.

Now a similar but complementary state of things meets us in the case of radicals. Instead of the coefficients of molecules, necessitated by having to consider many chemical changes as being cases of two or more single interactions occurring together, there are the valency coefficients of the polyvalent radicals, called out also by such a compound interaction. Thus, in the above case, whilst the single interaction between hydrogen sulphide and calcium hydroxide shows calciumhydroxyl as one of the radicals, the succeeding interaction between the calcium hydroxyhydrosulphide and more hydrogen sulphide shows the radical calciumhydrosulphuryl, and the common part of these two radicals is the bivalent radical, calcium. It will be evident that to give the atom of the calcium radical as bivalent is a statement reciprocal or complementary to that of giving two molecules of hydrogen sulphide as interacting with one of calcium hydroxide. Chemical equality remains still the measure of the atom, but that, in complex changes, whereas the number of molecules of one substance marks the number of single interactions, the valency number of the atom marks the same thing for the radical. It is a matter of valency, and not otherwise a matter of the atom. The radical calcium is never actively bivalent in a single interaction; in other words, it is never equal to two atoms of hydrogen. As a simple radical it does not take part in such an interaction; but it does do so as a radical of radicals, such as calciumhydroxyl and calciumhydrosulphuryl, and then has the same measure as—is equal in exchange to—the atom of hydrogen, though carrying with it of necessity other radicals, a thing the hydrogen radical never does or can do. To take another example: when acetamide is formed from acetic acid, the nitrogen of the amidogen and the oxygen of the hydroxyl are equal in exchange, but because of their valencies the one carries with it two atoms and the other one atom of hydrogen. This is no matter of merely academic contention, for upon its recognition rests the doctrine of valency itself.

The quantity of the radical is the only proper and sufficient definition of the atom, whether the radical be that of a single interaction, or a radical of radicals, that is, a polyvalent radical. The atom is, therefore, the quantified power of a substance, as the compound of the radical, to produce other compounds of the radical, including its compound with itself, where that is possible. As with the molecule of a substance, so with the atom of the radical, it is of no fixed magnitude, and may weigh a kilogram just as well as only a milligram or something much less. Being a relative quantity and nothing by itself, of its indivisibility there is nothing to be said outside its definition; whilst, as to its being the smallest relative quantity interchanging in an interaction, it had only thus to be defined when there was uncertainty as to the molecule and the single interaction.

It has been impossible for me to discuss the nature of the radical and the atom without referring to valency, but it is itself a subject of such importance as to need

special consideration. It does not seem right to me to say even the little I can say about valency without naming with the respect they deserve from us the distinguished chemists who laid the foundations of the doctrine and developed it: Williamson, Odling, Wurtz, Edward Frankland, and Kékulé. I had the good fortune to be in the same laboratory as, and then intimate with, Kékulé, when in 1854 he was working out the bivalency of sulphur and oxygen by his investigation of thioacetic acid, some time, that is, before he had thought out the benzene ring and the valency of carbon.

Only when, as is usual, propositions are made in which a separate and independent existence, with valency as a property, is imputed to a radical, does the question, as to what valency is, present any difficulty. Approaching it from the side of the molecule and of double decomposition, and therefore from the experimental side instead of from that of the radical itself, as is customary, valency presents itself as being the number of single interactions necessary, in order to have a certain radical occur, first as that of one substance, and then as that of another which has no other radical in common with the first substance. That ammonia possesses one atom of the radical nitrogen, and three atoms of the radical hydrogen, and that the nitrogen radical is trivalent and the hydrogen radical univalent are statements mutually based upon facts such as the following. Potassium nitrilosulphate, which contains nitrogen but no hydrogen, is converted by water in a sharply defined single interaction, into potassium hydrogen sulphate, and into potassium imidosulphate, a substance which contains all the nitrogen along now with hydrogen. This salt passes, also sharply and by a single interaction with water, into as much more sulphate along now with potassium amidosulphate, which latter substance contains all the nitrogen and twice as much hydrogen as belonged to the imidosulphate. Lastly, the amidosulphate interacting with water gives a third quantity of potassium sulphate, equal to the last, and also ammonia, having all the nitrogen of the nitrilosulphate started with, three times as much hydrogen as the imidosulphate, and nothing else. That is to say, the nitrilosulphate and the ammonia have no other radical than the nitrogen the same, while three single interactions have been necessary to separate in this way the nitrogen radical from the three atoms of the potassiumsulphonyl radical. Therefore the nitrogen radical is trivalent and its quantity is the atom. Again, there are three atoms of the univalent hydrogen radical in the ammonia molecule, because in each of the three interactions an equal quantity of this radical is brought in from water. Ammonia shows only one pair of radicals, behaving, so far as its own interactions go, exclusively as a compound of amidogen and hydrogen, and these radicals are referred to as united or bound together in being ammonia. It is only the interactions of its derivatives, primary, secondary, and tertiary, that are indicated by treating the amidogen as ultimately nitrogen and two hydrogen radicals. But this involves the consideration of all three hydrogens as bound to the nitrogen; and it becomes, therefore, of vital importance to bear in mind that the hydrogen radical, proper to the ammonia itself, is bound to a nitrogen radical which carries also bound to it two other hydrogen radicals.

Chemical formulæ still remain to be considered. They are symbolisations of deductions from experimentally ascertained facts, and are independent of the interpretation commonly given to them as referring to the minute differentiated structure of substances. A chemical equation expresses a chemical change quantitatively by means of chemical formulæ which are molecular. In a case of double decomposition, therefore, there are four formulæ; but when two or more such interactions are expressed in one equation, because they occur together, the formulæ of transition-substances do not appear, and then numerals before formulæ tell the number of interactions in which separate molecules of the substance have taken part. A formula represents the relative interacting quantity or molecule of a substance, while the single symbols composing it stand each for an atom of the radical of a certain simple substance as possessed by the substance formulated. The connecting lines and dots, and certain collocations of the symbols, indicate the association of the simple radicals as compound radicals in different interactions.

What is symbolised by position formulæ, and indeed by the formula altogether, are the chemical activities and abilities of the substance and its derivatives, and their analogies with those of other substances. When not in interaction, a substance has no constitution and no formula. It is certainly not on any experimental grounds that it can be regarded as some spatial arrangement of unlike parts. To take the simplest case; if we start with sodium hydroxide and symbolise its molecule by some mark, such as X to begin with, the interaction of the substance with an acid leads us to replace the X by two symbols and a connecting mark. One of these will be Na for the sodium radical; let the second be Z for the other radical, and let a dot or stroke be placed between the symbols to mark them as those of a pair of radicals in interaction. In other interactions, such as that with melted potassium acetate, we find need for a new pair of symbols, one being H for the hydrogen radical, while the other may be Q. But it is easy to decompose two molecules of sodium hydroxide in one operation into molecules of sodium, hydrogen, and oxygen, from which fact we learn that Z is replaceable by the double symbol O-H, and Q by O-Na. Thus, Na-Z and H-Q become equally Na-O-H, which records the ultimate radicals of sodium hydroxide, together with all its interactions, immediate and remote. But it does this with no more implication of spatially placed and tied parts than is made by expressing the measured flow of time by a straight line, or than is to be found in t^2 seconds of time, or in c^2 as the third power of a number, unless we specifically condition this symbol as stereometric. A formula is not to be read—on experimental grounds, I mean—as a symbol of parts juxtaposed and joined on, and should be regarded as an intricate but legible monogram telling the chemical nature of the substance. Every symbol in it is to call to mind a phase of the chemical activity of the substance or of its derivatives, a phase that may be for the time as the substance itself to the investigator, just as a pigment substance becomes only a red or a white to the painter. For example, salt is often nothing more than its chlorine phase to the chemist when he wants only a soluble chloride; whether it is of potassium, sodium, or ammonium, then matters not to him.

The double linking of the carbons in ethylene is a symbolised expression or facts, without reference to hypothesis. The two carbon radicals of ethane or of alcohol behave together just as does the single carbon of methane or the nitrogen of ammonia in being, but with a valency of six, continued to other compounds devoid of all the other radicals of the ethane or alcohol—that is, of the hydroxyl and the hydrogens. The quadrivalency of each carbon is made up by the interaction necessary to dissociate or to bring together the two methyls, which counts as a unit of valency to each carbon. Ethyl hydrogen sulphate decomposes into sulphuric acid and ethylene, the hydrogensulphate radical with a hydrogen radical becomes the acid as the one product, while the methylene radicals again pair off as the two methyls had done when ethane was formed, thus producing the non-saturated substance, ethylene. Since there is a perchlorethylene, the second linking mark falls between the two carbons; and when ethylene passes back to an ethane compound two units of valency are displayed by it without the carbons becoming dissociated.

Position formulæ of isomerides, such as those of propyl alcohol and acetone, present no difficulty, because they are interpreted as the expressions of unlike double decompositions. It is not unfrequently the case that no constitutional or structural formula can be given to a substance which shall express all the pairs of radicals possible in its interactions, of which the best-studied example is that of ethyl acetoacetate. This state of things, known as tautomerism, admits of no other interpretation than that there are really two substances existent, of which one only is known, the other or so-called 'pseudof orm' requiring the assumption of its existence as a transition-substance only. The notion of the shifting hydrogen radical is but the hypothetical way of viewing the intervention of the intramolecular change by which the substance becomes its 'pseudof orm.'

The cyclic formula of benzene expresses the fact that, unlike a fatty hydrocarbon, benzene shows but one pair of interaction radicals, hydrogen and phenyl. The 'ortho-,' 'meta-,' and 'para-' positions in benzene derivatives are only expres-

sions of facts of 'position' isomerism, such as those pertaining to other non-saturated compounds, but more complex to unravel and more varied and interesting. It is doubtful whether the Kékulé ring does not remain as efficient a symbol as any stereographic substitute yet proposed for it; but it itself is purely a symbol of chemical interactions, and has no spatial significance other than what may be put into it by convention. 'Adjacent,' 'opposite,' and the like have only application literally to the arrangement of the symbols; but if the symbolisation is perfect the 'opposite' carbons will, as a matter of course, always indicate the same point concerning the chemical interactions.

Whether the chemical formulæ for the lactic acids are better arranged in a plane or as a tetrahedron is to be decided by the facts concerning these and other asymmetric carbon compounds, the object being to symbolise or formulate as distinct and complementary in certain physical properties, but alike in their chemical interactions, two isomeric substances, simultaneously formed in molecular quantities. Enantiomorphous arrangements of the respective formulæ of dextro- and lævo-lactic acids fully meet the case, but the facts are in no way explained by these formulæ. In the enantiomorphously related hemihedral crystals of the corresponding salts of the dextro- and lævo-acids, and in their opposite rotatory effect in solution upon the plane of polarised light, we recognise something like a torsioned state of the whole homogeneous substance, something to be accounted for by peculiarity of chemical origin, but not something made more intelligible by any imagined arrangement of unlike parts. It is possible to give an account of the chemical facts without making reference to mechanical structure, and then to reason about them somewhat in the following way: Given the case of a substance doubly equipped with the power to take part in a certain interaction, and considering that the exercise of the power can only be single, and that it cannot be made without affecting and transforming, or perhaps nullifying, the second equipment with power, predict what will happen. That is the prediction called for concerning any interaction which generates an asymmetric carbon compound. The result could never have been predicted; yet how natural and beautiful it is when it comes to us through experiment enlightened by the genius of Pasteur, Le Bel, and Van 't Hoff! That answer is that a twinned substance results, one indeed in most respects and chemically, but two in certain physical properties, characterised by presenting phenomena as of equal and opposite strains, a polarised pair of substances, in fact. What I mean by the double equipment with power is, of course, the pair of identically related and self-identical radicals, or the bivalency of one radical wholly and directly associated with the carbon radical. The case of the oxygen radical of aldehyde is that of the bivalent radical; the other case is that of the two carboxyl radicals of hydroxytartaric acid, or that of the two methylene hydrogen radicals of alcohol which these carboxyls have replaced. The tetrahedral formula with its reflected form admirably symbolises the case of enantiomorphously related pairs of substances. But no light whatever is thrown upon the nature of this pairing by the tetrahedron model; its value depends upon the fact that as a symbol it so fully matches the constitution of the substances.

Here I bring my summary of chemical theory and its formulation without hypothesis to a conclusion, hoping that, to some extent, I may have impressed you with the fact that the exposition of even advanced chemistry, in its symbolic, equally as in its ordinary language and nomenclature, is independent of any hypothesis as to the mechanically and chemically differentiated structure of substances, and that chemistry can be studied and still further developed without reference to such a structure. I have asked for few or no reforms in the use of either terms or symbols, my point having been only to press for a consideration and discussion of the doctrines of chemistry and the great atomic theory itself as something concerned exclusively with experimental chemical facts.

British Association for the Advancement of Science.

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ADDRESS TO THE GEOLOGICAL SECTION,

BY

LIEUT.-GENERAL CHARLES ALEXANDER McMAHON,
F.R.S., F.G.S.

PRESIDENT OF THE SECTION.

Rock Metamorphism.

I WISH to offer some observations to-day on some aspects of rock metamorphism; and as this is a complex subject, and the time at my disposal is brief, I purpose to deal with it in simple language, and to avoid as far as possible all petrological technicalities.

A short description of a granite in the Satalj Valley of the Himalayas will, I think, introduce us by a short cut to the consideration of 'contact metamorphism,' an important branch of the subject under consideration.

The granite I allude to is an intruder in the normal gneissose-granite of the Himalayas, and cuts through it at right angles to its foliation.

The intruder, which is some yards wide, did not rise through a simple crack or fissure, for its passage upwards was interrupted by a sheet of dark intrusive diorite, older than itself, which ran, roughly speaking, parallel to the foliation of the gneissose-granite.

This sheet of diorite offered considerable resistance to the rising granite.

The granite zigzagged backwards and forwards across the diorite and ran along its edges for fifty yards or more, converting it into a mica trap.

It then tore itself away and continued its upward course. The granite I am describing was in a molten or fluid condition at the time of its eruption, as I hope to show in my subsequent remarks.

I may pause here, however, to consider in passing what was the probable temperature reached by a granite such as that above described.

The question is one of very great difficulty, as we know so little about the plutonic conditions of igneous rocks, and can only arrive at an answer to our question by indirect evidence.

The melting point of quartz ranges from 1425° to 1450° C., but the fusion point of granite need not necessarily be as high as this, inasmuch as the presence of water at high temperature materially lowers the melting or solution point.

The fusion point of the other constituents of granite may here be mentioned: that of orthoclase ranges from 1164° to 1168° ; microcline, 1169° ; albite, 1172° ; augite and hornblende, 1188° to 1200° ; apatite, 1221° . Zircon, which

is commonly found in granites, and is one of the first minerals to separate out of the magma, is shown by Ralph Cusack to have probably a melting point of 1760° ; whilst topaz, a not uncommon mineral in granite, is infusible up to the melting point of platinum, namely, 1770° C.

If we consider, therefore, the melting points of the mineral constituents of granite, we can hardly avoid the conclusion that for the magma to have attained perfect fluidity it must have reached a temperature of at least 1200° C.

Vernadsky has shown that kyanite is transformed into sillimanite, a well-known product of contact-metamorphism at a temperature of 1320° to 1380° .

If rocks in contact with granitic masses have been raised to this temperature, it follows that the granite itself must have been still more heated. Vernadsky's observations have been relied on by Mr. George Barrow in his well-known paper 'On an Intrusion of Muscovite-biotite Gneiss' in the S.E. Highlands of Scotland to account for the presence of sillimanite in the inner zone of metamorphism between the kyanite schists and the granite, and he considered that the temperature attained by the 'central masses of the Highland rocks' was probably higher than the figures indicated by Vernadsky.

Bearing all considerations in mind, including the influence of water and alkali in reducing, and of pressure in raising, the melting point, I think we may safely infer that granites, such as the Himalayan granite alluded to above, must have been raised at plutonic depth to a temperature midway between red and white heat, that is to say, to at least 1200° C.

To return to the granite of the Satlej Valley under consideration, I wish to draw attention to its condition just before crystallisation commenced.

A study of the mineral beryl will, it seems to me, throw light on this point.

Beryl is an important accessory mineral of the granite under description. It is clearly an original mineral, and it is material to note that it was the first mineral to crystallise out of the magma of the Satlej granite. This is shown by several circumstances.

In the first place the beryl preserved its perfect crystallographic shape, showing that its molecules during the entire period of crystallisation possessed comparative freedom of motion, and were not interfered with or molested by other solid minerals. In the second place all the essential minerals of the granite when they subsequently crystallised out of the magma were deposited on the crystals of beryl. I have specimens of the granite showing crystals of beryl enclosed in felspar, in muscovite, and in quartz.

The beryl, therefore, having been the first mineral to crystallise, the examination of thin slices of it under the microscope ought to give us a clue to the condition of the magma at the time the beryl was formed.

I have made such an examination, and I find that the beryl is crowded with liquid and gas cavities, the former containing movable bubbles and deposited crystals as well as water.

The bubbles are of substantial size relative to the area of the cavities, showing that the water suffered considerable contraction after it was sealed up in the beryl.

Scrope long ago suggested that the fluidity of a magma below the melting point was due chiefly to the water they contained, and attributed the liquidity of granite to the same cause.

Scrope, however, in ascribing the mobility of an igneous rock to the presence of water seems to have had regard principally or wholly to its mechanical action in furnishing an elastic medium in the interstices between the crystals or grains of the rock. He observes that a lava consists 'of more or less granular or crystalline matter, containing minute quantities of either red-hot water, or steam in a state of extreme condensation, and consequent tension, disseminated interstitially among the crystals or granules, so as to communicate a certain mobility to them, and an imperfect liquidity to the compound itself,' and he quotes Scheerer and Delesse, both of whom assert that water exists in mechanical combination with all crystalline rocks, 'its minute molecules being intercalated between the crystals.'

Nowadays one would attribute the liquidity of an igneous rock not so much

to the mechanical action of the water present in it as to the combination of the water with the mineral contents of the lava, producing a state of solution.

Sorby's investigations supported Scrope's observations, for he proved that the liquid contained in the inclusions in granite is water, and showed that it was caught up during the formation of the crystals, 'and was not introduced subsequent to the consolidation of the rock.'

The water now contained in cavities in the beryl was probably held in solution by the constituents of that mineral at the time of its formation, and as it cooled down the water separated from the substance of the beryl and formed the cavities in which we now find it imprisoned.

If this be so, it follows that when the beryl crystallised out of the magma, the latter was in a fluid condition, and held a considerable amount of heated water in solution. The temperature of the magma must have been above that of red heat, and the potential energy of the water held in a fluid state by pressure must have been great. When therefore in the course of the earth movements which accompany or in some cases are caused by the intrusion of eruptive igneous masses, pressure was temporarily relieved by the rupture and faulting of rocks, the superheated water contained in the magma would be ready to flash into steam with almost explosive violence.

It must also be borne in mind that water under great pressure, at or above a red heat, has a powerfully solvent action on most minerals, even on so refractory a mineral as quartz. When therefore granite in the molten and fluid condition of the Satlej granite was erupted along a line of faulting, fissure, or weakness, the superheated water or steam, bearing with it much mineral matter in solution, must have acted with great chemical energy on the rocks into which it was intruded.

I have spoken of water carrying mineral matter in solution, and of a magma carrying water in solution. These two conditions may rapidly succeed each other under varying conditions of temperature and pressure. To use the words of Van Hise, 'under sufficient pressure and at a high temperature there are all gradations between heated waters containing mineral material in solution and a magma containing water in solution.'

The condition of the beryl crystals, crowded as they are with liquid cavities, shows how high a proportion of superheated water was contained in the fluid granite magma at the time of their formation.

Sorby estimated that the fluid cavities in the quartz of granites sometimes amount to more than ten thousand millions to the cubic inch. As quartz, however, is usually the last mineral of a granite to consolidate, it may be thought that the water contained in it is a residuum left by the felspar and muscovite on their separation from the magma; but the case of the beryl above quoted shows clearly that the amount of water diffused through the magma before the mica, felspar, and quartz began to consolidate must have been very considerable. The amount of water held in solution by a granite, during the time of its aqueo-igneous fusion, cannot be estimated by the amount of water given in the analysis of consolidated and dried hand-specimens of that rock. A considerable proportion of this liquid must necessarily have been lost during the gradual cooling of the rock, and in the course of its intrusion into neighbouring sedimentary strata as sheets, dykes, and veins. Sorby, as the result of other lines of investigation, came to the conclusion that the amount of water present in granite, though limited, is considerable.

We must now turn for a few minutes to consider the important question of the porosity of minerals, and their permeability by heated water and gas at high pressure.

The fact that solid substances are built up of molecules having interstitial spaces between them hardly needs demonstration nowadays.

But have we all quite realised that the molecules of rock-forming minerals and crystals are not inert particles of matter, but that they vibrate or revolve or are endowed with other orderly movement that may be likened to the motion of the planets round the sun?

Far, far away in space the solar system would to an eye formed like our own,

in all probability present a nebulous appearance, because the eye would not be able to see the individual members of our system.

So, too, the molecules of which crystals are built up may have their appropriate motions, but we cannot see them with the eyes of sense because the molecules are beyond the highest powers of the microscope.

We can, however, I think, perceive them with the eye of the scientific imagination; and the hypothesis that the molecules of minerals are separated from each other by intermolecular spaces, and have their modes of motion, seems essential to the comprehension of rock metamorphism.

The important experiments of Sir W. Roberts-Austen on the diffusion of gold in pure lead throw considerable light on this subject.

Disks of solid gold were held against the bases of cylinders of lead by clamps, and were kept in an upright position at the ordinary temperature for four years. At the end of this time it was found that the gold had diffused upwards in the solid lead, for a distance of 7.65 mm., in sufficient quantity to be detected by the ordinary methods adopted by assayers. Traces of gold were found still higher.

When a column of molten lead, 16 cm. high, was placed above solid gold and kept at a mean temperature of 492°C ., that is to say, at 166° above the melting point of lead, but 569.7° below that of gold, the gold diffused in considerable amount, to the top of the lead column, in a single day.

Sir W. Roberts-Austen's experiments, above alluded to, demonstrate that even such metals as gold, whose melting point is as high as 1061.7°C ., exhibit a measurable amount of kinetic energy at the ordinary temperature and pressure. Great results may no doubt be brought about at ordinary temperatures and pressures, when time, as in the laboratory of nature, is practically unlimited; nevertheless the importance of high temperature and high pressure, in operations connected with metamorphism, can hardly be overrated.

Not only does a rise in temperature increase the energy of the chemical actions and reactions which produce the mineralogical changes embraced by the term metamorphism, but it increases the porosity of minerals and facilitates the passage of liquids and gases through their pores.

The cohesion of molecules is lessened, the amplitudes of their vibrations, rotatory or other movements, are increased, and a passage is opened for the advance of chemical materials into the heart of the crystal.

Increase of temperature thus not only throws open the doors of the mineral fortress attacked, but gives enhanced energy to the invaders. The fact that the mineral components of a rock are, under conditions of heat and pressure practically porous to heated water, laden with chemical reagents in solution, is frequently brought home to the mind of the petrologist in a very tangible way. We sometimes observe, for instance, that metamorphic changes begin at the heart of a crystal, and leave the peripheral portions of it fresh and unaltered.

In such cases the chemical agents of change have evidently passed freely through the outer parts of the crystal, and have by preference selected its internal parts for attack.

In order to explain clearly how this remarkable result takes place, in the cases referred to, it will be necessary to diverge for a few minutes to consider another branch of our subject. It is difficult, if not impossible, to lay down any hard-and-fast rule of universal application, because the conditions under which igneous rocks crystallise vary with temperature, pressure, the relative proportion of constituents, and other local causes, and these variations in the conditions may materially affect the results; but I think the rule that minerals crystallise out of a molten magma in the order of their basicity is of very frequent if not of absolutely general application. This rule also governs the growth of individual crystals, especially those that exhibit what is known as zonal structure. Take, for instance, the feldspars of an igneous rock. A gradual passage may frequently be traced by the petrologist from one species of feldspar at the heart of a crystal to another distinct species at its periphery. Sometimes a crystal is made up of more than two species, which shade more or less gradually into each other. In accordance with the rule laid down above, the more basic species formed first;

then, as the percentage of the bases left in the magma gradually decreased, owing to the first formed crystals having taken a lion's share of the available bases, the feldspars that formed later became gradually more and more acid in composition. Thus a large feldspar of slow and gradual growth may be composed of several zones, each zone being successively less basic and more acid than that upon which it crystallised, each successive zone thus possessing slightly different physical properties from the one that formed before it. These statements are capable of proof. When sections of feldspar, such as occur in thin slices of igneous rock, are examined under the microscope in polarised light, petrologists can distinguish one species from the other—when the direction in which the sections were cut is approximately known—by measuring the angles at which they extinguish from the twinning or the pinacoidal plane.

This is not mere theory. Each species of feldspar has its own angle of extinction and its own index of refraction. The determination of these two factors enables a petrologist to prove optically the change in composition; or, in other words, the change in species which has taken place in the successive zones, during the gradual growth of a large zonal feldspar.

Another general rule must now be mentioned. I think it may safely be asserted as a broad rule, that the different species of feldspars are attackable by the chemical reagents which make themselves felt in metamorphic action, in the order of their basicity; that is to say, the more basic feldspars are more easily attacked than the acid ones. When we bear in mind the facts stated above, we shall, I think, be able to see clearly how it is that the peripheral portions of large feldspars in igneous rocks sometimes escape alteration, whilst the cores of these crystals are converted into secondary minerals, such as chlorite, silvery mica, zoisite, epidote, kaolin, steatite, saussurite, calcite, and scapolite.

The chemical reagents flowing in solution through the pores of the feldspars, pass by the more acid and refractory species, and devote their energies to the more susceptible basic species entombed at the heart of the zonal crystals.

The point I wish to enforce most strongly is that the phenomenon above described, namely, the formation of secondary metamorphic minerals in the interior of a crystal, combined with the comparative immunity to change of the external portions, shows that the agents which brought about chemical changes at the core of the crystal flowed freely through its unaltered peripheral portions.

But some may ask whether the chemical agents referred to may not have gained access to the heart of a crystal by a crack. I answer that a crack is a coarse and tangible object that looms large under the microscope. A crack in a mineral liable to metamorphic action, through which chemical reagents have flowed, could not escape detection. The finest crack through a homogeneous mineral, such as, for instance, an olivine, can be readily seen, not only by the small canal worn by the corrosive action of the chemical agents that flowed through it, but by the alteration set up in the mineral along the whole course of the canal.

I have a thin slice from a beautifully fresh olivine contained in one of the lavas of Vesuvius collected by myself. A volcanic explosion or other cause, operating after the crystallisation of the olivine, produced a very fine crack in the mineral through which water, charged with chemical reagents, subsequently flowed. The crack, though of microscopic width, is filled with serpentine, and on both margins fibrous serpentine has been formed at the expense of the parent olivine, and constitutes a fibrous band on both sides of the crack throughout its entire length, the direction of the fibres being at right angles to the crack.

The rest of the olivine is of virgin purity and polarises in the most brilliant colours, contrasting strongly with the serpentine.

In this case it is clear that the chemical reagents, though free to flow along the crack, had commenced to extend beyond its walls, encouraged thereto by the porosity of the olivine itself. But how different is this case from those in which the entrance of the chemical agents had not been facilitated by a crack. In the case above described, the chemical changes set up were limited to the borders of the crack, and even had they gradually extended in the course of time to the

whole of the olivine, the original canal by which the chemical reagents had gained access to the crystal would have remained to tell its tale, and exhibit along its course the banks of iron oxide thrown down by the chemical navvies that had excavated it.

Cracks save time as roads and canals do, but they leave behind them evidence of their former existence. In order to understand fully how rocks and minerals are so completely open to the attacks of chemical reagents, which penetrate to and produce chemical and mineralogical changes at the very hearts of minerals, we must fully realise how completely porous rocks and minerals are, to the heated liquids which carry these reagents with them in solution. Heat, as before stated, not only increases chemical energy, but destroys more or less completely the cohesion between molecules, and increases the amplitude of the vibrations, or other motions of the molecules, and consequently facilitates the entrance of liquids and gases into the pores of minerals, and their complete permeation by these powerful agents of change. Thus far we have been chiefly concerned with some of the principles underlying the branch of our subject embraced by the term contact-metamorphism, which implies operations conducted at considerable depths below the surface of the ground, under conditions of heat and pressure.

We must now consider very briefly changes produced at or near the surface by the agency of water, or, as Bischof in his well-known work termed it, metamorphism in the 'wet way.'

No hard-and-fast line, however, can be drawn between the two classes of operations, as the one gradually shades by fine gradations into the other. At one end of the scale we have high pressure and high temperature, and a fluid igneous magma holding water in solution, above a red heat, and giving up heated water or vapour charged with salts to the rocks in contact with it.

Passing to the other end of the scale through diminishing temperatures and pressures, we reach a condition in which the water circulating through the rocks at ordinary pressure and temperature is more abundant in amount, and holds acids and salts in solution, capable of setting up important chemical reactions in the rocks and minerals to which it gains access.

In the case of surface operations, moreover, the metamorphic agents—water, acids, salts—are being constantly renewed. Conditions differing as widely as the conditions at the extreme ends of our scale do not yield, however, precisely the same results. In both metamorphic change goes on with more or less briskness, but the products are different. Some minerals require great heat and great pressure for their production, and such minerals are never formed by any surface process of weathering. For instance, the temperature reached determines whether titanium dioxide crystallises as rutile, or in one of its other two forms, rutile requiring a temperature of over 1000°C ., and being the only form of titanium dioxide 'stable at a high temperature.'

Temperature also seems to determine whether the silicate of alumina crystallises as andalusite, kyanite, or sillimanite, the two former being transformed into the latter, at a temperature of 1320°C . to 1380°C .

On the other hand some minerals require little heat for their formation, and are readily produced by metamorphic changes in the 'wet way.'

There seems to be some correspondence between the melting point of minerals and their density; thus in the case of eleven minerals produced by contact-metamorphism, whose average specific gravity ranges from 3.06 to 4.03, I find that their melting point ranges from 954° to above 1770°C ., high temperature and high pressure (a concomitant of plutonic conditions) appearing to be factors in the production of high specific gravity in minerals.

The genesis of individual species of minerals is a fascinating study, but the subject is too large to enter upon here.

Water gains access to rocks in several ways. It falls as rain; it rises from hidden depths; it leaks from the sea into horizontal beds or into strata dipping away from it; and it penetrates through faults and fissures. Rain in its descent takes up from the air oxygen, nitrogen, carbonic acid, and in some cases small amounts of nitric acid.

It is thus in itself a powerful solvent and potent agent in producing chemical change.

In its passage through the surface soil it dissolves humic and other organic acids, the products of vegetable decay, which add greatly to its solvent power and enable it to break up many silicates and to dissolve even silica.

By the time the rain-water reaches the solid rocks below the surface soil, it has become a very active agent in producing chemical change in them. It is by such agents, persistently applied during long periods of time, that large areas of ultra-basic igneous rocks have been altered into serpentine.

Hot springs are a well-known instance of water rising in considerable quantity from plutonic depths. They are known to occur in the plains of India, and are especially abundant in the Himalayas. I visited two very interesting ones at Suni, in the bed of the Satlej River, west of Simla. These springs rise apparently under the very bed of the river, and come to the surface on both banks within a yard or two of the rushing water of the Satlej. When I visited the springs they had a temperature of 130° F., and contrasted strongly with the cold water of the river flowing past them, which had descended from high Himalayan glaciers and had a temperature of 49° F.

The native inhabitants of neighbouring villages told me that the hot springs always appear at the very edge of the river, whatever may be the height of its waters during drought or flood. This statement is probably true, for I think the springs well up from below through the walls of a fault that traverses the bed of the Satlej at a high angle to its course, and the springs thus come to the surface on both its banks.

The metamorphic influence of these springs on the rocks in this locality has been very powerful. The ancient volcanic rocks there exposed have, for some distance up the river, been altered by aqueous agents almost out of recognition. The original structural characters of these lavas have been almost completely broken down and an amorphous substance substituted for the crystals and minerals of which they were originally composed.

This result shows that the crystals and minerals of these old lavas must, for all practical purposes, have been completely porous to the aqueous agents brought to bear on them.

The general transmutation of one mineral into another by the action of heated water holding mineral agents in solution, aided by heat and pressure, may take place in a variety of ways. Some of these processes are simple, but others are highly complex. Many are the results of a single operation, others of a series of changes, some of which prepare the way for those that follow.

In some cases the change may be brought about by the removal, in whole or in part, of one or more of the essential constituents of a mineral, whereby the relative proportions and mutual relations of those that remain are altered, as the following examples will show.

By loss of water limonite passes into hæmatite, and opal into crystalline quartz. Dyscrasite, by loss of antimony, passes into native silver, and pyroxene, by the removal of its lime and iron, is changed into talc. Simple oxidation or the absorption of oxygen by a mineral is responsible for another class of changes, as in the conversion of zinc blende into goslarite, and antimony into valentinite.

The loss of one or more of the ingredients, concurrently with the introduction of one or more new ones, causes many metamorphic changes, as in the conversion of marcasite into magnetite, of witherite into barite, and of azurite into malachite.

The well-known conversion of a peridotite into serpentine is a case in point. Here, part of the iron and magnesia is removed from the olivine, and water is introduced. A simple process like this, brought about by the percolation of surface waters through an igneous rock, is sufficient to transform considerable areas of rock masses into serpentine, as has been the case in parts of Cornwall.

Some metamorphic processes are more complex than those alluded to above, but Nature has unlimited time at her disposal, and is able to manufacture potent chemical reagents as her processes proceed. For instance, the sulphides of various metals of common occurrence in rocks, most of which, with the exception of those

of the alkaline metals, are insoluble in water, by taking up oxygen pass into sulphates, most of which are soluble in that liquid at the ordinary temperature.

These sulphates are readily carried away in solution, and become potent factors of change in rocks through which water charged with these salts flows. Again, carbon dioxide, so abundant in percolating water, decomposes minerals containing lime or alkali, and removes them as soluble carbonates to effect powerful chemical reactions elsewhere.

I must pass over the subjects of paramorphism and pseudo-morphism, as the limited time at my disposal does not permit me to enter upon these subjects.

In the above sketch I have contented myself with a brief discussion of some of the leading principles that seem to me to underlie contact action and metamorphism in the wet way, because I venture to think that, if we really understand these two divisions of our inquiry, it will be unnecessary on the present occasion to enlarge on other branches of our subject.

Take, for instance, what is commonly called dynamic metamorphism. The main factors in this kind of metamorphism are the folding, crumpling, crushing, and shearing of rocks by earth movements, especially during the upheaval of mountains.

But these dynamic forces are potent factors in the development of heat.

In the case, therefore, of dynamic metamorphism, as in contact metamorphism, pressure and heat are the main factors acting in conjunction with the water shut up in or circulating through a rock. If we understand how these factors operate and produce the results we see in cases of contact metamorphism, we shall not fail to understand their action in a case of dynamic metamorphism.

These observations also apply to regional metamorphism; that is to say, to metamorphism produced in rocks at great depth, by being brought within the influence of the interior heat of the earth. The action of heat in increasing molecular motion and kinetic energy is well understood nowadays, and so long as we get heat it seems to me immaterial how heat is generated in rocks subject to metamorphic action.

In the above sketch I have intentionally omitted to enter into the details of chemical and mineralogical action that has brought about individual cases of metamorphic change.

Volumes would be required to do justice to so complex a subject, and the details would, in an opening Address, be out of place.

In conclusion I have, I trust, shown how important a part water plays as an agent of metamorphism, not only at and near the surface of the earth, but at plutonic depths. We have seen that the molten granite of the Satlej Valley, which was given as an illustration of a fluid igneous magma, contained a considerable proportion of water held in solution at considerably above red heat, and that the fluidity of the magma was due to its presence. We also saw that the great heat to which the magma was raised increased the potential energy of the contained water when a relief of pressure opened the way for the intrusion of the molten magma into neighbouring rocks. We also saw that this water was rendered by heat a powerful solvent, and that it carried with it into the adjoining rocks the mineral matter of the granite in solution. We also saw that heat increased the porosity of minerals, facilitated the passage of liquids laden with mineral matter through their pores, and increased the potency of chemical action.

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ADDRESS TO THE ZOOLOGICAL SECTION

BY

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PRESIDENT OF THE SECTION.

The Morphological Method and Progress.

It is now twenty-eight years since this Association last assembled in Belfast, and to those present who can recall the meeting the proceedings of Section D will be best remembered for the delivery of an address by Huxley 'On the Hypothesis that Animals are Automata, and its History,' one of the finest philosophic products of his mind. At that date the zoological world were about to embark on a period of marked activity. Fired by the influence of the 'Origin of Species,' which had survived abuse and was taking immediate effect, the zoological mind, accepting the doctrine of evolution, had become eager to determine the lines of descent of animal forms. Marine observatories were in their infancy; the 'Challenger' was still at sea; the study of comparative embryology was but then becoming a science; and when, reflecting on this, we briefly survey the present field, we can but stand astonished at the enormity of the task which has been achieved.

Development has proceeded on every hand. The leavening influence, spreading with sure effect, has in due course extended to the Antipodes and the East, in each of which portions of the globe there have now arisen a band of earnest workers pledged to the investigation of their indigenous fauna, with which they are proceeding with might and main. Of the Japanese, let it be said that not only have they filled in gaps in our growing knowledge, for which they alone have the materials at hand, but that, with an acumen deserving the highest praise, they have put us right on first principles. I refer to the fact that they have shown, with respect to the embryonic membranes of the common chick, that we in the West, with our historic associations, our methods, and our skill, contenting ourselves with an ever-recurring restriction to the germinal area, have, by an error of orientation, missed an all-important septum, displaced under an inequality of growth.

Those of us who have lived and worked throughout this memorable period have had a unique experience, for never has there been progress so rapid, accumulation of observations so extensive and exact. Of the 386,000 living animal species, to compute the estimate low, every one available has been laid under hand, with the result that our annual literary output now amounts to close upon 10,000 contributions, the description of new genera and sub-genera, say 1,700. More than one half of this vast series refer to the Insecta alone; but notwithstanding this, the records of facts of structure and development, with which most of us are concerned, now amount to a formidable mass, calculated to awe the unlettered looker-on, to overwhelm the earnest devotee, unless by specialising he can secure relief. As an example of what may occur, it may be remarked that a recent exploration of the great African lakes has resulted in the discovery of over 130 new species.

As to the nature of this unprecedented progress, it will suffice to consider the Earthworms. In 1874 few were known to us. An advance in our knowledge, which had then commenced, had made known but few more which seemed likely to yield result. Darwin's book upon them had not appeared. Some were exotic, it is true, but no one suspected that a group so restricted in their habits could reveal aught beyond a dull monotony of form and structure. Never was surmise more wide of the mark, for the combined investigations of a score of earnest workers in all parts of the world have in the interval recorded some 700 odd species of about 140 genera. Mainly exotic, they exhibit among themselves a structural variation of the widest possible range. Not only do we recognise littoral and branchiate forms, but others achætous and leech-like in habit, to the extent of the discovery of a morphological overlap with the leeches, under which we are now compelled to remove them from their old association with the flat worms, and to unite them with the earthworms. And we even find these animals, as represented by the *Acanthodrilidae*, coming prominently into considerations which involve the theory of a former antarctic continent, one of the most revolutionary zoo-geographical topics of our time.

This case of the earthworm may be taken as typical of the rest, since for each and every class and order of animal forms, the progress of the period through which we have passed since last we assembled here has produced revolutionary results. Our knowledge of facts has become materially enhanced; our classifications, at best but the working expression of our ideas, have been to a large extent replaced in clearer, more comprehensive schemes; and we are to-day enabled to deduce, with an accuracy proportionate to our increased knowledge of fact, the nature of the interrelationships of the living forms which with ourselves inhabit the earth.

Satisfactory as is this result, it must be clearly borne in mind that its realisation could not have come about but for a knowledge of the animals of the past; and turning now to palæontology, it may be said that at the time of our last meeting in this city the scientific world were just becoming entranced, by the promise of unexpected results in the exploration of the American Tertiary beds, then being first opened up. The Rocky Mountain district was the area under investigation, and with this, as with the progress in our knowledge of recent forms, no one living was prepared for the discoveries which shortly came to pass. To consider a concrete case, we may premise that study of the placental mammals had justified the conclusion that their ancestors must have had equal and pentadactyle limbs, a complete ulna and fibula, a complete clavicle, and a skull with forty-four teeth; must have realised, that is, the predominant term of the living *Insectivora* as generally understood. Who among the zoologists of our time does not recall with enthusiasm the revelation which arose from the discovery, during these early days, in the Eocene of Central North America, of the genera at first described as *Eo-* and *Helohyus*? The evidence of the existence, in the locality named, of these forty-four toothed peccaries, as they were held to be, rendered clearer the records of the later Tertiary deposits of the old world, which were those of hogs, and, in correlation with the facts then known, suggested that the Rocky Mountain area was the home of the ancestral porcine stock, and that in Early Tertiary times their descendants must have migrated, on the one hand, across the northern belt, of which the Aleutian Islands now mark the course, into the old world, to beget, by complication of their teeth, the pigs and hogs; and on the other into Central South America, to give rise, by numerical reduction of teeth and toes, to the peccaries, still extant.

Migration in opposite directions with diversity of modification was the refrain of this remarkable find, far-reaching in its morphological and zoo-geographical effects. Nor can we allude with less fervour to the still more striking case of the horses, which proved not merely a similar, though perhaps a later, migration, but a parallelism of modification in both the old and new worlds, culminating in the latter in extinction, whereby it became necessary, on the advent of civilised man, to carry back the old-world horse to its ancestral American home. No wonder that this should have provoked our Huxley to the remark that in it we have the

'demonstrative evidence of the occurrence of evolution,' and that the facts of palæontology came to be regarded as certainly not second to those of the fascinating but seductive department of embryology, at the time making giant strides.

I have endeavoured thus to picture that state of zoological science at the time of our last meeting here; and I wish now to confine myself to some of the broader results since achieved on the morphological side. But let us first digress, in order to be clear as to the meaning of this phrase.

We do not expect the public to be accurate in their usage of scientific terms; but it is to me an astounding fact that among trained scientific experts, devotees to branches of science other than our own, there exists a gross misunderstanding as to the limitations of our departments. I quote from an official report in alluding to 'comparative anatomists, or biologists, as they call themselves,' and I but cite the words of an eminent scientific friend, in referring to biology and botany as coequal. In endeavouring to get rid of this prevailing error, let it be once more said that the term 'biology' was introduced at the beginning of the nineteenth century by Treviranus and Lamarck, and that in its usage it has come to signify two totally distinct things as employed by our Continental contemporaries and ourselves. By 'Biologie' they understand the study of the organism in relation to its environment. We, following Huxley, include in our term biology the study of all phenomena manifested by living matter; botany and zoology; and by morphology we zoologists mean the study of structure in all its forms, of anatomy, histology, and development, with palæontology—of all, that is, which can be preferably studied in the dead state, as distinct from physiology, the study of the living in action. Comparative morphology, the study of likeness and unlikeness, is the basis of our working classifications, and it is to the consideration of the morphological method, and the more salient of its recent results, that I would now proceed, in so far as it may be said to have marked progress and given precision to our ideas within the last eight-and-twenty years. I would deal in the main with facts, with theories only where self-evident, ignoring that type of generalisation to which the exclusive study of embryology has lent itself, which characterises, but does not grace, a vast portion of our recent zoological literature.

To the earnest student of zoology, intent on current advance, the mental image of the interrelationships of the greater groups of animal forms is ever changing, kaleidoscopically it may be, but with diminishing effect in proportion as our knowledge becomes the more precise.

Returning now to American palæontology, we may at once continue our theme. In this vast field, expedition after expedition has returned with material rich and plentiful; and while, by study of it, our knowledge of every living mammalian order, to say the least, has been extended, and in some cases revolutionised, we have come to regard the Early Tertiary period as the heyday of the mammals, in the sense that the present epoch is that of the smaller birds. No wonder then that there should have been discovered group after group which has become extinct, or evidence that in matters such as tooth-structure there is reason to believe that types identical with those of to-day have been previously evolved but to disappear. To contemplate the discovery of the Titanotheria, the Amblyopoda, the Dinocerata with their strange diminutive brain, chief among the heavier ungulate forms, is to consider the Mammalia anew; and when it is found that among late discoveries we have (1) that of a series of Rhinocerotoides, which though not yet known to extend so far back in time as the primitive tapirs and horses are complete as far as they go; (2) that among the Ruminants we have, in the Oreodontidæ of the American Eocene, primitive forms with a dentition of forty-four teeth, an absence of diastemata, a pentadactyle manus, a tetradactyle pes with traces of a hallux, and, as would appear from an example of *Mesoreodon*, a bony clavicle, such as is unknown in any later ungulate, we are aroused to a pitch of eager enthusiasm as to the outcome of labours now in hand; for, as I write, there reaches me a letter, to the effect that for most of the great vertebrate groups, and not the mammals alone, collections are still coming in, each more wonderful than the last.

In the extension of our knowledge of the Ancylopoda, an order of mammals named after the *Ancylotherium* of Pikermi and Samos, which occur in the Early Tertiary deposits of Europe, Asia, North America, and abundantly in Patagonia, we have been made aware of the existence of genera whose salient structural features combine the dentition of an ungulate with the possession of pointed claws, believed to have been retractile like those of the living cats. Conversely to these unguiculate herbivores, which include genera with limbs on both the artio- and perisso-dactyle lines, there have been found, among the so-called Mesonychidæ, undoubted primitive carnivores, indications of a type of terminal phalanx seal-like and approximately non-unguiculate; from all of which it is clear that we have in the rocks the remains of forms extinct which transpose the correlations of tooth and claw deducible from the living orders alone. Further, among the primitive pentadactyle Carnivora we meet, in the genus *Patriofelis*, with a reduction of the lower incisors to two, and characters of the fore limb which, with this, suggest the seals. It is, however, probable that these characters are in no way indicative of direct genetic relationship between the two, for, inasmuch as these animals were accustomed to seek their food in the water of the lake by which they dwelt, their seal-like characters may be but the expression of adaptation to a partially aquatic mode of life—of parallelism of modification with the seals and nothing more.

Early in the history of their inquiry, our American confrères recorded from the Pliocene the discovery of camel-like forms possessed of a full upper incisor dentition; for example, the genera *Protolabis* and *Ithygrammodon*; and now they have arrived at the conclusion that while the camels are of American origin one of their most characteristic ruminants, the Prongbuck (*Antilocapra*), would conversely appear to be the descendant of an ancestor (*Blastomeryx*) who migrated from the old world.

Sufficient this concerning the work in mammalogy of the American palæontologists. While we return them our devout and learned admiration, we would point out that the brilliance of their discoveries has but beclouded the recognition of equally important investigations going on elsewhere. In Argentina there have proceeded, side by side with the North American explorations, researches into the Pleistocene or Pampa fauna, which in result are not one whit behind, as has been proved by the recognition of a whole order of primitive ungulates, the Toxodontia, by that of toothed cetaceans with elongated nasals, as in the genera *Prosqalodon* and *Argyrosetus*, and of sperm whales with functional premaxillary teeth, viz., *Physodon* and *Hypocetus*, to say nothing of giant armadillos and pigmy glyptodonts.

It will be remembered by some present that, from Patagonian deposits of supposed Cretaceous age, there was exhibited at our Dover meeting the skull of a horned chelonian *Meiolania*, which animal, we were informed, is barely distinguishable from the species originally discovered in Cook's Island, one of the Society group, and which, being a marsh turtle highly specialised, would seem in all probability to furnish a forcible defence for the theory of the antarctic continent. But more than this, the results of renewed investigation of the Argentine beds by the members of the Princeton University of North America have recently resulted in collections which, we are informed, seem likely to surpass all precedent in their bearings upon our current ideas, not the least remarkable preliminary announcement being the statement that there occurs fossil a mole indistinguishable, so far as is known, from the golden mole (*Chrysochloris*) of South Africa.

Before I dismiss this fascinating subject let me disarm the notion, which may have arisen, that the palæontological work of the old world is done. Far from it! Even our American cousins have to come to us for important fossil forms; as, for example, the genus *Pliohyrax* of Samos and the Egyptian desert, while among the rodents and smaller carnivores there are large collections in our national museum waiting to be worked over afresh.

If one part of the globe more than another is just now the centre of interest concerning its vertebrate remains, it is the Egyptian desert. Here there have

recently been found the bones of a huge cetacean associated, as in South America, with those of a giant snake, one of the longest known, since it must have reached a length of thirty feet. There also occur the remains of other snakes, of chelonians of remarkable adaptive type, of crocodilians, fishes, and other animals. Interest, however, is greatest concerning the Mammalia, which for novelty are quite up to the American standard, as with an upper and a lower jaw of an anomalous creature, concerning which we can only at present remark that it may be a marsupial, or more probably a carnivore, which has taken on the rodent type in a manner peculiarly its own. Important beyond this, however, are a series of Eocene forms which more than fill a long-standing gap, viz., that of the ancestors of the Elephants and Mastodons, which hitherto stopped short in the Middle Miocene of both old and new worlds. As represented by the genus *Mærittherium*, they have three incisors above and two below, of which the second is in each case converted into a short but massive tusk. An upper canine is present, and in both upper and lower jaws a series of six cheek-teeth, distinct and bunodont in type. In the allied *Barytherium*, of which a large part of the skeleton is known, the upper incisors were presumably reduced to two, the tusks enlarged, with resemblances in detail to the Dinoceratan type.

So far as these remains are known, they appear to present in their combined characters all that the most ardent evolutionist could desire. There are with them Mastodons which simplify our knowledge of this group; and among the last discovered remains Sirenians, which, in presenting a certain similarity to the afore-named *Mærittherium*, strengthen the belief in the proboscidian relationships of these aquatic forms. Finally, and perhaps most noticeable of all, there is the genus *Arsinoitherium*, a heavy brute with an olfactory vacuity which outrivals that of *Grypottherium* itself, and is surmounted by a monstrous fronto-nasal horn, swollen and bifid, for which the most formidable among the Titanotheres might yearn in vain. There is an occiput to match! The suggestion that this extraordinary beast has relationships with the Rhinocerotidae is absurd, since its tooth pattern alone inverts the order of this type. That it is proboscidian may be nearer the mark, and if so it shows once more how subtle were the mammals of the past. Great as is this result, much remains to be done or done again, if only from the fact that in seeking to determine homologies our American brethren, in the opinion of some of us, have placed too much reliance on a so-called tritubercular theory of tooth genesis, of which we cannot admit the proof. How, we would ask, is it conceivable that a transversely ridged molar of the *Diprotodon* type can be of tritubercular origin?

Sufficient for the moment of palæontological advance, except to remark that the zoologist who neglects this branch of morphology misses the one leavening influence; neglects the court on whose ruling arguments deduced from embryological data alone must either stand or fall. We may form our own conclusions from facts of the order before us; but it is when we find their influence on the master-mind prompting to action, like that of Huxley with his mighty memoir of 1880, in which he revised our sub-class terms, that we appreciate them to the full.

With this consideration we pass to the living forms, and I have only time in dealing with these to comment on advance which affects our broadest conceptions and classifications of the past.

To commence with the Mammalia, we now know that the mammary gland when first it appears is in all forms tubular, and that this type is no longer distinctive of the Monotremata alone. We know, too, that the intranarial position of the epiglottis when at rest, long known for certain forms, is a distinction of the class. It explains the presence of the velum palatinum, by its association with the glottis for the restriction of the respiratory passage, the connection being lost in man alone, under specialisation of the organ of the voice.

Similarly, the doubly ossified condition of the coracoid may now be held diagnostic, for it is known that the epicoracoidal element, originally thought to characterise the monotremes alone, is always present, and that reduction to a varying degree characterises the metacoracoid, which retires, as in man, as the so-called coracoid epiphysis.

Our conceptions of the interrelationships of the Marsupialia and Placentalia have during the period we are considering been delimited beyond expectation, by the discovery of an allantoic placenta in a polyprotodont marsupial, in place of the vitelline, present in its allies. When it is remembered that in the formation of the placenta of the rabbit and a bat there is realised a provisional vitelline stage, it is tempting to suggest that the evidence for the direct relationship of the two mammalian sub-classes first named overlaps (there being a placental marsupial on one hand, a marsupial placental on the other), much as we have come to regard Archæopteryx as an avian reptile, the Odontornithes as reptilian birds. These facts, moreover, prove that the type of placenta inherited by the Placentalia must have been discoidal, and that from that all others were derived.

Equally important concerning our knowledge of the Marsupialia is the discovery, first made clear by Professor Symington, of this College, that Owen was correct in denying them a corpus callosum. How Owen arrived at this conclusion it is difficult to conceive; but in these later days the history of discovery is largely that of method; and it is by the employment of chrome-silver, methylene-blue, and other reagents, which in differentiating the fibre-tracts enable us to delimit their course, that this conclusion has been proved. By the corpus callosum we now understand a series of neo-pallial fibres which transect the alveus and are present only in the Placentalia.

There is no department of mammalogy in which recent work has been more luminous than this which concerns the brain; and, to mention but one result, it may be said that in the renewed study of the commissures there has been found a fibre-tract characteristic of the Diprotodontia alone, so situated as to prove that they and the Placentalia must have specialised on diverse lines from a polyprotodont stock. Interesting this, the more, since the phalangers and kangaroos are known to be polyprotodont when young. And when we add the discovery that in the detailed relationship of its commissures the brain of the Elephant Shrew, a lowly insectivore, alone among that of all Placentalia known realises the marsupial state, as does its accessory organ of smell, we have to admit the discovery of annectant conditions just where they should occur.

The morphological method is sound!

The master hand which has given us this result has also reinvestigated the Lemurs. From an exhaustive study of the brain or its cast of all species of the order, living and extinct, there has come the proof that the distinctive characters of the lemuroid brain are intelligible only on a knowledge of the pithecoïd type; that its structural simplicity in the so-called lower lemurs is due to retrogressive change, in some species proved to be ontogenetic; and that the Tarsier, recently claimed to be an insectivore, is a lemur of lemurs. It is impossible to overestimate the importance of this conclusion, which receives confirmation in recent palæontological work; and there is demanded a reinvestigation of those early described Tertiary fossil forms placed on the Ungulo-lemuroid border line, as also a reconsideration of current views on the evolution of the primates and of man.

In dismissing the Mammalia, we recall the capture during the period we review of three new genera, a fourth, the so-called *Neomylodon*, having proved by its skull to be *Gypotherium Darwinii*, already known. The African Okapi, an object of sensation beyond its deserts, has found its place at last. To have been dubbed a donkey, a zebra, and a primitive hornless giraffe, is distinction indeed; and we cannot refrain from contrasting the nonsensical statement that its discovery is 'the most important since Archæopteryx' with the truth that it is a giraffine, horned for both sexes, annectant between two groups well known. As a discovery it does not compare with that of the Mole-marsupial, and it falls into insignificance beside that of the South American diprotodont *Cænolestes*, the survivor of a family which there flourished in Middle Tertiary times.

Passing to Birds and Reptiles, it will be convenient to consider them together. A knowledge of their anatomy has extended on all hands, and in respect to nothing more instructively than their organs of respiration. Surprise must be expressed at the discovery, in the chelonian, of a mode of advancing complication of the lung suggestive of that of birds. On looking into this, I find that Huxley, who

rationalised our knowledge of the avian lung and its sacs, was aware of the fact that in our common Water-tortoise (*Emys orbicularis*), the lung is sharply differentiated along the bronchial line into a postero-dorsal more cellular mass, an antero-ventral more saccular, of which the posterior vesicle, in its extension and bronchial relationships, strangely simulates the so-called abdominal sac of birds. He had already instituted comparison with the Crocodiles, and was clearly coming to the conclusion that the arrangement in the bird is but the result of extreme specialisation of a type common to all Sauropsida with a 'cellular' lung. The respiratory process in the bird may be defined as *transpulmonary*, and it is an interesting coincidence that, as I write, there comes to hand a memoir, supporting Huxley's conclusion, and establishing the fact that there is a fundamental principle underlying the development and primary differentiation of all types of vertebrate lung.

The discovery of the Odontornithes in the American Cretaceous is so well known, that it is but necessary to remark that nine genera and some twenty species are recognised. To *Archæopteryx* I shall return. Before dismissing the *Chelonia*, however, it must be pointed out that palæontology has definitely clenched their supposed relationship to the Plesiosaurs. Of all recent palæontological collections there are none which, for care in collecting and skill in mounting, surpass the reptilian remains from the English Jurassic (Oxford Clay) now public in our national museum. The Plesiosaurs of this series must be seen to be appreciated, and nothing short of a merciful Providence can have interposed, to ensure the generic name *Cryptocleidus*, which one of them has received, since the hiding of the clavicle, its diagnostic character, is an accomplished fact. It is due to secondary displacement, under the approximation in the middle line of a pair of proscapular lobes, present in the Plesiosauria and *Chelonia* alone, and until the advent of this discovery misinterpreted. Taken in conjunction with other characters of little less importance, conspicuously those of the plastron and pelvis, this decides the question of affinity, and proves the *Chelonia* to have had a lowly ancestry, as has generally been maintained.

Recent research has fully recorded the facts of development of the rare New Zealand reptile *Sphenodon*, and it has more than justified the conclusion that it is the sole survivor of an originally extensive and primitive group, the Rhynchocephalia, as now understood. To confine our attention to its skeleton, as that portion of its body which can alone be compared with both the living and extinct, it may be said that positive proof has been for the first time obtained that the developing vertebral body of the terrestrial vertebrata passes through a paired cartilaginous stage, and that in its details the later development of this body is most nearly identical with that of the lower Batrachia. There has long been a consensus of opinion that the forward extension of the pterygoids to meet the vomers in the middle line, known hitherto in this animal and the crocodiles alone, is for the terrestrial Vertebrata a primitive character; and proof of this has been obtained by its presence in all the Rhynchocephalia known. The same condition has also been found to exist in the Plesiosaurs, the Ichthyosaurs, the Pterodactyles, the Dicynodontia, the Dinosaurs, and with modification in some Chelonians. It has, moreover, been found in living birds; a most welcome fact, since *Archæopteryx*, in the possession of a plastron, carries the avian type a stage lower than the Dinosaurs. It is pertinent here to remark that, inasmuch as in those Dinosaurs (e.g., *Compsognathus*) in which the characters of the hind limbs are most nearly avian, the pelvis, in respect to its pubis, is at the antipodes of that of all known birds, and the fore limb is shortened in excess of that of *Archæopteryx* itself, the long supposed dinosaurian ancestry for birds must be held in abeyance.

Passing through the Rhynchocephalia to the Batrachia, we have to countenance progress most definite in its results. The skull, the limbs and their girdles, are chiefly concerned, and this in a very remarkable way.

In the year 1881 there was made known by Professor Froriep, of Tübingen, the discovery that the hypoglossus nerve of the embryo mammal is possessed of dorsal ganglionated roots. Again and again have I heard Huxley insist on the fact that the ventral roots of this nerve are serial with the spinal set, but never did

he suspect the rest. It is, however, a most intensely interesting fact that, whereas by a Huxleian triumph the vertebral theory of the skull was overthrown, in these later Huxleian days the proof of the incorporation of a portion of the vertebral region of the trunk into the mammalian occiput should have marked the succeeding epoch in advance. The existence of twelve pairs of cranial nerves which all the Amniota possess involves them in this change; and the fact that in all Batrachia there are but ten, enables us to draw a hard-and-fast line between batrachian and amniote series.

It may be urged, as an objection, that since we have long been familiar with a fusion of vertebræ and skull in various piscine forms, the force of this distinction is weakened. But this cannot be; since, in respect to the investing sheaths and processes of development which lie at the root of the genesis of the vertebral skeleton, the fishes stand distinct from the Batrachia and Amniota, which are agreed. So forcible is this consideration that it behoves us to express it in words, and I have elsewhere proposed to discriminate between the series of terrestrial Vertebrata as *archæ-* and *syn-craniate*.

Similarly there is no proof that any batrachian, living or extinct (and in this I include the Stegocephala as a whole), possesses a costal sternum. So far as their development is known, the cartilages in these animals called 'sternal' are either coracoidal or *sui generis*. The costal sternum, like the syncraniate skull, is distinctive of the Amniota alone. Had the Stegocephala possessed it even in cartilage, there is reason to think it might have been preserved, as it has been in the colossal Mososaur *Tylosaurus* of the American Cretaceous. When to this it is added that whereas, in the presence of a costal sternum, the mechanism of inflation of the lung involves the body-wall, in its absence it mainly involves the mouth (as in all fishes and batrachians), the hard and sharp line between the Batrachia and Amniota may be expressed by the formula that the former are *archæcraniate* and *stomatophysous*, the latter *syncraniate* and *somatophysous*.

There are allied topics which might be considered did our time permit; but one certain outcome of this is that there is an end to the notion of a batrachian ancestry for the Mammalia. And when, on this basis, we sum up the characters demanded of the stock from which the Mammalia have been derived, we find them to be precisely those occurring outside the Mammalia in the Anomodont Reptiles alone. Beyond the sternum and skull, the chief characters are the possession of short and equal pentadactyle limbs, with never more than three phalanges to a digit, a complete fibula and clavicle, a doubly ossified coracoid, a heterodont dentition—a combination which, wholly or in part, we now associate with the Permian genera *Procolophon*, *Pariasaurus*, and others which might be named, the discovery of which constitutes one of the morphological triumphs of our time.

Beyond this, it may be added, concerning the Batrachia, that among living pedate forms the Anura have alone retained the pentadactyle state and the complete maxillo-jugal arch, and that the Eastern *Tylotriton*, in the possession of the latter, becomes the least modified urodele extant. These facts lead to the extraordinary conclusion that the living Urodela, while of general lowly organisation, are one and all aberrant; and it is not the least important sequel to this that, despite their total loss of limbs, the Apoda, in the retention of the dermal armour and other features which might be stated are the most primitive Batrachia that exist.

The batrachian phalangeal formula 22343 was until quite recently a difficulty in the determination of the precise zoological position of the class; but it has now been overcome, by the discovery of a *Keraterpeton* in the Irish Carboniferous having three phalanges on the second digit of both fore and hind limbs, and by that in the Permian of Saxony of a most remarkable creature, *Sclerocephalus*, which, if rightly referred to the Stegocephala, had a head encased, as its name implies, in an armature like that of a fish, and the phalangeal formula of a reptile, 23454.

Passing from the Batrachia to the Fishes, we have still to admit a gap, since an interminable discussion on fingers and fins has not narrowed it in the least. In compensation for this, however, we have to record—within the fish series itself

progress greater, perhaps, than with the higher groups. Certainly is this the case if, as to bulk, the literature in systematics and palæontology be alone taken into account.

Of the Dipnoi our knowledge is fast becoming complete. We know that *Lepidosiren* forms a burrow; and, in consideration of a former monstrous proposal to regard this animal, with its fifty-six pairs of ribs, and *Protopterus*, with its thirty to thirty-five, as varieties of a species, it is the more interesting to find that the Congo has lately yielded a *Protopterus* (*P. Dolloi*) with the lepidosiren rib formula, viz., fifty-four pairs.

As a foremost result of American palæontological research we have to record the occurrence, in the Devonian of Ohio, of a series of colossal fishes known as the Arthrodira, the supposed dipnoan affinities of which are still a matter of doubt.

We have evidence that the osseous skeleton in a plate-like form first appeared as a protection for the eye of a primitive shark. And coming to recent forms having special bearings on the teachings of the rocks, we have to acknowledge the capture in the Japanese seas of a couple of ancient sharks, of which one (*Cladoselachus*), since observed to have a distribution extending to the far North, is a survivor from Devonian times; the other (*Mitsukurina*), a genus whose grotesqueness leaves no doubt of its identity with the Cretaceous lamnoid *Scapanorhynchus*. In the elucidation of the Sturiones and the determination of their affinities with the ancient Palæoniscidæ a master stroke has been achieved. In the Old Red genus *Palæospondylus* we have become familiar with an unmistakable marsipobranch, possessing, as do certain living fishes, a notochord, annulated, but not vertebrated in the strict sense of the term. The climax in Ichthyopalæontology, however, has been reached, in the discovery of Silurian forms, which, there is every reason to believe, explain in an unexpected way the hitherto anomalous Pteras- and Cephalaspidians, by involving them in a community of ancestry with the primitive Elasmobranchs. The genera *Thelodus*, *Drepanaspis*, *Ateleaspis*, and *Lanarkia*, chief among these annectant and ancestral forms, are among the most remarkable vertebrate fossils known.

Passing to the Recent Fishes alone, the discovery which must take precedence is that of the mode of origin of the skeletogenous tissue of their vertebral column. The fishes, unlike all the higher Vertebrata, have, when young, a notochord invested in a double sheath, there being an inner chordal sheath, an outer cuticular, which latter is alone present in all the higher groups. The skeletogenous cells, by whose activity the cartilaginous vertebral skeleton is formed, arise outside these sheaths; but whereas, when proliferating, they in one series remain outside, they in the other, by the rupture of the cuticular sheath, invade the chordal. This distinction enables us to discriminate between a *Chordal series*, which embraces the Chimæroids, Elasmobranchs, and Dipnoi, and a *Perichordal*, consisting of the Teleosts, Ganoids, and Cyclostomes.

In consideration of the enormity of the structural gap between the cyclostomes and the higher Vertebrata this is an extraordinary result. For be it remembered that, in addition to their well-known characters, the lampreys and hags (1) in the total absence of paired fins; (2) in the presence of branchiæ, ordinarily seven in number, fourteen in *Bdellostoma polytrema*, numerically variable in individuals of certain species between six and fourteen, and doubtfully asserted in the young of one to be originally thirty-five; and (3) in the carrying up of their oral hypophysis by the nasal organ, whereby it perforates the cranium from above, as contrasted with all the higher Vertebrata, in which, carried in with the mouth-sac, it perforates it from beneath, exhibit morphological characters of an extraordinary kind. And if we are to express these characters in terms, we may distinguish the Cyclostomes as *apterygial* and *epicraniate*, the higher Vertebrata as *hypocraniate*.¹ But this notwithstanding, the aforementioned subdivision of the

¹ It is an interesting circumstance, if their 'ciliated sac' is rightly homologised, that *Amphioxus* and the *Tunicata* present a corresponding dissimilarity, allowance being made for the fact that in *Botryllus*, *Goodsiria*, and *Polysarpa* the sac overlies the ganglion. It is pertinent here to recall the ammocete-like condition of the 'endostyle' in *Oikopleura labellum*.

Pisces into two series, which would associate the teleosts and ganoids with the cyclostomes, as distinct from the rest, receives support from recent study of the head-kidney by a Japanese, who seeks to show that the organ so called in the Elasmobranchs is of a late-formed type peculiar to itself; and it is also in agreement with one set of conclusions previously deduced from the study of the reproductive organs.

To deal further with the fishes is impossible in this Address, except to remark that recent discovery in the Gambia that the young of the Teleostean genera *Heterotis* and *Gymnarchus* bear filamentous external gills, renders significant beyond expectation the alleged presence of these among the loaches, and shows that adaptive organs of this type are valueless as criteria of affinity.

In palæontology, as in recent anatomy, our records of detail have increased beyond precedent, often but to show how deficient in knowledge we are, how contradictory are our theories and facts.

In dismissing the fishes, I wish to comment upon our accepted terms of orientation. To speak of the median fins as dorsal, caudal, and anal, of the pelvic as ventral, and of the pectoral in its varying degrees of forward translocation as abdominal or thoracic, though a convention of the past, is to-day inaccurate and absurd. I question if the time has not come at which the terms thoracic (pulmo-cardiac) and abdominal are intolerable, as expressing either the subdivisions of the body-cavity or anything else, outside the Mammalia, which alone possess a diaphragm. Even in the birds, to grant the utmost, the subdivision of the coelom if accurately described, must be into pulmonary, hyper-pulmonary, and cardio-abdominal chambers; while with the reptiles the modes of subdivision are so complex that a special terminology is necessary for each of the several types extant.

In the fishes, where the pericardium is alone shut off, the retention of the mammalian terms but hampers progress. This was indeed felt by Duméril, when in 1865 he attempted a revisionary scheme. Since, however, one less fantastic than his seems desirable, I would propose that for the future the 'anal' fin be termed *ventral*, the 'ventral' *pelvic*; and that for the several positions of the pelvic, that immediately in front of the vent, primitive and embryonic (which is the position for the Elasmobranchs, Sturiones, Lower Siluroids, and all the higher Vertebrata), be termed *proctal*, the so-called 'abdominal' *pro-proctal*, the so-called 'thoracic' *jugular* (in that it denotes association with the area of the 'collar-bone'), and the so-called 'jugular' *mental*. The necessity for this becomes the more desirable, now that it is known that a group of Cretaceous fishes (the Ctenothrissidæ), hitherto regarded as Berycoids, are in reality of clupeoid affinity, despite the fact that at this early geologic period they had translocated their pelvic fin into the jugular ('thoracic') position.

The sum of our knowledge acquired during the last twenty-eight years proves to us that, among the bony fishes, the structural combination which would give us a premaxillo-maxillary gape dentigerous throughout, a proctal pelvic fin, a heart with conal valves, would be the lowest and most primitive. Inasmuch as this character of the heart, so far as at present known, exists only among the Clupeoeces (pikes and herrings and their immediate allies), these must be regarded as lowly forms; wherefore it follows that the possession of but a single dorsal fin is not, as might appear, a necessary index of a highly modified state.

Before I dismiss the vertebrates, a word or two upon a recent result of morphological inquiry which concerns them as a whole. I refer to the development of the skull. Up to 1878 it was everywhere thought and taught that the cartilaginous skull was a compound of paired elements, known as the trabeculæ cranii and parachordals, and that the former contributed the cranial wall. Huxley in 1874, from the study of the cranial nerves of fishes, had reiterated the suggestion he made in 1864, when dealing with the skull alone, that the trabeculæ might be a pair of præ-oral visceral arches, serial with those which support the mouth and carry the gills. The next step lay with the Sturgeon, in which in 1878 it was found that the cranial wall is originally distinct. And later, when the facts were more fully studied in sharks, batrachians, reptiles, and birds, it became evident that the trabeculæ, though ultimately associated with the cranial wall,

take no share in its formation, and that when first they appear they are disposed at right angles to the parachordals and the axis, serially with the visceral arches behind. Huxley was right; and although this consideration by no means exhausts the category of independent cartilages now known to contribute to the formation of the skull, it proves that the cartilaginous cranium, like the bony one, which in the higher vertebrate forms replaces it, is in its essence compound.

I now pass to the Invertebrata. Of the Oligochæta and Leeches I have spoken, and we may next consider the Arthropods. Of the Insecta, our knowledge has gained precision, by the conclusion that the primitive number of their Malpighian tubes is six, and by the study of development of these in the American cockroach *Doryphora*, which has rendered it probable they may be modified nephridia, carried in as are those of some oligochætes with the proctodeal invagination. An apparent cervical placenta has been discovered in the orthopteran *Hemimerus*, which would seem to suggest homology with the so-called 'trophic vesicle' of the Peripatoids, as exemplified by *P. Nova-Britannica*. In this same orthopteran there have been recognised, in secondary proximity to the 'lingua,' reduced maxillulæ, which, fully developed and interposed between the mandible and first maxilla, in *Japyx*, *Machilis*, *Forficula*, and the *Ephemera* larva, give us a fifth constituent for the insectan head. And when it is found that all the abdominal segments of the common cockroach, when young, are said to bear appendages, of which the cerci are the hindermost, we have a series of facts which revolutionise our ideas. Little less striking is the discovery that in the caterpillar of the bombycine genera *Lagoa* and *Chrysopyga* seven pairs of pro-legs occur.

The fuller study of the apertures of the tracheate body has resulted in the discovery that the Chilopoda are more nearly related to the Hexapoda than to the Diplopoda; wherefore it is proposed to reclassify the Tracheata, in accordance with the position of the genital orifice, into *Pro-* and *Opistho-gonata*. In a word the 'Myriapoda,' if a natural group, are diphyletic.

Our knowledge of the Peripatoids (Arthropoda malacopoda) has increased in all that concerns distribution and structure. They are now known, for example, from Africa, the West Indies, Australia, and New Zealand, and for examples from the two latter localities and Tasmania the generic name *Ooperipatus* has but lately been proposed, to include three species, characterised by the possession of an ovipositor, of which two have been observed to lay eggs.

Work upon the Crustacea in our own land, notorious for the tendencies of some of its devotees in their stickling for priority, has within the last twelve years advanced beyond all expectation. Much of our literature has been systematised, and an enormous increase in our knowledge of new forms has to be admitted, thanks to memoirs such as those of the 'Investigator,' 'Naples Zoological Station,' and others which might be named; while in the discovery and successful monographing, in the intervals of six years' labour at other groups, of a new family of minute Copepods (the Choniostomatidæ), parasitic on the Malacostraca, embracing forty-three species, difficult to find, we have an almost unique achievement. The hand which gave us this has also provided a report which embraces the description of a nauplius of exceptional type, which, by a process of reasoning by elimination, masterly in its method, has been 'run to ground' as in every degree of probability the larva of Darwin's apodal barnacle *Protolepas biveneta*, of which only the original specimen is known.

There is but one other crustacean record equal in rank with this, viz., the discovery of the genus *Anaspides*. Originally obtained from a fresh-water pool on Mount Wellington, Tasmania, at 4,000 feet, it has since been found in two other localities. It is unique among all living forms, in combining within itself characters of at least three distinct sub-orders of 'prawns,' for with a schizopod body it combines the double epipodial lamellæ of an amphipod, the head of a decapod (pedunculated eyes and antennular statocysts) apart from characters peculiarly its own. There is reason to believe that the nearest living ally to this remarkable creature is a small eyeless species (*Bathynella natana*) obtained from a Bohemian well; and if its presumed relationships to the Palæozoic 'pod-shrimps' be correct, this heterogeneous assemblage may perhaps be

the representatives of a group of primitive Malacostraca, through which, by structural divergence, the establishment of the higher crustacean sub-orders may have come about.

It is pertinent to this to note that work upon cave-dwelling and terrestrial forms, upon 'well-shrimps' and the like, has produced important results. And interesting indeed is the recent discovery of three species, living at 800-900 feet above sea-level, in Gippsland, one an amphipod, two of them isopods, which, though surface-dwellers, are all blind. While they prove to be species of genera normally eyed, they in their characters agree with well-known American forms; and the bleaching of their bodies and atrophy of their eyes proclaim them the descendants of cave-dwelling or subterranean ancestors, among whom the atrophy took place.

Huxley in 1880 rationalised our treatment of the higher Crustacea, by devising a classification by gills, expressive of the relationships of these to the limb-bases, interarticular membranes, and body-wall. Hardly had his influence taken effect when, by work extending over the years 1886 to 1893, in the study of Penæus, the Phyllopods, Ostracods, and other forms, evidence had been accumulating to show that the crustacean appendage, even to the mandible itself, has primarily a basal constituent (protopodite) of three segments; that the branchiæ one and all are originally appendicular in origin; and that the numerical reduction of the basal (protopoditic) segments to two, with the assumption of a non-appendicular relationship by the gills, is due to coalescence of parts, with or without suppression. The evidence for this epoch-making conclusion, which simplifies our conceptions and brings contradictory data into line, is as irresistible as it is important, and there has been nothing finer in the whole history of crustacean morphology. With it, the attempt to explain the supposed anomalous characters of the antennule by appeal to embryology goes to the wall; and, taking a deep breath, we view the Crustacea in a new light.

There remains for brief consideration one carcinological discovery second to none which bear on the significance of larval forms. It is that of the Trilobite *Triarthrus Becki*, obtained in abundance from the Lower Silurian near New York, with all its limbs preserved. In the simplicity of its segmentation and the biramous condition of its limbs it is primitive to a degree. Chief among its characters are the total absence of jaws in the strict sense of the term, and the fact that of its three anterior pairs of appendages the third is certainly and the second is apparently biramous, the first uniramous and antenniform. In this we have a combination of characters known only in the nauplius larva among all living crustacean forms; and the conclusion that the adult trilobite, like that of the Euphausiacea, Sergestidae, Penæidae, the Ostracods, and Cirripedes of to-day, was derived by direct expansion of the nauplius larva can hardly be doubted. Much yet remains to be done with the study of the *Triarthrus* limbs; and the suggestion of a foliaceous condition by those of the pygidium, which are the youngest, is a remarkable fact, the meaning of which the future must decide. We should expect the condition to be a provisional one, since while we admit the primitive nature of the phyllopods as an Order, we cannot regard the foliation of their appendages as anything but a specialisation. Be this as it may, the structural community between the nauplius larva and the trilobite is now proved; and when we add that in the yolk-bearing higher Crustacean types (e.g., *Astacus*) a perceptible halt in the development may be observed at the three-limb-bearing stage; that in *Mysis* the vitelline membrane is shed but to make way for a nauplius cuticle; and that the median nauplius eye has long been found sessile on the adult brain of representative members of the higher crustacean groups, up to the lobster itself, our belief in the ancestral significance of the nauplius larval form is established beyond doubt.

The thought of the nauplius suggests other larval forms. The gastrula is no longer accepted without reserve; the claims of the blastula, planula, parenchymella, not to say the plakula, have all to be borne in mind. It is of the Trochophore, however, as familiar as the nauplius, that I would rather speak, as influenced by recent research. It is supposed to be primitive for the molluscs and chætopod worms at least; and various attempts have been made to bolster it up,

and to show that if we allow for adaptive change, its characters, well known, are constant within the limits of its simpler forms.

It is now more than forty years ago that the late Lacaze-Duthiers described for *Dentalium* a larval stage, characterised by the possession of recurrently ciliated zones, which by reduction, with union and translocation forwards, give rise to the trochal lobe. It is now known that in the American pelecypod *Yoldia limatula* a similar stage is found, in which a 'test,' of five rows of ciliated cells, is present; and of the young of *Dondersia banyulensis* the like is true. But whereas in the *Yoldia* the ciliated sac is ultimately shed, in the Myzomenian the escape of the embryo is accompanied by rupture, which liberates the anterior series of ciliated zones in a manner strongly suggestive of forward concentration, leaving the posterior circlet with its cilia attached.

This 'test' has also been seen in two species of *Nucula*, and pending fuller inquiry into the Myzomenian and a reinvestigation of *Dentalium*, I would suggest that this recurrently ciliated sac is representative of a larval stage antecedent to the trochophore, for which the term *protrochal* may suffice. This term has indeed been already applied to a larva of certain Polychæta, which might well represent a modification of that for which I am arguing; and quite recently it appears to have been observed near Ceylon for a species of the genus *Marphysa*.

The discovery of this larva in *Dondersia* was accompanied by that of a later-formed series of dorsal spicular plates, which for once and for all, in realising a chitonid stage, demolish the heresy of the 'Solenogastres,' mischievous as suggesting an affinity with the worms. Like that of the supposed cephalopod affinities of the so-called 'Pteropods,' it must be ignored as an error of the past.

Returning to the protrochal stage, whatever the future may reveal concerning it, by bringing together the Lamellibranchiata, Scaphopoda, and Polyplacophora, it associates in one natural series all the bilaterally symmetrical Mollusca except the cephalopods. In doing this, it deals the death-blow to the supposed Rhipidoglossan affinity of the Lamellibranchiata; and in support of this conclusion I would point out that the recently discovered eyes of the mytilids are in the position of those of the embryo *Chiton*, and that just as *Dentalium*, in the formation of its mantle, passes through a lamellibranchiate stage, so are there lamellibranchs in number in which a tubular investment is found.

This protrochal larva has an important part to play. It may very possibly explain phenomena such as the compound nature of the trochal lobe of the limpet, the presence of a post-oral ciliated band in the larva of the ship-worm, and of a præ-anal one in that of various molluscan forms. In view of it, we must hesitate before we fully accept the belief in the ancestral significance of the trochophore. And it is certain that an idea, at one time entertained, that the Rotifer (*Trochosphaera*) which so closely resembles it as to bear its name, is its persistent representative, is wrong, since this is now known to be but the female of a species having a very ordinary male.

Through the Rhipidoglossa we pass to the Gastropods, which are one and all asymmetrical, for even *Fissurella*, *Patella*, and *Doris*, when young, develop a spiral shell; while Huxley in 1877 had observed that the shell of *Aplysia*, in its asymmetry, betrays its spiral source.

The notion, which until recently prevailed, that among these gastropods the non-twisted or so-called euthyneurous condition of the visceral nerve-cords, as exemplified by the Opisthobranchs, is a direct derivative of that of the Chitons have been proved to be erroneous, since the nerves in *Actæon* and *Chilina*, like those of the prosobranchs, are twisted or streptoneurous. And as to the torsion of the gastropod body, recent research, in which one of my pupils has played a part, involving the discovery of paired reno-pericardial apertures in *Haliotis*, *Patella*, and *Trochus*, has resulted in proof that the dextral torsion which leads to the monotocardiac condition, does not uniformly affect all organs lying primitively to the left of the rectum, as we have been taught; since, concerning the renal organs, it is the *primitively* (pretorsional) left one which remains as the functional kidney, its ostium as the genital aperture. Nor is the primitively right kidney necessarily lost, for while its ostium remains as the renal orifice, its body, by modification and

reduction, may become an appendage of the functional kidney, the so-called nephridial gland. And we now know there are cases of sinistral torsion of the visceral hump, in which the order of suppression of the organs is not reversed, the arrangement being one of adaptation of a dextral organisation to a sinistral shell.

Though thus specialised and asymmetrical as a group, the gastropods are yet plastic to an unexpected degree. Madagascar has yielded a *Physa* (*P. lamellata*) with a neomorphic gill, a character shared by species of *Planorbis* (*P. corneus* and *P. marginatus*), and an *Ancylus* in which the lung-sac is suppressed; while St. Thomas's Island has given us a snail (*Thyrophorella Thomensis*), the peristome of whose shell is produced into a protective lid.

In palæontology, history records the fact that in 1864 Huxley observed that the genus *Belemnites* appears to have borne but six free arms; a startling discovery which lay dormant till the present year. And the recent study of the fauna of the great African lakes, in bringing to light the existence of a halolimnic molluscan series in Lake Tanganyika, has opened up new possibilities concerning the palæontological resources of enormous aqueous deposits, recently discovered in the interior, and has entirely changed our geological conceptions of the nature of Equatorial Africa.

Time prevents my dealing with other groups, and it must suffice to say that with those I have not considered substantial work has been done. From what has been said, it is natural to expect that in some direction or another so vast an accumulation of facts must have extended the Darwinian teaching; and it is now quite clear that this has been the case with the two post-Darwinian principles known as 'Substitution' and Isomorphism or 'Convergence.'

The former may be exemplified by nothing better than the case of the Rays and Skates, in which, under the usurpation of the propelling function of the tail by the expanded pectoral fins, the tail, free to modify, becomes in one species a lengthy whiplash, in another a vestigial stump, in others, by the development of powerful spines, a formidable organ of defence. In both the Rays and certain other fishes subject to the working of this law, modification goes further still, in the appearance of electric organs in remotely related genera and species, by specialisation of the muscular system of the trunk or tail, or, as in the case of *Malapterurus*, of 'tegumental glands.' In this we have a difficulty admitted by Darwin himself, which now becomes clear and intelligible, since there is nothing new. There has simply come about the conversion, in one case of the energy of muscular contraction, in the other of glandular secretion, into that of electrical discharge, with accompanying structural change. The blind locust (*Pachyrhina fuscifer*) of the New Zealand Limestone caves presents an allied case, since here, under the reduction of the eye, the antennæ, elongated to a remarkable degree, have become the more efficiently tactile; and it is an interesting question whether this principle may not explain the attenuation of the limbs in the recently discovered American Proteoid (*Typhlomolga Rathburni*) of the Texan subterranean waters.

And as to isomorphism, by which we mean the assumption of a similar structural state by members of diverse or independent groups, I would recall the case of the Eocene Creodont *Patriofelis* and the Seals, and that of the Myriapods to which I have already alluded, and would cite that of the Dinosaurs and Birds, heterodox though it may appear, for reasons I have given.

As our knowledge increases, there is every reason to believe that, in the non-appreciation of these principles in the past, not a few of our classifications are wrong. We have even had our bogies, as, for example, the so-called *Physemaria*, which deceived the very elect; and before I close I wish to deal briefly with a question of serious doubt, which these considerations suggest.

It is that of the position in the zoological series of the Limuloids, popularly termed the King Crabs. These creatures, best known from the opposite shores of the Northern Pacific, but found in the oriental seas as well as far south as Torres Strait, have been since 1829 the subject of a difference of opinion as to their zoological position and affinities. Within the last twenty years there have been

three determined advances upon them, and of these the third and most recent may be first discussed. It has for its object the attempt to prove that they are intimately associated with the cephalaspidian and other shield-bearing fishes of the Devonian and Silurian epochs, and that through them they are ancestral to the Vertebrata. The latest phase of this idea is based on the supposed existence in a *Cephalaspis* of a series of twenty-five to thirty lateral appendages of arthropod type. When, however, it is found that the would-be limbs are but the edges of body-scutes misinterpreted, suspicion is aroused; and when, working back from this, an earlier attempt reveals the fact that the author, compelled to find trabeculæ, in order to force a presupposed comparison between the architecture of the Cephalaspidian head-shield and the *Limulus*' prosomal hood, resorts to a comparison between the structure of the former in general and that of the cornu of the latter, with details which on the piscine side are not to date, the argument must be condemned. It violates the first principles of comparative morphology, and is revolting to common sense; and as to the fishes concerned, we know that they have nothing whatever to do with the Limuloids, for we have already seen that, with their allies the Pteraspidiæ, they are a lateral branch of the ancestral piscine stem.

The second advance upon the king crabs has very much in common with the first. It has engrossed the attention of an eminent physiologist for the last six or seven years, and by him it was in detail set before Section I at our meeting of 1896. Suffice it to say that it specially aims at establishing a structural community between the king crabs and certain vertebrates, favourable to the conviction that the Vertebrata have had an arthropod ancestry. When we critically survey the appalling accumulation of words begotten of this task, it is sufficient to consider its opening and closing phases. At the outset, under the conclusion that the vertebrate nervous axis is the metamorphosed alimentary canal of the arthropod ancestor, the necessity for finding a digestive gland is mainly met by homologising the so-called liver of the arthropod with the cellular arachnoid of the larval lamprey, in violation of the first principles of comparative histology! At the close we find ingenious attempts to homologise nerve tracts and commissures related to the organs of sense, such as are invariably present wherever such organs occur. Sufficient this to show that the comparison, in respect to its leading features, is in the opening case strained to an unnatural degree, in the closing case no comparison at all. Finding, as we do, that the rest of the work is on a par with this, we are compelled to reject the main conclusion as unnatural and unsound; and when we seek the explanation of this remarkable course of action, we are forced to believe that it lies in the failure to understand the nature of the morphological method. For the proper pursuit of comparative morphology, it is not sufficient that any two organisms chosen here and there should be compared, with total disregard of even elementary principles. Comparison should be first close and with nearly related forms, passing later into larger groups, with the progressive elimination of those characters which are found to be least constant. And necessary is it, above all things, that in instituting comparison it should be first ascertained what it is that constitutes a crustacean a crustacean, a marsipobranch a cyclostome, and so on for the rest. We have tried to accept this theory, fascinated both by the arguments employed and by the idea itself, which for ingenuity it would be difficult to beat, but we cannot; and we dismiss it as misleading, as a fallacy, begotten of a misconception of the nature of the morphological method of research. It is of the order of events which led Owen to compare a cephalopod and a vertebrate, led Lacaze-Duthiers to regard the Tunicata and Lamellibranchs as allied; and with these and other heresies it must be denounced.

Passing to the third advance, extending over the last twenty years, it may be said to consist in the revival of a theory of 1829, which boldly asserts that *Limulus* is an Arachnid. In the development of the defence there have been two weak points but lately strengthened, viz., the insufficient consideration of the palæontological side of the question and of the presence of tracheæ among the Arachnida. Under the former there was, until recently, assumed the absence of

the first pair of appendages in the Eurypterida; but it may be said that they have since been observed in *Eurypterus Fischeri* of the Russian Silurian, and *E. scoticus* from the Pentland Hills, in both of which they consist of small chelate appendages flexed and limuloid in detail, somewhat reduced perhaps, and enclosed by the bases of the succeeding limbs, which become apposed as the anterior end is reached. Since by this discovery the Limuloids, Eurypterids, and Scorpionids are brought into a numerical harmony of limb-bearing parts, we may at once proceed to other points at issue. So far as the broader structural plan of Limulus and the Scorpion are concerned, all will agree to a general community, except for the organs of respiration; but concerning the coelom, the mobile spermatozoa, and the more detailed features under which Limulus is held to differ from the Crustacea and to resemble the Arachnida, I would remark that while motile spermatozoa are characteristic of the Cirripedes, the rest of the argument is weakened, by the probability that the 'arachnidan' characters which remain may well have been possessed by the crustacean ancestors, and that Limulus, though specialised, being still an ancient form, might have retained them. The difficulty does not seem to me to lie in this, nor with the excretory organs, if we are justified in accepting the aforementioned argument that the so-called Malpighian tubes may be inturned nephridia, ectodermal in origin, and in knowledge of the existence of endodermal excretory diverticula in the Amphipods. These facts would seem to suggest that as our experience widens, differences of this kind will disappear.

As to the tracheal system, now adequately recognised by the upholders of the arachnid theory, the presumed origin of tracheæ from lung-books, the probability that the ram's-horn organ of the Chernetidæ may be tracheal, the presence of tracheæ in a simple form in the Acari, and, by way of an anomaly, in a highly organised form on the tibiae of the walking legs of the harvestmen (Phalangidæ), are all features to be borne in mind. While I am prepared to admit that this wide structural range and varied distribution of the tracheæ lessens their importance as a criterion of affinity, I cannot accept as conclusive the evidence for the assumed homology between lung-books and gills. And here it may be remarked that a series of paired abdominal vesicles, recently found in the remarkable arachnid *Kœnenia*, invaginate as a rule but in one example everted, seized upon in defence of this homology, have not been so regarded by those most competent to judge.

There remains the entosternite, an organ upon which much emphasis has been placed. Not only does a similar organ exist, apart from an endophragmal system, in *Apus*, *Cyclops*, some Ostracods and Decapods; but, regarding the question of its histology, it may be pointed out that from all that is at present known, the structural differences between these several entosternites do not exceed those between the cartilages of the Sepia body. And when it is found that the figures and descriptions of the entosternite of *Mygale* ('*Mygale* sp.,' '*Mygalomorphous Spider*,' *auct.*) have been thrice presented upside down! the reliability of this portion of the argument is lessened, to say the least.

Recent observation has sought to clench the homology of the four posterior pairs of limbs of the King crab and Scorpion, by appeal to a furrow on the fourth segment in the former, believed to denote an original division into two; but I hesitate to accept this until myological proof has been sought.

Returning, amidst so much that is problematic, to the sure ground of palæontology, I wish to point out that when all is considered in favour of the arachnid theory there still remains another way of interpreting the facts.

In both Limulus and the Scorpion the first six of the eighteen segments are well known to be fused into a prosoma bearing the limbs, but while in the Scorpion the remaining twelve are free, in Limulus they are united into a compact opisthosomal mass. In dealing with the living arthropods, there is no character determinative of position in the scale of this or that series more trustworthy than the antero-posterior fusion of segments. It has been called the process of 'cephalisation,' and the degree of its backward extension furnishes the most reliable standard of highness or lowness in a given assemblage of forms. In passing from the lower to the higher Crustacea, we find this fusion increasing as we ascend: and it therefore

becomes necessary to compare the Scorpion with the other Arachnida, *Limulus* with the Eurypterida, in order the better to determine the position of each in its respective series, by the application of this rule.

As to the number of segments present, variation is a matter of small concern, in consideration of the mode of origin of segmentation and the wide numerical range—from seven in the Ostracods to more than sixty in Apus—the segments of the crustacean class present.

On the arachnid side, in the Solifugæ but the third and fourth segments are fused; the remaining four of the prosomal series with the ten which remain are free. In *Kœnenia* four of the prosomal segments alone unite; the fifth and sixth with the rest are free. And when we pass to the Limuloids and the descending series of their allies, we find it distinctive of the Eurypterida that all the opisthosomal segments are free. If we can trust these comparisons, we must conclude that the Eurypterida of the past, in respect to their segmentation, simplify the Limuloid type, on lines similar to that on which the Solifugæ and *Kœnenia* simplify the Higher Arachnid and Scorpionid type, and that therefore if the degree of antero-posterior fusion of segments has the significance attached to it, *Limulus* and *Scorpio* must each stand at the summit of its respective series. If this be admitted, it has next to be asked if, in comparing them, we may not be comparing culminating types, which might well be isomorphic.

The scorpions are known fossil by two genera, *Palæophonus* and *Proscorpius*, from the Silurian of Gotland and Lanarkshire, the Pentland Hills, and New York State; while recent research, in the discovery of the genus *Strabops*, has traced the Eurypterida back to the Cambrian, leaving the scorpions far behind. One striking feature of the limbs of the Palæozoic Eurypterids is their constantly recurring shortness and uniformly segmented character, long known in *Slimonia*, and less conspicuously in *Pterygotus* itself, retained with development of spines in three of five known appendages of the recently described eurypterid giant *Stylonurus*. The minimum length yet observed for these appendages is that of the Silurian species *Eurypterus Fischeri*, discovered by Holm in Russia in 1898. This creature is one of the few eurypterids in which all the appendages are preserved, and it is the more strange therefore that the advocates of the arachnid theory should ignore it in their most recent account. Allowing for the specialisation of its sixth prosomal appendage for swimming, the fifth is but little elongated, the second, third, and fourth are each in total length less, by far, than the transverse diameter of the prosoma, and uniformly segmented, giving the appearance of short antennæ. They seem to be seven-jointed, and are just such appendages as exist in the simpler crustacean and tracheate forms; and in the fact that their structural simplicity is correlated with the independence of the whole series of opisthosomal segments they lend support to the argument for isomorphism.

With this conclusion, we turn once more to the Scorpions, if perchance something akin to it may not be in them forthcoming. The Silurian genus *Palæophonus*, especially as represented by the Gotland specimen, reveals the one character desired. Its body does not appear to be in any marked degree simpler than that of the living forms; but on turning to its limbs, we find the four posterior pairs, in length much shorter than those of any living species, all but uniformly segmented. In this they approximate towards the condition of the limbs of the Eurypterida just dismissed, and their condition is such that had they been found fossil in the isolated state they would have been described as the limbs of a Myriapod, and not of a scorpion at all. Indeed, their very details are what is required, since in the possession of a single terminal claw they differ from the limbs of the recent scorpions as do those of the Chilopoda from the hexapods.

With this the scorpionid type is carried back, with a structural simplification indicative of a parallelism with the other arthropod groups; and while the facts do not prove the total independence of the scorpionid and limuloid series, they bring the latter into closer harmony with the Eurypterida of the past. They prove that the Silurian Scorpions simplify the existing Scorpionid type, on precisely the lines on which the Eurypterida simplify the Limuloid; and they do so in a manner which suggests that a distinction between the *Crustacea vera* and the

Crustacea gigantostrea (to include the Eurypterida and Xiphosura) is the nearest expression of the truth. It becomes thereby the more regrettable that in a recent revision of the taxonomy of the Limuloids the generic name *Carcinoscorpius* should have found a place.

I foresee the objection that the antenniform condition of the shorter limbs may be secondary and due to change. There is no proof of this. Against it, it may be said that the number of the segments is normal, and that where nature effects such a change, elongation is with the multi-articulate state the only process known; as, for example, with the second leg of the Phrynidæ, the so-called second pareiopod of the Polycarpidea, and the last abdominal appendage of *Apseudes*.

That advances such as we have now considered should lead to new departures is a necessity of the case; and it but remains for me to remind you that within the last decade statistical and experimental methods have very properly come more prominently into vogue, in the desire to solve the problems of variation and heredity. Of the statistical method, by no means new, I have but time to recall to you the Presidential Address of 1898 by my friend and predecessor in this chair, himself a pioneer; and of the experimental method I can but cite an example, and that a most satisfactory one, justifying our confidence and support. It concerns the late Professor Milne-Edwards, who in 1864 described, from the Paris Museum, the head of a rock lobster (*Palinurus penicillatus*), having on the left side an antenniform eye-stalk. With the perspicuity distinctive of his race, he argued in favour of the 'fundamental similarity of parts susceptible to revert to their opposite states.' The matter remained at this, till, on the removal of the ophthalmite of certain Crustacea, it was found that in regeneration it assumes a uniramous multiarticulate form; and it is an interesting circumstance that in the common crayfish the biramous condition normal to the antennule may occur. An example this of a fact which no other method could explain.

When all is said and done, however, it is to the morphological method that I would appeal as most reliable and sound. And when we find (i.) that in certain Compound Tunicates the atrial wall, in the egg development delimited by a pair of ectoblastic invaginations, in the bud development may be formed from the parental endodermic branchial sac; (ii.) that regenerated organs are by no means derivative of the blastemata whence they originally arose; (iii.) that in the development of a familiar starfish the inner cells of the earliest segmentation stages, by intercalation among the outer, contribute half the fully formed blastula; (iv.) that there are Diptera in existence in which, while it is well-nigh impossible to discriminate between the adult forms, there is reason to believe the pupa cases are markedly and constantly distinct; it becomes only too evident that the later embryonic and adult states are those most reliable for all purposes of comparison, and that it is by these that our animals can best be known and judged. Caution is, however, necessary with senility and age, since certain skulls have been found to assume at this period characters and proportions strikingly abnormal, and by virtue of the most important discovery, which we owe to the Japanese, that in certain Holothurians, the calcareous skeletal deposits may so change with age, as to render specific diagnoses based on their presumed immutability invalid. Advance, real and progressive, is in no department of zoological inquiry better marked than comparative morphology, and it is for the pre-eminence of this that I would plead. Educationally, it affords a mental discipline second to none.

We live by ideas, we advance by a knowledge of facts, content to discover the meaning of phenomena, since the nature of things will be for ever beyond our grasp.

And now my task is done, except that I feel that we must not leave this place without a word of sympathy and respect for the memory of one of its sons, an earnest devotee to our cause. William Thompson, born in Belfast, 1806, became in due time known as 'the father of Irish natural history.' By his writings on the Irish fauna, and his numerous additions to its lists, he secured for himself a lasting

fame. In his desire to benefit others, he early associated himself with the work of the Natural History Society, which still flourishes in this city. He was President of this Section in 1843, and died in London in 1852, while in the service of our Association, in his forty-seventh year, beloved by all who knew him. His memory still survives; and if, as a result of this meeting, we can inspire in the members of the Natural History and Philosophic Society of this city, as it is now termed, and of its Naturalists' Field Club, an enthusiasm equal to his, we shall not have assembled in vain.

British Association for the Advancement of Science.

BELFAST, 1902.

ADDRESS

TO THE

GEOGRAPHICAL SECTION,

BY

COLONEL SIR T. H. HOLDICH, C.B., K.C.I.E, F.R.G.S.,

PRESIDENT OF THE SECTION.

The Progress of Geographical Knowledge.

WITH so large a field as that which is embraced by geography before us, I feel a little doubtful which way to turn in order to gather into one short space both the scattered records of recent geographical history and to present to you at the same time illustrations of some fixed principle which in the course of the development of our geographical knowledge must govern the progress of it. Last year you heard from Dr. Mill a most excellent summary of the present phase of that development in this country. You heard not only of great activity in the wide world of the unexplored and unknown, but of new efforts to train up a fresh generation of explorers; of new schools springing up amongst us; fresh evidence of the faith that is in us that geographical knowledge points the road to commercial success; happy intimations of the existence of a yet higher faith—the faith which believes that scientific knowledge of the world's physiology is worth the getting for its own sake, whether it paves the way to golden success or not. And now, whilst recalling the chief geographical events of the year that has passed; whilst counting the landmarks on the road to a higher geographical education, I would also claim your attention for a brief space to a few technical problems which beset the business aspect of future procedure, and which so long as we make it our boast that we belong to the biggest empire in the world ought most certainly to attract our earnest attention.

The unknown world is growing daily smaller. It is, indeed, narrowing its area with a rapidity which is absolutely regrettable. If you think of those delightful days when the men who went 'down to the sea in ships' brought gold and ivory to the steps of Solomon's Temple, believing that beyond their nautical ken all the rest of the world was but flat emptiness; or even centuries later when Marco Polo's truthful tales of Asia were discredited as wild fables; or again in almost modern times when Vasco da Gama bent his knees in pious prayer ere starting on the buccaneering venture which was to change the destinies of the East, you will find it almost impossible to look at the well-turned-out maps of to-day, wondering where next it may be possible to strike a new feature or unfold a new vista to geographical enterprise, without something like a sigh. But it is with the world as we find it mapped to-day that we have now to do, searching out the position of such blank spaces as still exist and considering the best means of dealing with the vast area of its half-exploited surface so as to obtain the best results for the time and labour spent on completing our knowledge of it.

Antarctic Prospects.

To the Polar regions we naturally turn first, for they form the special domain of modern initial exploration. We are very far yet from having elucidated the great geographical problems of sea and land distribution which lie hidden under the depths of palæocrystal ice. We only know indeed from inference that at one end of the world there exists an unmapped sea, and at the other an unmapped continent, round the edges of which we are even now feeling our way. When the 'Discovery' left the New Zealand port of Chalmers on December 24 last for the South Polar regions, this was the quest which, in the modest language of her originator, Sir Clements Markham, lay before her: 'To determine as far as possible the nature and extent of the South Polar lands' and to 'conduct a magnetic survey.' If we look at the unexplored area of these South Polar lands as a whole and examine the plan of international geographical campaign which has now been directed against them, we shall find, I think, that the present enterprise is by far the most complete and systematic, as it is the most scientific, that has yet been undertaken in the Far South. It is impossible but that great results should be attained from so complete an investment of the unknown continent.

With the 'Discovery's' investigations, which will be directed to Victoria Land—the land of the historic volcanoes Erebus and Terror—from the side of Tasmania and New Zealand, will be associated at least three other expeditions, all aiming at a final solution of the South Pole problem. From South America Otto Nordenskiöld's expedition has taken the shortest sea route past the South Shetlands to Graham's Land, and has already passed a winter amidst the ice. From South America, again, the Scottish expedition under Bruce will work its way past the Sandwich Islands, skirting the Antarctic Circle, some fifty degrees to the east of Nordenskiöld, almost on the Greenwich meridian, and as nearly opposite as possible to the 'Discovery's' attack from the other side of the Pole; whilst between the two will be the German expedition, of the 'Gauss,' pushing southward about the meridian of 90° E., a worthy rival in scientific equipment to our own ship the 'Discovery.' And there is no branch of scientific inquiry which will be advanced by this international attack on the great unknown southern land of more interest than that which pertains to the history of the world's geography. Independently of securing a firmer outline to the vague definition of southern land areas of the present day, it is there that we hope to find evidences of another distribution of those areas in primeval times. Shall we be able to trace the Patagonian formations, those recent basaltic lavas which overlies trees, beyond that point in Graham's Land where we know that they occur again, to the Australian side of the Southern Pole? Shall we find that Erebus and Terror are but the natural extension of that magnificent array of volcanic cones which overlook the Pacific from the Patagonian Andes? Will the Miolania, the great turtle of Patagonia—not unknown in Australia—complete with his bones another link in that chain of many evidences that Patagonia and Australia once met across the extreme south? You may say this is not geography. I hardly know whether in these days it is still necessary to plead that between geography and natural sciences, whether of geology, biology, or anthropology, the connection is so intimate that in the actual field of research it is impossible to disconnect them. Modern geography is but a development, and whilst the process of its evolution is perhaps to be found in strictly geological fields, it has so modified and influenced the problems of life and the distribution of it throughout the world that a collector of facts like myself finds it convenient to accept, for the mere sake of simplicity, the science of geography as the best basis for divergent inquiries into many other scientific fields, which can be differentiated at leisure by the natural philosopher.

Necessity for Study of Geographical History.

But whilst we are justified in expecting much from this great international movement we must still moderate our expectations. We must admit that in the

field of purely naval exploration we have not the same developments in mechanical and instrumental accessories which place within our reach the possibility of conducting land expeditions on far more scientific and exact methods than were possible to our grandfathers. Wireless telegraphy, for instance, will not yet enable a ship fast bound in Arctic ice to determine her longitude, and the restless ocean still precludes the use of many of the more finely graduated instruments which are essential to the exact measurements pertaining to triangulation. Methods and instruments, indeed, will not differ materially from those adopted by Franklin or by Ross more than half a century ago. Better instruments of their class no doubt are within reach, owing to the extraordinary accuracy of modern production; but better hands to hold them it would be impossible to find. We are often so pleased with ourselves in these days that we are apt to forget what has been done by our geographical forerunners in the same field as ourselves. I have but lately returned from a journey full of geographical interest which has carried me over some of the tracks left many years ago by a British scientific expedition to the South Seas, which will be ever associated in the memory of all geographers with the names of Charles Darwin, and H.M.S. 'Beagle.' With the wider scope for gathering information which is afforded in these days by the growth of civilisation and the shooting out of its long tendrils into the waste places of Patagonia, it has been possible to verify some of the suggestions as to the structure and geographical configuration of that southern continent which were offered by the observations of Darwin, and to examine here and there, in some detail, the results of recent local surveys in testing the accuracy of the coast outline and of the coast soundings established by the 'Beagle.' Of the former I can only say that they seem to me prophetic; of the latter, so little change has taken place in South American coast configuration during the last fifty years that practically the charts of the 'Beagle' are the charts of the Chilean and Argentine Admiralties of to-day, with hardly a noticeable variation. Such magnificent results as were achieved then are hard to beat at any time. We do not hope to beat them. We can only hope to imitate them. They stand good for all time, and it is useful to recall them now and then in order to emphasise a truism which is occasionally overlooked by modern geographical explorers. It is not the most recent work in the field of exploration which is necessarily the most valuable. One of the great sins of omission in modern exploration is that of a failure to appreciate the efforts of preceding geographers in the same field of research as ourselves—the want of a patient absorption of all available previous knowledge before we attempt to add to the sum of it. We are not all of us gifted with the patient determination of that great traveller Sven Hedin, who spent three years in reading about Central Asia before he wrote a word on the subject. It cannot be too strongly urged in these days of narrowing fields for activity that although geographical research is essentially an active function of an active life it demands yet more and more, as time goes on, the application of the scholar added to the determined energy of the explorer.

Formation of a Central Committee of Geographical Advisers.

It is in this connection that I would advance a suggestion which I have already heard discussed by travellers anxious to apply their energies in well-directed efforts towards the acquisition of really useful scientific information. It concerns the possibility of establishing a central geographical committee which should gather together expert knowledge in all branches of natural science, and be prepared to give technical advice to travellers and explorers, not only as to the literary sources from which the best information may be derived, but also to furnish hints as to the best localities for research in any special branch of science. This would certainly shorten the preliminary labour of collecting information; and in many cases when expeditions are planned at short notice it would be invaluable in indicating opportunities for special research which would otherwise be overlooked. It is so much more frequently want of time, rather than want of inclination, which prevents the acquisition of that preliminary and most essential knowledge which

alone can rightly direct the effort to the opportunity and fit the two together, that I have much sympathy with the pathetic appeal of more than one young explorer who has complained that it is necessary to travel all round London in order to find the man (to say nothing of the book) who will tell you in concise language exactly what to look for in the land which you are visiting.

Contraction of the World's 'Terra Incognita.'

It is, however, when we leave the high seas with their almost inexhaustible store of unexplored ocean floors and icebound coast-line, and turn from oceanography to the more familiar aspects of land geography that we find those spaces within which 'pioneer' exploration can be usefully carried to be so rapidly contracting year by year as to force upon our attention the necessity for adapting our methods for a progressive system of world-wide map making, not only to the requirements of abstract science, but to the utilitarian demands of commercial and political enterprise.

Take Asia, for example: nearly half of the great continent pertains to Siberia, and within the limits of Russian territory the admirable organisation of her own system of geographical exploration leaves no room for outsiders to assist usefully, even if political objections did not exist. In Central and Southern Arabia there is undoubtedly still much to learn, but of the remaining countries which intervene between the Mediterranean and India, of Persia, Afghanistan, and Baluchistan, it can only be said that the work of the geographical pioneer has already ended where that of the engineer and surveyor has commenced. In the Furthest East again—in Manchuria, China, Tonkin, and Siam—there is much more room for the practical exploration of the road and railway maker than there is for the irresponsible career of the geographical traveller. The highway from China to India is almost as well known as that from London to India, and the activity of railway enterprise in the south of Asia bids fair to rival the triumphs of Siberia. It is only in the central deserts of Mongolia and the wastes of Tibet spreading southwards to the Himalayas that we can find untrodden areas of any great magnitude, and even in Central Asia before venturing on a statement of future possibilities in the field of exploration, it would be well to wait for the records of that most intrepid traveller, Sven Hedin, who promises us material of scientific and historical interest as the result of his last three years' travel far in excess of the monumental contributions which he has already made public. Historically the interest of the world of inquiry in Asia where we find the origin of the great races of the world and the birthplace of all religions must always be immense; but that history can only be elucidated by a clear illustration of the great highways of the Continent which were open to the vast migratory movements of mankind in prehistoric periods. We do not in the least understand the condition of climate, nor are we quite certain even of the relative distribution of land and water in High Asia in the days when its swarming population first began to flow south and west, carrying the elements of a language which we have been accustomed to regard as primeval into the swamps and plains which lay beyond the Himalayas or the Caspian. It is only through geographical research that some dim outline of those early stories can be realised; and although the researches of Stein and the marvellous discoveries of Sven Hedin around the ancient lake district of Lob Nor will, after all, only throw the world's history back for a few centuries, it is by means of these first steps backward that we can feel our way to an appreciation of the earlier processes of this phase of human evolution. Nor in the interests of utilitarian commercial speculation is geographical research in Asia yet to be set aside. We indeed know comparatively nothing of its resources in mineral wealth. It is quite within the bounds of possibility that one of the great central treasure houses of nature lie enveloped in the geological axis of the highest mountains of the world, and that we may yet be enabled to explain why every river which flows from Tibet washes down gold in its bed. But this will only be

when the Tibetan Lama is prepared to shake hands with the Uitlander; and I fear that recent South African history will not encourage the embrace. Meanwhile there is no more promising field still open to the *bona fide* explorer than that of Tibet and the farthest ranges of the Himalayas. Few people are aware how vast an extent of the Himalayan area still remains untrodden by any European. This is due to no want of enterprise on the part of our Indian surveyors and political officials. It is due partly to physical inaccessibility, and partly to that intense (and easily understood) objection to the interference of the stranger in which many of our transfrontier neighbours permit themselves to indulge. Nevertheless would I commend to those who still desire to walk in the rough and thorny path of pioneer geographical discovery a similar enterprise to that of our aforetime Secretary, Mr. Douglas Freshfield, who lately succeeded in passing beyond the bounds of official exploration into the Eastern Himalayas. We have had many travellers in the Himalayas, but they have not always distinguished between the fascinating pleasures of romantic adventure and the earnest pursuit of geographical business.

Study of Glaciers.

To Mr. Freshfield we certainly owe an introduction to a new vista of great scientific interest in the study of the formation and movements of glaciers. Here, perhaps, we are treading gently on the skirts of geological science; but I have never yet found that part of the world where the careful study of local geographical conformation will not inevitably invoke an inquiry into geological construction. We must accept the inevitable criticism and go on with our glaciers. Where in the world can there be such an area for research into the conditions of glacial formations as is presented by the Himalayas? I grant the physical and political difficulties in the way to which I have referred, but still well within the limits of our own red border there are glaciers yet to be studied, which if not the largest are yet large enough to satisfy the loftiest aspirations, and beyond that border the difficulties of approach are lessening day by day, and are no longer so formidable that they need hinder the steps of any determined explorer.

South American Glaciers.

The speculative interest in glacial movements and their influence on the geographical conformation become far greater when one moves in a country which has been recently shaped and polished, grooved and fashioned, by glacial action; when huge blocks of granite or porphyry, standing sentinel over terraces and ancient glacier-beds, witness to the passing of icebergs in prehistoric seas. Such conditions one may find in two widely separated areas—viz., in the Pamirs and in Patagonia. What causes led to the formation of the first vast ice-cap of which the glacier is the latest evidence? what caused its disappearance, its reappearance? why are the glaciers again withdrawing from the mountains? and what causes the universal process of modern desiccation, of which there is such ample evidence in the Pamirs, in Baluchistan, in Patagonia? It is to the Himalayas that we turn first for an answer to this question; but there are other fields almost equally promising, and one of them is to be found in South America. No one now can pretend any longer that we know nothing of Patagonia. Probably no country in the world has been described by so many geographers in so many different ways; there, at any rate, is a land of glaciers and snowfields awaiting research which presents few of the physical difficulties of the Himalayas. Here is a wonderful country truly, where glaciers reach down to the sea in low latitudes, casting little icebergs into waters fringed by green banks of fuchsia and myrtle, and of bamboo; where the laurel grows into magnificent timber, competing with the Patagonian beech for root-hold on the moss-covered soil. The round grey heads of the granite hills, scratched and seamed by a discarded ice-cap on one side of the narrow straits balance the snow-bound peaks of the Cordilleras on the other. No physical difficulties bar the way to the investigation of glacial phenomena amidst some of the most striking coast scenery in the world. Near

the parallel of 51° S. are two Patagonian lakes closely associated—Argentina and Viedma—which offer opportunities for the study of glaciers such as are probably not to be found anywhere else in the same latitude. For here the phenomenon of disappearance is in the stage of natural illustration. Glaciers are disappearing rapidly which but a few years ago seemed to be a permanent feature of the surrounding mountains, and the lake surface is chequered with their débris. There, too, may be studied for hundreds of miles northward the natural sequences of their disappearance—the formation of freshwater lakes and their gradual desiccation in turn—whilst all around there is the continued story of geographical evolution due to the alternate forces of glacial and volcanic action written in gigantic characters on the face of Nature.

Central South America.

Not very much has been added of late years to our practical knowledge of the hidden depths of Central South America except from the inexhaustible mine of information possessed by that eminent geographer Colonel Church. A Brazilian expedition in 1890; the explorations of a commission sent to investigate the interior with a view to the establishment of new political capital to Brazil in 1892–93; the discoveries of Dr. Ramon Paz in 1894, and a chequered journey in the Valley of the Orinoco by Stanley Paterson in 1897, form the principal records of modern days. There is doubtless much which is of the greatest commercial and political interest still to unravel in connection with the geography of the great river basins of the continent. But in South America we are threatened with perhaps the greatest development of what I may call artificial geography that the world has ever seen. Not only will the consummation of the Panama Canal project change the whole system of our western sea communications, and probably exercise a more enduring effect on the world's commerce than even the Suez connection between East and West, but the possibilities of linking up by a central canal system the three great river basins of the South—that of the Orinoco, the Amazon, and the Plata—is under serious consideration, and the mere project will in itself lead to an exhaustive examination of much untravelled country. Thus, even South America no longer offers a large field for the geographical pioneer of the future. With its narrowing areas of *terra incognita* and its almost phenomenal advance towards a leading position as the pastoral and meat-producing quarter of the habitable globe; with possibilities of development in this particular line probably exceeding those of Australia, New Zealand, and South Africa all put together, it is surely high time that South America turned her attention towards a combined and sustained international effort to place her scattered and most insufficient geographical surveys on a sound geodetic basis extending through the whole continent.

North America.

In the geographical fields presented by North America, as also by Australia, magnificent as are the opportunities for acquiring that personal acquaintance with the great depositions of nature which environ new conditions of life, and shape the course of human existence to its appointed ends, or, in other words, to acquire a geographical education from original sources of instruction, there is but little opening for the enterprise of the pioneer who aspires to show the way into new fields. There is no lack of native enterprise in colonies perplexed by the stout-hearted descendants of generations of explorers. Neither Canadians nor Australians wait for England to show them how to develop the resources of their own country, or pilot the road to new ventures. On the contrary, we have to turn to Canada now for instruction in the higher art of geographical map-making, and to admit that England has been left far behind in the development of the special branch of science which deals with the illustration of the main features of geographical configuration in relation to their geological construction.

Africa.

In Africa the advance of our knowledge of the main outline of the geographical features of the continent has been so rapid since the days when the Nile was first traced to its source by Speke that a perfect network of explorers' lines of travel now embraces the continent in its meshes, and it is only in the intermediate spaces that room for enterprise on the part of the pioneer is left, even if it may not be said altogether to have vanished. A reference to the little map published by Mr. Ravenstein in the 'R.G.S. Journal' for last December will show you at once that the hydrography of Africa has been fairly well traced out in all its main arteries, leaving but few unexplored spaces of any great extent; and that such spaces, where they occur within the area which is especially open to Englishmen, demand an organised system of exploration more complete in its results, more carefully balanced in its relation to the geographical illustration of those lands which are beginning to form centres of civilisation than can be secured by the process of pioneer route making. In short, we want a system of geographical surveying allied to those systems which have been perfected after years of careful experiment by Canada, or Russia, or France, or by England in India. This, however, brings us into a field of technical inquiry of great importance, into which, so far as it deals with geography, *i.e.*, with the measurement of the earth's surface and the illustration of its configuration by means of maps, I propose to enter briefly in this Address.

Modern Requirements in Geographical Map-making.

You will agree with me that geography in the abstract, without illustration—the geography which used to be taught by geography books without maps—is but a poor and inefficient branch of academic knowledge, hardly worthy even of an infant school. It does not matter what branch of this comprehensive science you approach, whether it is historical, or physical, or political, modern or ancient, the only substantial presentment of the subject to man's understanding is that which has recourse to map illustration. Words (especially words bearing such indefinite applications as our modern geographical terminology) can never convey to the imagination the same substantial illustration as maps convey to the eye. You may think that all this is mere truism; so it may be; but I assure you that what I may call descriptive geography, that is to say, geography without the aid of maps, has more than once nearly precipitated national disaster in quite modern times—disaster quite as perilous as any which in military fields has been caused by blank, wholesale ignorance of the features of a country in which strategic movements are undertaken. There comes a time in the history of every developing country when the increase of its people, and the consequent distribution of land, demands surveys for the purposes of fiscal administration. Consequently such surveys are common everywhere; and from these have been built up, piece by piece, like a child's puzzle, the geographical maps of many half-occupied lands, illustrating only such portions as are adaptable to economic development, and leaving blank all that promised to be unproductive and unprofitable.

Field of Geodesy.

It was only when it was discovered that the sum total of such a production was apt to cause great confusion in land assessment, inasmuch as it often did not equal the actual area of the land distributed, that there arose a school of mathematicians who concerned themselves with determining the dimensions and figure of the earth, and founded that apparently complicated system of primary map-making which now takes count of such matters as the curvature of the earth's surface, the convergence of meridians, and other spheroidal problems which affect the construction of the map. Thus arose 'geodesy,' and geodesy has numbered amongst its apostles many of the greatest mathematicians of the age. Geodesy, the science which deals with exact measurements, was never an embodiment of abstract

mathematical investigation. It had always a utilitarian side to it, and it is unfortunate that this view of the science has been occasionally lost sight of in late years. For we have not done with geodetic investigation yet. Magnificent as are the results obtained by the mathematicians of the past, there are still further refinements to be introduced into those factors which we daily use for the reduction of our terrestrial observations ere we obtain perfect mathematical exactness (if we ever attain it) in our results; and we still must look to the processes of geodesy to give us that backbone, that main axis of indisputable values from which our network of triangulations may spread during the first steps in geographical map-making. To a certain extent geodesy is the support of technical geography, and a short inquiry into its present conditions of existence may not be out of place.

It is to North America that we must now turn for instruction in the latest development of the science, and to South Africa that we must look for its future application. Russia has not lost sight of the necessity imposed on her for an extension of her magnificent European geodetic system through the vast breadth of her Asiatic possessions, but we ourselves in India are concerned nowadays rather with scientific observations on collateral lines, and with the collating and perfecting of the results attained by the great achievements of past years, than with any developments in fresh fields of geodetic triangulation. Germany and France, ever alert where colonial interests are concerned, are busy in Africa, but I am not prepared to say how far their geographical efforts are based on the strict principles of geodesy.

In North America, along the meridian of 98° through Texas, Kansas, and Nebraska, geodetic triangulation still forms one of the most prominent schemes of modern work undertaken by the Coast and Geodetic Survey; and in South Africa there is growing northward into the Transvaal slowly, but we hope surely, the framework of a gigantic arc which one day will be extended by Sir David Gill from the Cape to Cairo.

I am anxious to impress on you that the science of geodesy is not a science of the past. It is still active, and with all its refinements of minute accuracy and exact precision in observation and in calculation, it should be the initial mainstay, and it must be the final court of appeal, as it were, for all those less rigorously conducted surveys of the reconnaissance and exploration class which we term geographical.

But this accurate framework, this rigorously exact line of precise values which ultimately becomes the backbone of an otherwise invertebrate survey anatomy, is painfully slow in its progress, and it is usually haunted by the bogey of finance. It does not appeal to the imagination like an Antarctic expedition, although it may lead to far more solid results, and it generally has to sue *in forma pauperis* to Government for its support.

Geographical Surveys.

And thus it happens that long before the tedious and expensive processes which are involved in the term geodetic triangulation can possibly be carried to an effective end the cry goes up for a geographical survey. It is wanted by the administrator to whom it is all important that he should know the roads and river communications, and the productive areas of the land he has to administer, and be able to locate the various tribal sections or peoples with whom he has to deal. In the political department a geographical map may be said to be absolutely necessary for the political purpose of defining limits and boundaries. It has been, I am aware, occasionally dispensed with, but never with satisfactory results. To the officer on whom rests the responsibility of preserving peace and good order it is most desirable that the military features should be fairly represented in such a manner that at least a general plan of action can be arranged at short notice. For the economic development of the country it cannot be too strongly urged that a general geographical outline of its surface is indispensable to the selection of lines for special technical examination, whether for roads, railways, canals, or telegraphs. How often lately in the history of our colonial or frontier progress have vast

sums been expended on special lines of railway in ignorance of the fact that better alignments of infinitely less physical difficulty would have been at once revealed by a general geographical map even on the smallest scale? In short, the cheapest, the quickest, the surest, indeed the only satisfactory method of regulating the progression of public works, the development of commerce, the proper recognition of the frontier boundaries, the administration of justice, and the military control of a large and growing colony, or of a long stretch of military frontier, is to be armed with a perfect summary of what that country contains in the shape of a geographical map; and yet it is only quite lately that this fact has been recognised by English administrators and English generals in their dealings with new colonies and new frontiers. Russia learnt the lesson a generation ago at least. When she reached out a hand for Constantinople her army was accompanied across the Balkans by whole companies of surveyors, who worked on no sketchy system of indicating lines of route here and there. They pushed at least seven series of triangulation across the mountains, and on that as a basis they mapped the whole country in detail on a good military scale (about an inch per mile) right up to the very gates of the Turkish capital. For years her brigade of topographers has been busy along her Afghan and Siberian frontiers. In Persia, Baluchistan, the Pamirs, and China, wherever in fact there may be in the future some prospective view of a closer political, commercial, or military interest than exists at present, there they are to be found. France has always been strong in the geographical field, and the late achievements of Frenchmen in the world of exploration and of exploratory map-making are only equalled by the scientific knowledge and literary ability displayed in their technical literature on the subject. Colonel Laussedat's contribution to the 'History of Topography' is to be reckoned with as a standard work. In Canada and North America we have perhaps a practical exposition of the art of geographical surveying which is as unequalled in completeness and comprehensiveness as the country with which it has to deal is unequalled as a subject for its application. There the close association between geological structure and geographical conformation is so fully recognised that the same technical process of surveying is applied for the purpose of the double illustration. The Canadian geological survey is their geographical survey, and I think that it is to Canada (if not to India) that we owe the first recognition of the fact that geographical surveying is a separate, distinct, and most important branch of the general art, which should form the basis—the mother survey as it were—from which all other surveys should spring. In India I am happy to think that this advance in the science of geography is now well understood. It has been more or less forced on us by the necessity for such rapid and comprehensive surveys as are required for frontier military operations, for the purposes of boundary demarcation, and for the important duty of keeping our own trans-frontier information up to the level of that of our neighbours. In our African colonies it has, alas! been discovered a little too late that geographical surveys are a sound preliminary to military operations, but the discovery once made it is not likely to be overlooked. Here, indeed, was presented a most forcible illustration of the danger of building up a geographical puzzle map; of piling one on to another the results of local fiscal surveys in the hope that when they were all put together they might make a good topographical guide to the country. Needless to say the result was disastrous from the scientific point of view, and it might almost be said of it that it was disastrous from the military point of view as well. Imagine for an instant that the Canadian system of a geological survey (involving of course accurate topography) had been applied *ab initio* to South Africa, who can possibly say what the result might not have been by this time? The expansion of the Randt mines, for example, depends at present on local experiment carried out no doubt by most able engineers with all the knowledge of scientific mining that is to be acquired in these days of advanced specialism. But all the same I may be permitted to suggest that their experimental ventures, their tentative borings, are subject to a good deal that is almost guess work for their application, and that a comprehensive, carefully conducted geological survey of the whole country would probably have afforded valuable

indications in many unexpected directions. So also as regards schemes for local irrigation. Take the north-western part of Cape Colony, for instance, the district known as the Karoo, where the best military map existing at the time of the war did not even pretend to show the main roads through the country. The stage of development at which that part of the colony has arrived in the all-important matter of local irrigation is only worthy of the Dark Ages. It would be laughed at in Persia or Afghanistan. The Arabs of mediæval times were experts in the art of the conservancy and distribution of water in dry lands compared to the modern South African (or South American) farmer. Now I do not say that schemes for merely local irrigation require geographical maps to support them. Such schemes only require a little enterprise, a little common sense, and a little capital, but I do say that the geographical map would long ago have revealed the opportunity for comprehensive schemes, such as exist in India, just as it would have pointed out the best alignment for roads and railways, the best means for dealing with an enemy who can move fifty miles in a night, and who can make, not merely a few square miles, but a whole district the theatre of his operations. What was wanted (and is still wanted) in South Africa is what is wanted in every part of the continent subject to British suzerainty. I know that I am but echoing the urgent demand which has been made by every commissioner and governor within the limits of that vast area—not for elaborate or special maps for fiscal and revenue purposes, all of which will come in due time—but for scientific geography which shall now take the place of the preliminary work of pioneer explorers, and deal with the country as a whole instead of tracing it in outlines and in disjointed parts. In short they require all gaps filled up. They want to know what the country contains in the way of forests, of open land suitable for agriculture, of desert and swamp, of opportunities for roads and railways, for telegraphs and irrigation, before deciding on the right portion for the centre of an arterial system of public works which shall pervade in natural and orderly sequence, and in due time, every part of the body of the country of their administration. Now this is scientific geography. It is not ordnance map-making nor anything very much like it. It is a comparatively new demand on the scientific resources of England, and those resources are by no means equal to the demand. Before considering resources, however, we must look to the scientific means to this geographical end. I have already referred briefly to the subject of geodesy, and I have told you that what is termed geodetic triangulation is a function of high scientific order, demanding not only minute and painstaking care on the part of an able staff of observers, but very considerable time and very considerable expense to carry it to a satisfactory issue. I have also pointed out that inasmuch as the exact distribution into parts of any large space of the world's area must ultimately depend on the exact measurements which are a function of only the highest class of geodetic triangulation, we must look finally to geodesy to support the framework of our geography and to give it its rightful place in the great total of the world's mapping. But the demand for geographical mapping is not satisfied with the promise of an elaborate basis for the work which has first to be constructed with the expenditure of much time and money before anything in the nature of a final map can be produced for purposes of administration. The political world, too, cannot always sit patiently through all the international disagreements, the losses, the unrest, and the positive national danger to which an unsettled boundary gives rise, whilst the geodesist works slowly through the country year after year, piling up sheaves of equations and folios of observations, but never a square mile of practical topography. As for the military department I hardly know what to say. There is the example before us of Germans, Russians, French, and Americans, all conducting their campaigns with maps in their hands, taking every special means at their command in order to acquire such maps before they commence operations: whilst the Boers have fought us to the bitter end with a practical knowledge of the country which is even better than maps, and which is exactly that class of knowledge which maps are supposed to replace or supplement. None of them wait for geodesy.

Certainly the attitude of the military department is not one of neutrality. They would like the maps, they are even anxious to get them, but they are not quite certain that they are worth paying for. However that may be, I can only express my own conviction that geographical mapping will be found to be an urgent necessity in every corner of the unmapped world subject to British influence. We would like to wait for those accurate determinations of geodesy which would at once furnish us with the best of all possible means for commencing a comprehensive geographical survey. But we cannot afford to wait, and the great geographical problem of the age is how to reverse the natural sequence of scientific procedure and to obtain maps of the unmapped world which no subsequent geodetic operations shall condemn as inaccurate. It is not a question of expediency; it has been one of necessity for many years past; and inasmuch as necessity is the mother of invention, I think that it will finally be conceded that means *have* been found for ensuring sufficient accuracy in geographical work to render it capable of enduring the subsequent tests of completed geodetic measurement without dislocation and without interference with the general utility of the maps, even if that accuracy be not scientifically perfect.

It is not my intention to bore you with technical details. I only wish to impress upon you that in the field of scientific geography, as in other fields, 'the old order changeth.' We must work on new principles in order to meet new demands.

Use of the Telegraph in Geography.

One of the chief means to this end is the telegraph. Few people appreciate the important rôle which is played by the telegraph in these days in the field of geography. It was not so very long ago that the first step towards regenerating a natural wilderness, or for securing access to new commercial openings or centres of uncivilised population was held to be the construction of roads and railways. Means of physical access was the first step towards the development of a country which was regarded as unenlightened from the standpoint of European civilisation. It is so no longer, for the telegraph often threads its way through many a dreary waste of unpeopled earth, uncoiling its length for hundreds of miles in advance of any railway, or indeed of any road, which can in the ordinary sense of the term be described as a constructed road. I will give you an illustration. On the Patagonian pampas not so very long ago, in the midst of a wide wilderness of snow, after losing our way in a blinding snowstorm and camping on our tracks for the night, we struck the end of the telegraph line which is now being pushed across Patagonia, and which will eventually connect the Atlantic with the Pacific. We had seen no roads whatever for a great part of the distance we had traversed. Our daily procedure was the simple process of following a guide over the illimitable stretches of bush-covered uplands which reach down from the eastern foot of the Andes in gentle grades to the Atlantic shore; and when we did at last fall in with the great central line of trans-continental communication we found it to consist of the wheel-marks of certain previous waggons which had drifted along that way, a sort of road which it was exceedingly easy to lose in the fading light of a stormy winter's day. On this road there was nothing but a telegraph end and the tents of a few telegraph officials, and we were some 150 miles from our destination on the Atlantic coast. And so it happened that after weeks of absence from any means of communication with the outside world we were thus suddenly put in possession of its very latest news; and the very first message that passed from the end of that line into my hands was the message of peace with South Africa, signed an hour or two previously. I accepted that message as a happy omen for the result of our Patagonian mission. And thenceforward (thanks to the courtesy of the telegraph chief at Buenos Aires) nightly as we sat in the snow we read all that was important from the London evening papers of that self-same day. We were not starving by any means, but had we wanted a loaf of bread in that unbroken stretch of snow-covered bushland we certainly could not have got it; whilst here was information flowing in with

a daily ease and regularity that I greatly missed when once again I was within reach of clubs and civilisation. The importance of telegraphs in the field of geography, however, is not confined to the transfer of news to casual travellers. It is the facility which it places in the hands of the geographer for determining his position in longitude that renders it so important a factor in the prosecution of a geographical survey. Everyone knows that the first duty of a geographer is to discover his latitude and his longitude. Hitherto the determination of the first has been a matter of no great uncertainty, but, as regards the latter, one can only say that the confidence expressed by most explorers in the results of their observations has never been justified by the final verdict of a subsequent determination. It is, in truth, most difficult even for the most practised observer to obtain an absolute value in longitude on which he can rely within such limits of accuracy as are essential to the construction of a map where these values have to be employed differentially. The telegraph places in our hands the means of differential determinations within a degree of exactness that surpasses even that of the most careful determination of latitude; and the telegraph is everywhere. Supplementary to the facilities of time-signalling by telegraph is the wonderful accuracy of graduation introduced into the smaller classes of new instruments which in these days replace the cumbersome equipment of the past. With a small 6-inch theodolite fitted with a complete vertical circle time values can be determined within a fraction of a second, and latitude values to within two seconds of arc, always provided that that great bugbear of the astronomical geographer, level deflection, does not interfere with his results. But the same minute accuracy in graduation which has so improved the ordinary little instruments which you find in the hands of the professional geographer has, when combined with new methods for accurate linear measurement, also placed it in his power to carry out a fairly coherent and systematic triangulation with great rapidity and accuracy over large areas of country whenever the configuration and characteristics of that country are favourable. Usually they are favourable. Large expanses of flat desert, of undulating veldt or of unbroken forest are the exception, not the rule, and they must of course be dealt with as their special peculiarities demand; and for the normal conditions of land configuration, given that the explorer is specially careful about his base measurements and his initial data, he can certainly with modern instruments and the facilities for check given him by the telegraph, carry on a rapid and comprehensive geographical survey which will fulfil all the conditions required by the administrator, economist, political geographer, or military commander within such limits of accuracy as will ensure its standing all the subsequent tests that geodesy may apply without any apparent map dislocation. And practically that is all that is wanted for a first map. I have used the word 'rapidly.' Few people (even scientific geographers) have really grasped the full meaning of the term as applied to surveys on geographical scales (*i.e.*, 1:250000, or about four inches per mile, or less) under normal conditions. Such surveys can be completed quite as fast as an army can advance in the field, even granting that the advance is continuous. They can even to a certain extent precede that advance in face of an enemy. A single triangulator with a staff of two or three topographers in a fairly favourable country will be responsible for an outturn which may be counted by hundreds of square miles per day. The records of both American and Canadian surveys will prove that the marvellous progress made in the frontier reconnaissance surveys of India is nothing abnormal or unexpected.

Necessity for Training Schools.

So far I have spoken about the system only, a system which has been nearly perfected by experiments in Canada, Russia, India, and elsewhere. Now we have to turn from the work to the workmen. It is only lately, quite lately, that England has discovered that such workmen are wanted at all. Five or six years ago there was not a topographer nor a topographical school in England. But the demand during late years has been insistent and constant, with the result, I am

glad to say, that efforts have been made in various directions to start topographical schools, and a distinct change is apparent in our methods of instruction at military headquarters. No purely technical central civil schools such as exist on the Continent are to be found in England, and the natural result is that at present England possesses no finished topographers and not many men who know what is meant by a geographical survey. In the wilds of Patagonia (which is, I must premise, a country beset with special climatic difficulties, but not otherwise one unsuitable to the topographer's art) I met many men of great intelligence and exceptional skill who had been gathered from various quarters for the purpose of topography. There were Italians, Argentines, Germans, French, and Swiss, but not an Englishman amongst them. Russians of the type of my old and forgotten friend Benderski have long been famous for their skill; but although English administrators and soldiers are alike crying out for more and better assistance in the active field of topography they cannot get it from England. The establishment of a school of practical geography such as must eventually guarantee the existence of a military topographical corps would be a matter of congratulation deserving to be noted as an important step in the advance of the geographical education of the country, no less than the school at Oxford which deals more directly with civil interests, and is rightly most concerned with the academic aspects of geographical instruction. Even this, however, is hardly sufficient. I am convinced that the recommendation which arose from certain resolutions found in the Geographical Section of the British Association Meeting at Bradford two years ago in favour of the employment of natives in Africa for African work, just as Indian natives are employed in India, is thoroughly sound. We want schools in Africa as well as in England. Only in this way will the vast areas still unmapped in our African protectorates be dealt with at reasonable cost and in a reasonable space of time.

Photo-topography.

Certain developments in the practical field of geography have lately been brought to the test of continued experimental application, and the progress of these experiments deserves a passing record. Notably the application of photography to purposes of geographical illustration has received immense impetus from the apparent facility with which the experimental media can be handled. In favour of the haphazard landscape illustrations with which we are usually deluged by travellers there is little to be said. They are far more frequently illustrations of the personal progress of the author than of the general character of the country he progressed through. Neither is there much more to commend in photographs designed to reproduce geological or tectonic features, glacial configuration, special orographical conditions, or the like unless the position of them and the direction of the line of sight from the point of view are very clearly indicated on a corresponding map. At the best they are apt to be deceptive, for the reason that they can but deal with one side of a subject and with only a partial view of the particular feature they represent. Everyone knows that an apparent range, or even a system of ranges, of mountains may be nothing but the *revêtement* of a high plateau or tableland; but the photograph of such a mountain system will give no indication of the plateau beyond which can indeed only be determined by a survey, and properly illustrated by a map. I need hardly say that a topographical delineation of ground derived from observations made by the aid of photography demands as much technical skill on the part of the topographer and as much systematic application of the use of instruments as any other survey. It must be a combination of careful triangulation and skilful plane-tableing precisely as is the product of a topographical survey. It demands, if anything, more special training and a more elaborate method of procedure than does ordinary survey. So far as the results of experiments made over suitable fields in Canada can teach us, the verdict is in favour of the process only under certain conditions of light and climate when it is desirable to obtain a record of observations in as short a space of time as possible, either in high altitudes, when passing clouds afford but a fleeting view of the landscape, or in low-lying districts, where active tribal hostility

in the field or some similar condition renders it desirable to curtail operations as much as possible. Under all other ordinary conditions it is maintained by Canadian surveyors that although both time and labour may be saved on the field operations, the resulting map can never attain the same standard of accuracy in detail that distinguishes good topographical illustration of the usual variety of natural features. I am, of course, now speaking of geographical surveying as an art, not of mere geographical exploitation. In the latter case doubtless every traveller who can 'pull the string' in these days can add immensely to the personal interest of his journeys by his illustrations of them. But I would earnestly impress upon all travellers that if they desire those illustrations to be of any use for geographical compilation it is absolutely necessary to know the point from which they were taken and the direction of the view.

Barometric Records.

Once again too would I warn travellers of the utter uncertainty of all classes of barometric determinations for altitude. Very little has been done in recent years towards improving instruments of the barometric class, and meteorological science has not yet taught us how to deal with the constant variations in air pressure produced over local areas by changeable weather. There are some countries where barometric records can hardly be regarded as offering a clue even to differential heights. It cannot be too often insisted on that the determination of the relative heights of mountain peaks and of the local value of refraction by means of the theodolite is as much the duty of the triangulator as is the fixing of those peaks in position for the use of the topographer. From these again the altitude of positions in the plains can be safely determined by small instruments of the clinometer class without resorting to the barometer at all, although it may still be necessary to ascertain the value of one initial (or final) point which must be determined by many observations spread over a considerable length of time and synchronous with another set of observations determined at sea, or some already known, level. This of course will occur only when a new geographical area is opened up to survey at some distance from the sea.

Universal Mapping.

It will be remembered that a scheme was set afloat some years ago by Dr. Penck, the eminent German geographer, for the mapping of the whole world on the scale of one-millionth, which is very nearly equivalent to the scale of sixteen miles to one inch. Substantial progress has now been made in support of this scheme by English map-makers, especially in India, where all the trans-border countries which have fallen geographically into the hands of Indian surveyors are now being mapped on this scale. In the commencement of all great colonial survey schemes it is much to be hoped that this project for one homogeneous and universal map will not be lost sight of.

Map Spelling.

I wish that we were as well on the way towards homogeneity in spelling as we are in scale; but it is much to be feared that arbitrary rules will have to be applied to so many special localities that no universal system is ever likely to be adopted. The further that exact geography extends the more difficult becomes this problem, until at last we shall probably arrive at the conclusions adopted long ago by the Government of India, and consider it best to lay down by order an arbitrary list of prominent names, and rule that the spelling of them shall be maintained as in this list in all Government records and maps. Scientists may disagree, but after all it seems the only practical way out of the confusion that exists at present.

Terminology.

There is yet another subject of world-wide interest to the geographical student equally with the practical geographer which requires something of the erudition of the philological scholar to be brought to bear upon it in order to arrive at a satisfactory issue. I refer to the subject of geographical terminology. It may seem an easy thing to be satisfied with such general definitions as are involved in the terms 'range of mountains,' 'coast lines,' 'main channels,' 'watersheds,' 'slopes,' 'affluents,' and the like; but when these terms, and terms similar to them, are employed in international agreements and treaties, carrying with them the necessity for identifying on the face of nature the feature which corresponds to the term employed, there is always to be found room for discussion as to what its exact meaning may be. For the variations of nature are infinite, and no two features classified under the same generic name are alike. Were I to give you examples of only a few of the geographical expressions which, carelessly used, have led up to serious international disagreements you would, I am assured, agree with me that it is high time that geographers all the world over came to some definite understanding about the meaning of geographical terms. To take an instance. What is a 'range' or a 'main range' of mountains? Where does it begin? Where does it end? How far does the term involve geological structure? When a continuous line of similar structure is split across the axis of it, does it become two ranges or does it remain one and the same range? Or, again, what is 'the foot of the hills'? Is it where the steep slopes end and the talus or gentle gradients of its detritus commence, or must you follow the latter down to the nearest watercourse? If you talk of the coast line of Western Patagonia or of Norway do you include such headlands as are connected with the mainland at low water, and exclude the islands, or do you mean the coast line of both? What is the main channel of a river? Is it where the flowing water scours deepest from time to time, or is it a fixture amongst a score of minor channels that shift and change? Perfect definition is of course hopeless. It is not in the power of man to deal with all the infinite variations of geographical feature and to classify them as he would specimens of botanical origin or of natural history. But we might arrive at a much more satisfactory dictionary of geographical terms in our own language than at present exists, and we might offer that dictionary to the geographers of the world at large and say, 'Here we have at least endeavoured to explain our meaning when we make use of geographical expressions. This is what is taught in our schools as the best means of translating the general idea into a distinct mental conception of natural features; and in future when we use these terms you will know on the best authority that England can produce what it is that we mean by them.' Then possibly instead of having to turn to Germany and France for assistance in expressing ourselves clearly when drawing up legal documents dealing with geographical conditions, we may find the English language become the standard for this special class of literature in spite of its verbal poverty. This at any rate is what is now being attempted by the Geographical Society, which spares no effort in order to obtain the best literary assistance in its compilation that the country affords. We shall soon have a geographical dictionary, I trust, and be able to enter with a little more ease and confidence into the field of literary discussion of geographical subjects.

Progress of Geographical Education.

The progress of geographical education in the country, although it is by no means so universally apparent as might be considered desirable, yet shows encouraging symptoms of vitality in many directions.

The Civil School at Oxford, for instance, conducted by Mr. Mackinder, has already made most successful efforts to produce expert teachers of geography. Here, in addition to 163 undergraduates attending courses during the past year,

five students have already won the Post-graduate Diploma granted by the University, and it is encouraging to note that four out of the five have already obtained distinctively geographical work. Others similarly qualified, if of sufficient ability, would probably not have long to wait for opportunities. In addition to its regular University functions, the Oxford school has this year organised a summer course of three weeks' study. This has been well attended by teachers and instructors from all parts of the country, and even from America.

In London a department of economic geography is in course of organisation at the School of Economics and Political Science, and geography will become a compulsory subject in examinations. In the matter of examinations we have to chronicle the issue of a most excellent syllabus for the new London Matriculation which should ultimately have great influence on the teaching in many schools.

Further, the 'Geographical Association,' a body now of several hundred teachers, has made great progress. It has recently commenced the issue of a journal known as the 'Geographical Teacher,' one of whose functions appears to be the criticism of the questions set in various public examinations.

In the University of Cambridge the interests of geography are doubtless not overlooked, but they are not conspicuously *en évidence*, and I have no trustworthy data of the progress made in their maintenance.

In military schools the report of the late Committee appointed to Consider the Education of Army Officers shows clearly enough that amongst all the necessary subjects for a cadet's education which have to be crammed into the exceedingly short course of his military schooling that branch of geography which is embraced by the term 'military topography' finds a very conspicuous place. The short course of a military school will never turn out an accomplished geographical surveyor; nor does it in any way outflank the necessity for a military school for professional topographers. But it teaches the young officer how maps are made, and instructs him in the use of topographical symbols. It would be well if it could be pushed a little further—if it could teach him how to make use of the maps when they *are* made—for personal experience convinces me that the apathy shown by many of our foremost generals and leaders on the subject of maps arises chiefly from a well-founded doubt of their own ability to make use of them. As for the broader basis of general geographical instruction which would deal with the distribution of important military posts and strategic positions throughout the Empire, and teach officers the functions of such positions, either individually or in combination, during military or naval operations, it is perhaps better that such a strategic aspect of geography should be relegated to a later age, when the average intelligence of the cadet has become more fully developed.

Taking it for all in all there are distinct signs of a more general interest and more scholarly standard of thought in the subject of geography. This is probably due to the efforts of a comparatively small group of workers at a time of general educational reform, possibly partly stimulated by the disclosures in connection with the late war.

The methods of further improvement are simple—better teachers and better examining—and for both it is probable that we must look more directly to civil sources than to the tentative efforts of the military schools.

British Association for the Advancement of Science.

BELFAST, 1902.

ADDRESS

TO THE

ECONOMIC SCIENCE AND STATISTICS SECTION

BY

EDWIN CANNAN, M.A., LL.D.,

PRESIDENT OF THE SECTION.

If it happened every year that the President of this Section undertook to justify his own existence, I am afraid the Section would become weary. But my four distinguished predecessors have all been drawn from the Civil Service, and though each of us may have doubts about particular branches of the Civil Service, we are mostly willing to allow that as a whole it is at least a necessary evil, so that we do not get apologies from the Presidents who, so to speak, represent the practice of political economy. I hope, therefore, that you will bear with me if I offer some reasons for thinking that the teaching and study of the theory of economics is not, as many people seem to suppose, a wholly unnecessary evil, but, on the contrary, a thing of very great practical utility.

I do not mean to argue that a knowledge of economic theory will enable a man to conduct his private business with success. Doubtless many of the particular subjects of study which come under the head of economics are useful in the conduct of business, but I doubt if economic theory itself is. It does not indeed in any way disable a man from successful conduct of business; I have never met a decent economist who was in a position of pecuniary embarrassment, and many good economists have died wealthy. But economic theory does not tell a man the exact moment to leave off the production of one thing and begin that of another; it does not tell him the precise moment when prices have reached the bottom or the top. It is, perhaps, rather likely to make him expect the inevitable to arrive far sooner than it actually does, and to make him underrate, not the foresight, but the want of foresight of the rest of the world.

The practical usefulness of economic theory is not in private business but in politics, and I for one regret the disappearance of the old-name 'political economy,' in which that truth was recognised.

One of the commonest complaints of the time is that there is no text-book of economics which commands any really wide approval, and you may therefore, I think, fairly ask me to explain what I mean by the teaching and study of economic theory before I undertake to prove its practical usefulness in the discussion of legislative and administrative measures. I will therefore endeavour to sketch as shortly as possible the course of instruction which the modern teacher of economic theory, if unhampered by too close adherence to traditional standards, puts before those who come to him for instruction.

The first, or almost the first, thing he will do is to try to open the eyes of his pupils to the wonderful way in which the people of the whole civilised world now co-operate in the production of wealth. He may perhaps read them Adam Smith's famous description of the making of the labourer's coat, a description which

required three generations and three great writers to elaborate in the form in which we know it. Or he will ask them to consider the daily feeding of London. There are, he will point out, six millions of people in and about London, so closely packed together that they cannot grow anything for their own consumption, and yet every morning their food arrives with unfailing regularity, so that all but an infinitesimal fraction of them would be extremely surprised if they did not find their breakfast ready to hand. To prepare it they use coal which has been dug from great depths hundreds of miles away in the Midlands or Durham; in consuming it they eat and drink products which have come from Wiltshire, Jamaica, Dakota, India, or China, with no more thought than an infant consuming its mother's milk. It is clear that there is in existence some machinery, some organisation for production which, in spite of occasional failures here and there, does its work on the whole with extraordinary success. It is easy to be pessimistic, especially when the weather is damp, and we are apt to concentrate our attention, and to endeavour to make others concentrate their attention, on this or that defect, and to forget that the system is not made up of defects, but on the whole works very well. Imagine the report of a really outside observer. In all civilised planets, I have no doubt, there must be an institution more or less resembling the British Association. An economist in Mars, let us say, has been favoured with a glimpse of this island through a new mammoth telescope of sufficient power to let him see us walking about, and he is reporting to Section F what he saw. Will he say that he saw a confused scramble for the scanty natural products of the earth? That most people were obviously in a state of starvation? That few had clothes? And that scarcely any were housed? No, truly; he will be much more likely to report that he saw a wonderfully orderly population, going to and from its work with amazing regularity, without a sign of compulsion or unwillingness; that it appeared to be fed and clothed and housed in a way extraordinarily creditable on the whole to some mysterious organisation, the nature of which he could only guess at.

Having endeavoured to make his pupils recognise that we are organised, and that the organisation works, the teacher will go on to show how it works: why things that are wanted are produced in the places where they can be easiest produced and taken to the places where it is most convenient to consume them; why people go to live in large numbers in spots where it is desirable they should work, and leave great areas sparsely inhabited; why more people are brought up to follow an occupation when the desire for its products increases, and fewer when it decreases; why if the harvest is short the consumption is economised so as to spread it over the year; and so on. The answer to all these questions is of course 'self-interest' or 'the hope of gain.' Durham coal, Wiltshire milk, Danish butter, Jamaica sugar, Dakota wheat, and China tea go to London because it pays to send them there. People congregate in London or Belfast because it pays them to work there. More do not come, because it would not pay them. Young people leave agriculture and go to towns to make agricultural implements or bicycles because it pays. The consumption of grain is economised and spread over the year because it pays to hold the stock. If people with one accord left off doing what paid we should all be dead in two months.

The reasons why it pays to do the right thing—to do nearly what an omniscient and omnipotent benevolent Inca would order to be done—are to be looked for in the laws of value. This used to be regarded as a somewhat arid subject, but the discussions of recent years, especially the contribution made by Jevons and the Austrian school, have fertilised it. Long ago, economists pointed out how the much-abused corn-dealer who held out for a higher price saved the people from starvation; and we now, thanks to the theory of final utility, not only know that it is a fact, but also why it is a fact, that value rises with the extent and urgency of demand, so that when a thing is much wanted much is offered to those who produce it, or are ready to part with it, and consequently its production is stimulated or its consumption economised, as need be.

This will naturally lead to the question of distribution—the question, that is, why much of the produce falls to the share of one individual and little to that of another:

why, in a word, some are rich and others poor. The teacher will here explain that the share of each person depends on the amount and value of his contribution to production, whether that contribution be labour or the use of property. He will show how this system of distribution is essential to the existing system of production, where no man is compelled to work or to allow his property to be used by others, and where every man has legal freedom to choose his own occupation and the uses to which he will put his property. He will beware of claiming for it that it is just in the sense in which justice is understood in the nurseries where jam is given when the children are good. There is, he will explain, no claim on behalf of the system that it rewards moral excellence, but only that it rewards economic service. There is no claim that economic service is meritorious. Whether a man can and does perform valuable economic service does not by any means depend entirely on his own volition. His valuable property may have come to him by bequest or inheritance; his incapacity to do any but the least valuable work may be the result of conditions over which he has had no control. The system exists not because it is just, or to reward merit, but because it is inextricably mixed up with the system of production. It has one great evil—its inequality. Moralists and statesmen have long seen the evils of great inequality of wealth, and now, thanks to modern discoveries in economic theory, the economist is able to explain that it is wasteful, that it makes a given amount of produce less useful, because each successive increment of expenditure yields, as a rule, less enjoyment to the spender. The teacher will go on to show how this organisation of production and distribution is made possible by the order enforced by government, and how, in various ways, government supplements or modifies it; but I shall not enlarge upon this part of the teaching of economics, as its practical usefulness is obvious. My theme is the usefulness of the other part, the explanation of the organisation of production and distribution in so far as it depends on separate property, free labour, and the consequent action of self-interest.

In the first place, I maintain that the widespread dissemination of such teaching would help to do away with a vast amount of most disastrous obstruction of necessary and desirable changes. Take, for example, the obstruction offered to changes in international trade. Of course every conceivable argument has been used by different writers in wholly different circumstances for obstructing the co-operation of mankind in production, as soon as it oversteps a national boundary. But what is the real support of this kind of obstruction? Obviously the fact that certain producers, or owners of certain means of production, are damaged by an increase in the importation of a particular article. Their loss, their suffering, if their loss is severe enough to deserve that name, appeals to popular compassion, and their request for 'protection' is easily granted, the new trade is nipped in the bud, and things are forced to remain in their accustomed channels. The same principle is not applied as between county and county or between province and province, simply because there is then visible to everyone an opposing interest, the interest of the new producers, within the hallowed pale of the national boundary. Adam Smith tells us that when the great roads into London were improved, some of the landlords in the home counties protested on the ground that the competition of the more distant counties would reduce their rent. The home counties did not get the protection they wanted, because it was obviously to the interest of the more distant counties that they should not have it. These two interests being balanced, the interest of the consumer, London, turned the scale. So it usually happens that beneficial changes in internal trade are allowed to take their course without obstruction because the votes of two sets of producers counteract each other, and the consumer's interest settles the question. But in international trade one of the two sets of producers is outside the country: it consists of hated foreigners, the fact that it will benefit is an argument against rather than for the threatened change in trade, and the consumers therefore feel it patriotic to sacrifice their own interest and vote for protection. But if they were properly instructed in economic theory they would see at once that such magnanimity is entirely misplaced. They would see that it would cut away all international trade, since, if there were no fallacy involved in it, the stoppage of each import taken separately would benefit home

producers and damage foreign producers. Even if some of the imported commodities could not be produced at all at home, substitutes, more or less efficient, could be produced and give all the more employment. Having acquired some notion of the advantages of co-operation and the territorial division of labour, the consumers would regard this as a *reductio ad absurdum*, and after thinking a little further they would soon see that, after all, there is another set of producers, actual or potential, within the country who will gain—namely, the producers, present or future, who will supply the articles which are to go abroad in exchange for the new import. They will see that what they are asked to do is not to maintain the amount of national production, but merely to prevent a change in its character which will be accompanied by an increase in its amount.

Take another example of Chinese obstructiveness to desirable change. As great cities grow, it becomes convenient that their centres should be devoted to offices, warehouses, and shops, and that people who work in these places, and still more their families, should live in the outskirts. I do not know that anyone has denied this. Certainly the great majority are willing to admit it. At one time it is believed that a quarter of a million people lived in the square mile comprised within the City of London; no one supposes that would be convenient now. There is no reason to suppose that further change in the same direction will not be desirable in the future. Yet, incredible as it will appear to future generations, public opinion, the House of Commons, the London County Council, and some town councils think, or at any rate act as if they thought, that the process has now gone far enough and ought to be stopped; as if the state of things reached about the year 1891 was to be permanent, to last for ever and ever. Private owners are indeed still allowed to pull down dwelling-houses and erect shops and offices, but they are abused for doing so, and their liberty is at least threatened. But if a new railway or a new street is made—in all probability with the intention of increasing the accessibility of the centre from the suburbs—if even a new London Board School is built, and houses inhabited by persons who have less than a certain income are pulled down in any of these processes, it is required by law or parliamentary resolution that other houses for these people must be built in the neighbourhood. So it comes about that there are in quarters of London most unsuitable for the purpose enormous and repulsive barrack dwellings, the sites of which are devoted in *secula seculorum* to the housing of the working classes; while the immense cost of devoting them to this instead of to their proper purpose is debited to the cost of improving the facilities for locomotion or to education, and is defrayed principally by the rates on London property, which chiefly consists of houses, and to some extent by the higher charges on the railways consequent on the restriction of facilities for extension. Fifty pounds a head is the average loss involved to the rates of London on every man, woman, and child for whom these dwellings are provided. Such is the wisdom of practical men uninformed by instruction in economic theory.

This palpable absurdity could never have been perpetrated if the general working of the economic organisation had been understood. In that case it would have been seen at once that the extrusion of over 200,000 inhabitants from the City of London in the past, which is admitted to have been desirable, was effected by the quiet operation of the laws of value. It would have been seen that as it became desirable to turn the City to other purposes, the ground in the City became too valuable to use as bedrooms and as living-rooms for mothers and children, and this increase of value drove out the 200,000 inhabitants. It would have been seen that the change had not come to an end, and no responsible body would have dreamt of putting themselves in opposition to it by buying sites and writing them down to 2 per cent. of their actual value in order that they might be tied up for ever and ever to be the homes of a certain number of persons with less than a certain income. If some unusually dense individual who had failed after many attempts to pass his examination in economic theory had proposed the policy which has been adopted, he would have been asked two questions: first, 'What peculiar sanctity is there about the position occupied in the closing years of the nineteenth century? Why should

this be stereotyped for all time? Why should not the position at the end of the seventeenth century have been maintained? Why should we not endeavour to restore the working classes to their old home in the City, and remove the Bank of England to Tooting?' Secondly, 'Whom do you imagine you will benefit by the policy you propose?'

It is difficult to conceive of any answer to the first question. To the second the reply of the dunce would of course be that he thought the policy proposed would benefit the people housed on these expensive sites. This answer would at once be condemned as unsatisfactory. To build houses on land worth 100,000*l.*, and let them to the first-comers of respectable antecedents at rents which would pay if the land were worth 2,000*l.*, would be a very stupid sort of almsgiving if these respectable first-comers actually got the difference between the interest on the 100,000*l.* and the 2,000*l.* But no one supposes that they do get this difference or any considerable part of it. The difference is almost entirely pure loss to the community. The chief immediate effects of the policy are, first, to retain in the centre the men, women, and children who inhabit the dwellings; secondly, to retain other workers who perform various offices for these inhabitants; and thirdly, to ensure a supply of labour for factories which would otherwise (to the advantage of everyone concerned) be driven into the country by the pressure of the high wages necessary to bring workmen to the centre or to pay their house rent if they lived there.

So much for the utility of economic theory in preventing obstruction of desirable changes. My second claim on its behalf is that it serves to hinder the adoption of specious but illusory projects. This, I think, may be illustrated by examples closely connected with those which we have already considered under the head of obstruction.

The people who are most anxious to obstruct changes in the channels of trade which are coming about of themselves because they are profitable, are often extremely anxious to promote changes which will not come about of themselves because they are not profitable. For this end one of their most favourite devices at present is a State or municipal subsidy to locomotion or transport between particular points. So we have shipping subsidies, free grants to light railways, the construction of unprofitable telegraph lines by the post office, and the advocacy, at any rate, of the construction of unprofitable tramways by municipalities. The practical man, uninstructed in economic theory, feels uneasy about such projects because he does not see where he is to stop, and he feels obscurely that a universal subsidisation would mean ruin. But he does not see why he should not go a little way, and he goes sufficiently far to involve a loss quite worth considering. A knowledge of economic theory would come to his assistance by showing him that, as a rule, the most profitable enterprises are those which it is most desirable to undertake first, and that the subsidisation of the less profitable does not create new enterprises, but merely changes the order from the more desirable to the less desirable. I suppose that if in 1830 Parliament had offered a sufficient subsidy a railway might have been at once made and worked from Fort William to Fort Augustus, to the great satisfaction of the inhabitants of Fort Augustus and the intermediate places. But it is obvious that it was more desirable, in the interests of the whole community, that the railway from Fort William to Fort Augustus should wait for seventy years, and that the railway from Manchester to Liverpool, and many others, should be made first.

Then, too, we find people who are not quite so stupid as to think the working classes should always remain in the places where they were at the end of the nineteenth century, alleging that the way to cure overcrowding is for local authorities to enter the building trade in a general way, and build houses inside or outside their districts, wherever it seems most convenient. To the mind uninstructed in economic theory it seems obvious that the larger amount of housing there is the less overcrowding there will be, and that the more housing local authorities provide the more housing there will be. Economic theory, with its explanation of the general working of the organisation of production, suggests two objections. First, an addition to the housing in any locality will not be effectual in diminishing

overcrowding, in so far as it attracts new inhabitants to the spot; a policy which assumes that the comparative plentifulness of houses is not a factor in the determination of the enormous and perpetual migration of people from place to place which is indicated in the tables of birthplaces and births and deaths in the census, is doomed to failure. Secondly, economic theory suggests the reflection that the mere fact of a local authority building some houses will not cause the whole number to be greater, if for every house built by the local authority one less is built by private enterprise, and that this is very likely to happen. Houses have been built by private enterprise in the past, and in these houses nearly the whole population is at present housed. I have seen an enthusiast for municipal housing stand in the empty streets of a town late at night, when every soul in the town was evidently housed, and say, in a tone of conviction, 'Private enterprise has failed.' In that town four small houses had been built by municipal enterprise and more than ten thousand by private enterprise, and private enterprise was adding hundreds every year, while the housing committee of the corporation was meeting once a year to re-elect its chairman. Is it likely that private enterprise will build as much when it is competed with or supplemented by—the term does not matter—municipal enterprise? Why should it? If the municipality turned baker, would the private bakers continue to bake as much bread? Is not the attempt to stop overcrowding by inducing local authorities to build houses exactly the same thing and just as absurd as it would be to attempt to cure under-feeding by opening municipal butchers' and bakers' shops?

In the long run, I admit, experience teaches. Protection has fallen once in this country, and I have little doubt that it will fall again if it becomes considerable. The policy of obstructing the removal of dwellings from the centre of a great city already excites opposition in the London County Council, though unanimity still reigns in those last homes of extinct superstitions, the Houses of Parliament. Chancellors of the Exchequer and finance committees may be trusted to offer a stout resistance, on what they call financial grounds, to any really great development of the system of subsidies. There is hope even that the municipal building policy may be checked by the laborious inquiries which show by statistics what everyone knows, that the poor are ill-fed and ill-clothed as well as ill-housed, and therefore lead people to consider how the poor may be made more able to pay for houses, among other things, instead of simply how houses may be built in the absence of an effective demand for them. But I claim that, in matters such as these, a more widespread appreciation of economic theory, and the quickened intelligence which that would produce, would save us much painful experience, many expensive experiments, and an enormous mass of tedious investigation.

Thirdly and, at any rate on the present occasion, lastly, I claim that the teaching and study of economic theory has great practical utility in promoting peace and good will between classes and nations.

Between classes within the same nation the peacemaking influence of economic theory lies chiefly in the fact that it tends to get rid of that stupid cry for 'rights' and 'justice' which causes and exacerbates industrial and commercial quarrels. When demand for some commodity falls, or supply from some new quarter arises, and profits and wages fall, the workers cry out that they are being unjustly treated, because they have the unfounded belief that reward is or ought to be proportional to moral merit, and they are not conscious of any diminution of their moral merit. They demand a living wage or a minimum wage and employment for all who happen to have been hitherto employed in the trade, rend the air with complaints, and get subscriptions from a compassionate but ill-informed public. We cannot, of course, expect people who suffer by them to regard even the most beneficial operations of the economic organisation with enthusiasm or even satisfaction. It would be absurd to do so. But all the same, it is true that a wider apprehension of the fact that it is only by raising and lowering the advantages offered by different employments that production is at present regulated so as to meet demand would not only diminish the dissatisfaction, but also, which is more important, diminish the actual suffering by causing transitions to be less obstinately resisted. The present fashion of deploring rapid changes of trade and

dwelling-place is a most unfortunate one; the ordinary forms of labour do not, as a matter of fact, require such specialised ability that there should be much difficulty in changing from one to another; and surely it is much better for a man to work at several different things at different places in the course of his life than to stick for ever in the same place, surrounded by the same objects, going through the same monotonous round of duties. Anything which will weaken the present obstructive sentiment and lead people to regard the necessity of a change of employment or residence as a temporary inconvenience rather than a cruel injustice is to be warmly welcomed.

It is not, however, only the poor and the industrious who would be taught by a greater knowledge of economic theory not to kick against very necessary pricks. The rich, both industrious and idle, would be taught to be far more tolerant than they are of attempts to diminish inequality of wealth by reducing the wealth of the rich as well as increasing that of the poor. The economist may be a little annoyed with the workman who insists that he ought to have thirty shillings a week for producing something worth fifteen shillings, or five shillings, or nothing at all, but he can only have hearty contempt for the millionaire who holds up his hands in holy horror and murmurs 'confiscation,' 'robbery,' 'eighth commandment,' when it is proposed to relieve him of a fraction of a farthing in the pound in order to bring up destitute orphans to an occupation in which they may earn twenty-five shillings a week. The sanguine teacher of economic theory has hopes of making even such a man see that he has his wealth, not because Moses brought it down from Sinai, or because of his own super-eminent virtue, but simply because it happens to be convenient, at any rate for the present, for society to allow him to hold it, whether he obtained it by inheritance or otherwise. In other words, that private property exists for the sake of production, not for the sake of the particular kind of distribution which it causes. Some, I know, say that the rich are so few that it does not much matter whether they acquiesce in the measure meted to them or not; but that is not the teaching of history, and I think you will agree with me that for the progress of the whole community it is, in practice, quite as important to secure the acquiescence of the rich as of the poor.

In regard to international relations, the first business of the teacher of economic theory is to tear to pieces and trample upon the misleading military metaphors which have been applied by sciolists to the peaceful exchange of commodities. We hear much, for example, in these days of 'England's commercial supremacy,' and of other nations 'challenging' it, and how it is our duty to 'repel the attack,' and so on. The economist asks what is 'commercial supremacy?' and there is no answer. No one knows what it means, least of all those who talk most about it. Is it selling goods dear? Is it selling them cheap? Is it selling a large quantity of goods in proportion to the area or of the country? or in proportion to its population? or absolutely, without any reference to its area or population? It seems to be a wonderful muddle of all these various and often contradictory ideas rolled into one. Yet what a pile of international jealousy and ill-feeling rests on that and equally meaningless phrases! The teacher of economic theory analyses or attempts to analyse these phrases, and they disappear, and with them go the jealousies suggested by them.

When misleading metaphors and fallacies are dismissed, we are left with the facts that foreign trade—the trade of an area under one government with areas under other governments—is merely an incident of the division of labour, and that its magnitude and increase are no measures of the wealth and prosperity of the country, but merely of the extent to which the country finds it convenient to exchange commodities of its own growth or manufacture for commodities produced elsewhere. If the city of York were made independent, and registered its imports and exports, they would come out far larger per head of population than those of the United Kingdom or any other great country. Should we be justified in concluding York to be far richer than any great country? If means were discovered of doubling the present produce of arable land with no increase of labour, much less corn would be imported into Great Britain and less of other goods would be exported to pay for it; the foreign trade of the country would

consequently be diminished, but would the people be any less prosperous? What jealousies, heart-burnings, and unfounded terrors leading to hatred would be extinguished if only these elementary facts were generally understood!

To anyone who has once grasped the main drift of economic theory, it will be plain that the economic ideal is not for the nation any more than for the family that it should buy and sell the largest possible quantity of goods. The true statesman desires for his countrymen, just as the sensible parent desires for his children, that they should do the best paid work of the world. This ideal is not to be obtained by wars of tariffs, still less by that much greater abomination, real war, with all its degrading accompaniments, but by health, strength and skill, honesty, energy, and intelligence.

British Association for the Advancement of Science.

BELFAST, 1902.

ADDRESS

TO THE

ENGINEERING SECTION

BY

PROFESSOR JOHN PERRY, M.E., D.Sc., LL.D., F.R.S.

PRESIDENT OF THE SECTION.

THIS Section has had sixty-six Presidents, all different types of engineer. As each has had perfect freedom in choosing the subject for his Address, and each has known of the rule¹ that Presidential Addresses are not subject to debate afterwards, and as, being an engineer, he has always been a man of originality, of course he has always chosen a subject outside his own work. An engineer knows that the great inventions, the great suggestions of change in any profession, come from outsiders. Lawyers seem like fish out of water when trying to act as law-makers. The radical change that some of us hope to see before we die in the construction of locomotives will certainly not come from a locomotive superintendent who cannot imagine a locomotive which is not somehow a lineal descendant of the Rocket.

Hence it is that in almost every case the President of this Section has devoted a small or large part of his Address to the subject of the education of engineers. I grant that every President has devoted his life to the education of one engineer—himself—and it is characteristic of engineers that their professional education proceeds throughout the whole of their lives. Perhaps of no other men can this be said so completely. To utilise the forces of Nature, to combat Nature, to comprehend Nature as a child comprehends its mother, this is the pleasure and the pain of the engineer.² A mere scientific man analyses Nature: takes a phenomenon, dissects it into its simpler elements, and investigates these elements separately in his laboratory. The engineer cannot do this. He must take Nature as she is, in all her exasperating complexity. He must understand one of Nature's problems as a whole. He must have all the knowledge of the scientific man, and ever so much more. He uses the methods of the scientific man, and adds to them methods of his own. The name given to these scientific methods of his own or their results is sometimes 'common-sense,' sometimes 'character,' or 'individuality,' or 'faculty,' or 'business ability,' or 'instinct.' They come to him through a very wide experience of engineering processes, of acquaintance with things and men. No school or

¹ The Committees of Sections G and L have arranged a discussion on 'The Education of Engineers,' this Address being regarded as opening the discussion. Thus the rule is not in force this year.

² Of all the unskilled labour of the present day, surely that of the modern poet is the most grotesque. How much more powerful and powerless man seems to us now; how much more wonderful is the universe than it was to the ancients! Yet our too learned poets prefer to copy and recopy the sentiments of the ancients rather than try to see the romance which fills the lives of engineers and scientific men with joy.

college can do more than prepare a young man for this higher engineering education which lasts through life. Without it a man follows only rule of thumb, like a sheep following the bell-wether, or else he lets his inventiveness or love of theory act the tyrant.

When a man has become a great engineer, and he is asked how it happened, what his education has been, how young engineers ought to be trained, as a rule it is a question that he is least able to answer, and yet it is a question that he is most ready to answer. He sees that he benefited greatly by overcoming certain difficulties in his life; and forgetting that every boy will have difficulties enough of his own, forgetting that although a few difficulties may be good for discipline many difficulties may be overwhelming, forgetting also that he himself is a very exceptional man, he insists upon it that those difficulties which were personal to himself ought to be thrown in the path of every boy. It often happens that he is a man who is accustomed to think that early education can only be given through ancient classics. He forgets the dullness, the weariness of his schooldays. Whatever pleasure he had in youth—pleasure mainly due to the fact that the average Anglo-Saxon boy invents infinite ways of escaping school drudgery—he somehow connects with the fact that he had to learn classics. Being an exceptional boy, he was not altogether stupefied, and did not altogether lose his natural inclination to know something of his own language; and he is in the habit of thinking that he learnt English through Latin, and that ancient classics are the best mediums through which an English boy can study anything.¹ The cleverest men of our time have been brought up on the classics, and so the engineer who cannot even quote correctly a tag from the Latin grammar, who never knew anything of classical literature, insists upon it that a classical education is essential for all men. He forgets the weary hours he spent getting off Euclid, and the relief it was to escape from the class-room not quite stupefied, and he advocates the study of pure mathematics and abstract dynamics as absolutely necessary for the training of the mind of every young engineer. I have known the ordinary abominable system of mathematical study to be advocated by engineers who, because they had passed through it themselves, had really got to loathe all kinds of mathematics higher than that of the grocer or housekeeper. They said that mathematics had trained their minds, but they did not need it in their profession. There is no profession which so much requires a man to have the mathematical tool always ready for use on all sorts of problems, the mathematical habit of thought the one most exercised by him; and yet these men insist upon it that they can get all their calculations done for them by mathematicians paid so much a week. If they really thought about what they were saying, it would be an expression of the greatest contempt for all engineering computation and knowledge. He was pitchforked into works with no knowledge of mathematics, or dynamics, or physics, or chemistry, and, worse still, ignorant of the methods of study which a study of these things would have produced; into works where there was no man whose duty it was to teach an apprentice; and because he, one in a thousand, has been successful, he assures us that this pitchforking process is absolutely necessary for every young engineer. He forgets that the average boy leaves an English school with no power to think for himself, with a hatred for books, with less than none of the knowledge which might help him to understand what he sees, and he has learnt what is called mathematics in such a fashion that he hates the sight of an algebraic expression all his life after.

I do not want to speak of boys in general. I want only to speak of the boy who may become an engineer, and before speaking of his training I want to mention his essential natural qualification—that he really wishes to become an engineer. I take it to be a rule to which there are no exceptions that no boy ought to enter a profession—or, rather, to continue in a profession—if he does not love it. We all know the young man who thinks of engineering things during office hours and

¹ The very people who talk so much of learning-English through Latin neglect in the most curious ways those Platt-Deutsch languages, Dutch and Scandinavian, a knowledge of which is ten times more valuable in the study of what is becoming the speech of the world. And how they do scorn Lowland Scotch!

never thinks of them outside office hours. We know how his fond mother talks of her son as an engineer who, with a little more family influence and personal favour, and if there was not so much competition in the profession, would do so well. It is true, family influence may perhaps get such a man a better position, but he will never be an engineer. He is not fit even to be a hewer of wood and drawer of water to engineers. Love for his profession keeps a man alive to its interests all his time, although, of course, it does not prevent his taking an interest in all sorts of other things as well; but it is only a professional problem that warms him through with enthusiasm. I think we may assume that there never yet was an engineer worth his salt who was not fond of engineering, and so I shall speak only of the education of the young man who is likely to be fond of engineering.

How are we to detect this fondness in a boy? I think that if the general education of all boys were of the rational kind which I shall presently describe, there would be no great difficulty; but as the present academic want of system is likely to continue for some time, it is well to consider things as they are. Mistakes must be made, and the parent who tries during the early years of his offspring to find out by crafty suggestion what line his son is likely to wish to follow will just as probably do evil by commission as the utterly careless parent is likely to do evil by omission. He is like the botanical enthusiast who digs up plants to see how they are getting on. But in my experience the Anglo-Saxon boy can stand a very great deal of mismanagement without permanent hurt, and it can do no kind of boy any very great harm to try him on engineering for a while. Even R. L. Stevenson, whose father seems to have been very persistent indeed in trying to make an engineer of him against his will, does not seem, to a Philistine like myself, to have been really hurt as a literary man through his attendance on Fleeming Jenkins' course at Edinburgh—on the contrary, indeed. It may be prejudice, but I have always felt that there is no great public person of whom I have ever read who would not have benefited by the early training which is suitable for an engineer. I am glad to see that Mr. Wells, whose literary fame, great as it is, is still on the increase, distinguishes the salt of the earth or saviours of society from the degraded, useless, luxurious, pleasure-loving people doomed to the abyss by their having had the training of engineers and by their possessing the engineer's methods of thinking.

It may be that there are some boys of great genius to whom all physical science or application of science is hateful. I have been told that this is so, and if so I still think that only gross mismanagement of a youthful nature can have produced such detestation. For such curious persons engineering experience is, of course, quite unsuitable. I call them 'curious' because every child's education in very early years is one in the methods of the study of physical science; it is Nature's own method of training, which proceeds successfully until it is interfered with by ignorant teachers who check all power of observation, and the natural desire of every boy to find out things for himself. If he asks a question, he is snubbed; if he observes Nature as a loving student, he is said to be lazy and a dunce, and is punished as being neglectful of school work. Unprovided with apparatus, he makes experiments in his own way, and he is said to be destructive and full of mischief. But however much we try to make the wild ass submit to bonds and the unicorn to abide by the crib, however bullied and beaten into the average schoolboy type, I cannot imagine any healthy boy suffering afterwards by part of a course of study suitable for engineers, for all such study must follow Nature's own system of observation and experiment. Well, whether or not a mistake has been made, I shall assume the boy to be likely to love engineering, and we have to consider how he ought to be prepared for his profession.

I want to say at the outset that I usually care only to speak of the average boy, the boy usually said to be stupid, ninety-five per cent. of all boys. Of the boy said to be exceptionally clever I need not speak much. Even if he is pitchforked into works immediately on leaving a bad school, it will not be long before he chooses his own course of study and follows it, whatever course may have been laid down for him by others. I recollect that when in 1863 I attended an evening class held in the Model School, Belfast, under the Science and Art Department, on Practical Geometry and Mechanical Drawing, there was a young man attending

it who is now well known as the Right Honourable William J. Pirrie. He had found out for himself that he needed a certain kind of knowledge if he was to escape from mere rule-of-thumb methods in shipbuilding work; it could at that time be obtained nowhere in the North of Ireland except at that class, and of course he attended the class. For forty-two years the Science and Art Department, which has recently doubled its already great efficiency, has been giving chances of this kind to every clever young man in the country, from long before any Physical Science was taught in any English public school.¹ The one essential thing for the exceptional boy is that he shall find within his reach chances to take advantage of; chances of learning; chances of practice; and, over and above all, chances of meeting great men. It takes me off my subject a little, but I should like here to illustrate this matter from my own personal experience.

I had already been an apprentice for four years at the Lagan Foundry when I entered Queen's College for a course of Civil Engineering. I suppose that there never was on this earth a college so poorly equipped for a course of engineering study. Even the lecture room—this lecture room in which you are now sitting—was borrowed from the Physics Professor. There was a narrow passage, ironically called a 'Drawing Room,' and this was the only space reserved for engineering in a town whose engineering work was even then very important. There were some theodolites and levels and chains for surveying, but nothing else in the way of apparatus. But there was as Professor a man of very great individuality; he acted as President of this Section twenty-eight years ago. I can hardly express my obligations to Professor James Thomson. It was my good fortune to be a pupil both of this great man and of his younger brother Lord Kelvin, as well as of Dr. Andrews. It is not because these three men were born in Belfast that we here call them great. It is not because Tait, late of Edinburgh, and Parser, now the President of Section A, were professors at this College that we call them great. All the scientific men of the world are agreed to call these men very great indeed. To come in contact with any of them, even for a little while, as a student, altered for ever one's attitude to Nature. It was not that they gave us information, knowledge, facts. The syllabuses of their courses of study were nothing like so perfect as that of the smallest German polytechnic. And yet if a youth with a liking for physical science had gone to a German Gymnasium to the age of nineteen, and had become a walking encyclopedia on leaving one's polytechnic at the age of twenty-four, the course of that life-study would not have done for him as much good as was done by a month's contact with one of these men. People call it 'personal magnetism,' and think there is something occult about it. In truth, they revealed to the student that he himself was a man, that mere learning was unimportant, that one's own observation of some common phenomenon might lead to important results unknown to the writers of books. They made one begin to think for oneself for the first time. Let me give an example of how the thing worked.

James Thomson was known to me as the son of the author of my best mathematical books, but more particularly as the man who had first used Carnot's principle in combination with the discovery of Joule, and I often wondered why Rankine and Clausius and Kelvin got all the credit of the discovery of the second law of thermodynamics. Men think of this work of his merely as having given the first explanation of regelation of ice and the motion of glaciers. He was known to me as the inventor of the Thomson Turbine and Centrifugal Pump and Jet Pump. His name was to be found here and there in all my text-books, always in connection with some thoroughly well-worked-out investigation, as it is to be found in all good text-books now; for wherever he left a subject, there that subject has remained until this day; nobody has added to it or found a mistake in it. He was to me a very famous man, and yet he treated me as a fellow-

¹ I once stated that my workshop at Clifton College in 1871 was the first school workshop in England. I understand that this is a mistake; there had been a workshop at Rossall for some years. But I believe I am right in saying that my physical laboratory at Clifton was the first school laboratory in England. These ideas were not mine: they were those of the Headmaster, now the Bishop of Hereford

student. One of his early lectures was about flowing water, and he told us of a lot of things he had observed, which I also had observed without much thought; and he showed how these simple observations completely destroyed the value of everything printed in every text-book on the subject of water flowing over gauge-notches, even in the otherwise very perfect Rankine. I felt how stupid I had been in not having drawn these conclusions myself, but in truth till then I had never ventured for a moment to criticise anything in a book. I have been a cautious critic of all statements in text-books ever since. If any engineer wants to read what is almost the most instructive paper that has ever been written for engineers, let him refer to the latest paper written by James Thomson on this subject.¹ The reasoning there given was given to me in lectures in this very room in 1868, and had been given to students for many years previous.

Again, soon afterwards, he let me see that although I had often looked at the whirlpool in a basin of water when the central bottom hole is open, and although I had read Edgar Allen Poe's mythical description of the Maelstrom, I had been very much too careless in my observation. Among other things, Thomson had observed that particles of sand gradually passed along the bottom towards the hole. When he found out the cause of this, it led him at once to several discoveries of great importance. Indeed, the study of this simple observation gave rise to all his work on (1) What occurs at bends of pipes and channels, and why rivers in alluvial plains bend more and more; (2) The explanation of the curious phenomena that accompany great forest fires; (3) The complete theory of the great wind circulation of the earth, published in its final form as the Bakerian Lecture of the Royal Society in 1892.

But why go on? He taught me to see that the very commonest phenomenon had still to reveal important secrets to the understanding eye and brain, and that no man is a true student unless he is a discoverer. And so it was with Kelvin and Andrews. Their names were great before the world, and yet they treated one as a fellow-student. Is any expenditure of money too large if we can obtain great men like these for our Engineering Colleges? Money is wanted for apparatus, and more particularly for men, and we spend what little we have on bricks and mortar!

The memory of a man so absolutely honest as Professor James Thomson was compels me to say here that I was in an exceptionally fit state to benefit by contact with him, for I hungered for scientific information.² I do not think that there was so much benefit for the average student whose early education had almost unfitted him for engineering studies. To work quantitatively with apparatus is good for all students, but it is absolutely necessary for the average student, and, as I said before, there was no apparatus. Also the average student cannot learn from lectures merely, but needs constant tutorial teaching, and the Professor had no assistant.

Anybody who wants to know what kind of engineering school there ought to be

¹ *Brit. Assoc. Report*, 1876, pp. 243-266.

² Some of our most successful graduates went direct to works from the Model School, Belfast, and afterwards attended this College. No school in the British Islands could have given better the sort of general education which I recommend for all boys. English subjects were especially well taught, so that boys became fond of reading all manner of books. There were good classes in freehand and machine drawing, classes in chemistry and physics (at that time I believe that there were no such classes in any English public school), and the teaching of mathematics was good. Some of the masters started classes also under the Science and Art Department. Some of the masters had much individuality, and there was no outside examination to restrain it; there was only encouragement. Evidence has been given before a committee of the London School Board as to the excellence of the teaching at this school forty years ago. Foreign languages were not in the regular curriculum, but they could be studied by boys inclined that way; and in my opinion this is the position that all languages other than English ought to take in any British school. With such preparation a boy was eager and able to understand what went on in engineering works from his first day there.

in such a college as this can see excellent specimens (sometimes several in one town) in Glasgow, Birmingham, Liverpool, London, Manchester, Leeds, Bristol, Nottingham, Edinburgh, and other great cities. There the fortunate manufacturers have given many hundreds of thousands of pounds for instruction in applied science (engineering). In America the equipment of such schools is much more thorough, and there are large staffs of teachers, for fortunate Americans have contributed tens of millions of pounds for this kind of assistance to the rising generation. Germany and Switzerland compete with America in such preparation for supremacy in manufacture and engineering, and nearly every country in the world is more and more recognising its importance as they see the great inventions of Englishmen like Faraday and Perkin and Hughes and Swan developed almost altogether in those countries which believe in education. Even one hundred thousand pounds would provide Queen's College, Belfast, with the equipment of an engineering school worthy of its traditions and position, and Belfast is a city in which many large business fortunes have been made.

It is interesting to note that the present arrangements of the Royal University of Ireland, with which this College is affiliated, are such that most of the successful graduates in engineering of Queen's University would now be debarred from taking the degree. Even in London University, Latin is not a compulsory subject for degrees in science; Ireland has taken a step backwards towards the Middle Ages at the very time when other countries are stepping forward.

Well-equipped schools of applied science are getting to be numerous, but I am sorry to say that only a few of the men who leave them every year are really likely to become good engineers. The most important reason for this is that the students who enter them come usually from the public schools; they cannot write English; they know nothing of English subjects; they do not care to read anything except the sporting news in the daily papers; they cannot compute; they know nothing of natural science; in fact, they are quite deficient in that kind of general education which every man ought to have.

I am not sure that such ignorant boys would not benefit more by entering works at once than by entering a great engineering school. They cannot follow the College courses of instruction at all, in spite of having passed the entrance examination by cramming. Whereas after a while they do begin to understand what goes on in a workshop; and if they have the true engineer's spirit, their workshop observation will greatly correct the faults due to stupid schoolwork.¹

Perhaps I had better state plainly my views as to what general education is best for the average English boy. The public schools of England teach English through Latin, a survival of the time when only special boys were taught at all, and when there was only one language in which people wrote. Now the *average* boy is also taught Latin, and when he leaves school for the army or any other pursuit open to average boys he cannot write a letter, he cannot construct a grammatical sentence, he cannot describe anything he has seen. The public-school curriculum is always growing, and it is never subtracted from or rearranged. There is one subject which ordinary schoolmasters can teach well—Latin.²

¹ When I was young I remember that there were many agricultural colleges in Ireland; they have all but one been failures. Why? Because the entering pupils were not prepared by early education to understand the instruction; this had done as much as possible to unfit them.

² Only one subject—Latin—is really educational in our schools. I do not mean that the average boy reads any Latin author after he leaves school, or knows any Latin at all ten years after he leaves school. I do not mean that his Latin helps him even slightly in learning any modern language, for he is always found to be ludicrously ignorant of French or German, even after an elaborate course of instruction in these languages. I do not mean that his Latin helps him in studying English, for he can hardly write a sentence without error. I do not mean that it makes him fond of literature, for of ancient literature or history he never has any knowledge except that Cæsar wrote a book for the third form, and on English literature his mind is a blank. But I do mean that as the ordinary public-school master is really able to give a boy easy mental exercises through the study of Latin, this subject is in quite a

The other usual nine subjects have gradually been added to the curriculum for examination purposes; they are taught in water-tight compartments—or, rather, they are only crammed, and not taught at all. Our school system resembles the ordinary type of old-established works, where gradual accretion has produced a higgledy-piggledy set of shops which one looks at with stupefaction, for it is impossible to get business done in them well and promptly, and yet it seems impossible to start a reform anywhere. What is wanted is an earthquake or a fire—a good fire—to destroy the whole works and enable the business to be reconstructed on a consistent and simple plan. And for much the same reason our whole public-school system ought to be ‘scrapped.’ What we want to see is that a boy of fifteen shall be fond of reading, shall be able to compute, and shall have some knowledge of natural science; or, to put it in another way, that he shall have had mental training in the study of his own language, in the experimental study of mathematics, and in the methods of the student of natural science. Such a boy is fit to begin any ordinary profession, and whether

different position from that of the others. If any proof of this statement is wanted, it will be found in the published utterances of all sorts of men—military officers, business men, lawyers, men of science, and others—who, confessedly ignorant of ‘the tongues,’ get into a state of rapture over their school experiences and the efficiency of Latin as a means of education. All this comes from the fact, which schoolboys are sharp enough to observe, that English schoolmasters can teach Latin well, and they do not take much interest in teaching anything else. It is a power inherited from the Middle Ages, when there really was a simple system of education. I ask for a return to simplicity of system. English (the King’s English; I exclude Johnsonese) is probably the richest, the most complex language, the one most worthy of philologic study; English literature is certainly more valuable than any ancient or modern literature of any one other country, yet admiration for it among learned Englishmen is wonderfully mixed with patronage and even contempt. At present, is there one man who can teach English as Latin is taught by nearly every master of every school? Just imagine that English could be so taught by teachers capable of rising to the level of our literature!

I have often to give advice to parents. I find the average parent exceedingly ignorant of his son’s character or inclinations or ability. He pays a schoolmaster handsomely for taking his son off his hands except during holidays. During the holidays, so terrible to a parent, he sees his son as little as possible. One question always asked is: Do you think it better to have ‘theoretical’ instruction (they always call it by this absurd name) before or after an actual apprenticeship in works? Of course, such a question cannot be answered offhand. You tell the parent, to his great astonishment, that you must see the boy himself. When at length you see him, the chances are that you will find him to be what the schoolmasters are making of all our average boys. No part of his school work has been a pleasure to him, and, although he has had to work hard at his books, not one of the above three powers is his—power to use books and to write his own language; the language of his nurse, his mother, his mistress that is to be, his enemies and friends; the only language in which he thinks—power to compute and a liking for computation—power to understand a little of natural phenomena. Honestly I practically never find that such a boy has had any education at all except what he has obtained at home or from his school companions or from his sports. Even his sports are to keep him healthy of body only and not at all to cultivate his mental powers. Those old games like ‘Prisoners’ Base,’ which really developed in a wonderful way not only all the muscles of the body, but also the thinking power, are scorned in the public schools. Think now how such a boy is handicapped if we pitchfork him into works where it is nobody’s duty to teach him anything, or send him to college, where he cannot understand the lectures. Of course, if he is very eager to be an engineer he will, by hook or by crook, get to understand things. I have met some such men—clever, successful engineers in spite of all sorts of adverse circumstances—but the best of them are willing to admit that they are, and have always been, greatly hurt by the absence of the three powers which I have specified. And if this has been so in the past, when the scientific principles underlying engineering have been simple, how much more so is it now, when every new discovery in physics is producing new branches of engineering!

he is to enter the Church, or take up medicine or surgery, or become a soldier, every boy ought to have this kind of training. When I have advocated this kind of education in the past I have usually been told that I was thinking only of boys who intend to be engineers; that it was a specialised kind of instruction. But this is very untrue. Let me quote from the recommendations of the 1902 Military Education Committee (Report, p. 5):—

‘The fifth subject which may be considered as an essential part of a sound general education is experimental science; that is to say, the science of physics and chemistry treated experimentally. As a means of mental training, and also viewed as useful knowledge, this may be considered a necessary part of the intellectual equipment of every educated man, and especially so of the officer, whose profession in all its branches is daily becoming more and more dependent on science.’ When statements of this kind have been made by some of us in the past, nobody has paid much attention; but I beg you to observe that the headmaster of Eton and the headmaster of St. Paul’s School are two of the members of the important Committee who signed this recommendation, and it is impossible to ignore it. Last year, for the first time, the President of the Royal Society made a statement of much the same kind, only stronger, in his annual address. I am glad to see that the real value of education in physical science is now appreciated; that mere knowledge of scientific facts is known to be unimportant compared with the production of certain habits of thought and action which the methods of scientific study usually produce.

As to English, the Committee say: ‘They have no hesitation in insisting that a knowledge of English,¹ as tested by composition, together with an acquaintance with the main facts of the history and geography of the British Empire, ought in future to hold the *first* place in the examination and to be exacted from all candidates.’ The italics are mine. It will be noticed that they say nothing about the practical impossibility of obtaining teachers. As to mathematics, the Committee say: ‘It is of almost equal importance that every officer should have a thorough grounding in the elementary part of mathematics. But they think that elementary mechanics and geometrical drawing, which under the name of practical geometry is now often used as an introduction to theoretical instruction, should be added to this part of the examination, so as to ensure that at this stage of instruction the practical application of mathematics may not be left out of sight.’ As Sir Hugh Evans would have said, ‘It is a very discretion answer—the meaning is good’; but I would that the Committee had condemned abstract mathematics for these army candidates altogether.

This report appears in good time. It would be well if Committees would sit and take evidence as to the education of men in the other professions entered by our average boys. It is likely that when an authoritative report is prepared on the want of education of clergymen, for example, exactly the same statements will be made in regard to the general education which ought to precede the technical training; but perhaps a reference may be made in the report to the importance of a study of geology and biology as well as physical science. Think of the clergyman being able to meet his scientific enemies in the gate!

¹ This Committee recommends for the Woolwich and Sandhurst candidates a reform that has already been carried out by London University. No dead language is to be compulsory, but unfortunately some language other than English is still to be compulsory. Those boys, of whom there are so many, who dislike and cannot learn another language are still to be labelled ‘uneducated.’ Must there, then, be national defeat and captivity before our chosen race gives up its false academic gods? We think of education in the most slovenly fashion. The very men who say that *utility* is of no importance are the men who insist on the usefulness of a knowledge of French or German. They say that a man is illiterate if he knows only English, although he may be familiar with all English literature and with other literatures through translations. The man who has passed certain examinations in his youth and never cares to read anything is said to be educated. The men of the city of the Violet Crown, were they not educated? And did they know any other than their own language?

Thanks mainly to the efforts of a British Association Committee, really good teaching of experimental science is now being introduced into all public schools, in spite of most persistent opposition wearing an appearance of friendliness. In consequence, too, of the appointment of a British Association Committee last year, at what might be called the psychological moment, a great reform has already begun in the teaching of mathematics.¹ Even in the regulations for the Oxford Locals for 1903 Euclid is repudiated. It seems probable that at the end of another five years no average boy of fifteen years of age will have been compelled to attempt any abstract reasoning about things of which he knows nothing; he will be versed in experimental mathematics, which he may or may not call mensuration; he will use logarithms, and mere multiplication and division will be a joy to him; he will have a working power with algebra and sines and cosines; he will be able to tackle at once any curious new problem which can be solved by squared paper; and he will have no fear of the symbols of the infinitesimal calculus. When I insist that a boy ought to be able to compute, this is the sort of computation that I mean. Five years hence it will be called 'elementary mathematics.' Four years ago it was an unorthodox subject called 'practical mathematics,' but it is establishing itself in every polytechnic and technical college and evening or day science school in the country. Several times I have been informed that on starting an evening class, when plans have been made for a possible attendance of ten or twenty students, the actual attendance has been 200 to 300. Pupils may come for one or two nights to a class on academic mathematics, but then stay away for ever; a class in practical mathematics maintains its large numbers to the end of the winter.²

Hitherto the average boy has been taught mathematics and mechanics as if he were going to be a Newton or a Laplace; he learnt nothing and became stupid. I am sorry to say that the teaching of mechanics and mechanical engineering through experiment is comparatively unknown. Cambridge writers and other writers of books on experimental mechanics are unfortunately ignorant of engineering. University courses on engineering—with one splendid exception, under Professor Ewing at Cambridge—assume that undergraduates are taught their mechanics as a logical development of one or two axioms; whereas in many technical schools under the Science and Art Department apprentices go through a wonderfully good laboratory course in mechanical engineering. We really want to give only a few fundamental ideas about momentum and the transformations of energy and the properties of materials, and to give them from so many points of view that they become part of a student's mental machinery, so that he uses them continually. Instead of giving a hundred labour-saving rules which must be forgotten, we ought to give the one or two ideas which a man's common-sense will enable him to apply to any problem whatsoever and which cannot be forgotten. A boy of good mathematical attainments may build on this experimental knowledge afterwards a superstructure more elaborate than Rankine or Kelvin or Maxwell ever dreamt of as being possible. Every boy will build some superstructure of his own.

I must not dwell any longer on the three essential parts of a good general education which lead to the three powers which all boys of fifteen ought to possess; power to use books and to enjoy reading; power to use mathematics and to enjoy its use; power to study Nature sympathetically. English Board School boys who go to evening classes in many technical schools after they become apprentices are really obtaining this kind of education. The Scotch Education Board is trying to give it to all boys in primary and secondary schools. It will, I fear, be some time before the sons of well-to-do parents in England have a chance of obtaining it.

When a boy or man of any age or any kind of experience enters an engineering

¹ Discussion last year and report of Committee, published by Macmillan.

² To many men it will seem absurd that a real working knowledge of what is usually called higher mathematics, accompanied by mental training, can be given to the average boy. In the same way it seemed absurd 500 years ago that power to read and write and cipher could be given to everybody. These general beliefs of ours are very wonderful.

college and wishes to learn the scientific principles underlying a trade or profession, how ought we to teach him? Here is the reasonable general principle which Professors Ayrton and Armstrong and I have acted upon, and which has so far led us to much success. Whether he comes from a bad or a good school, whether he is an old or young boy or man, approach his intelligence through the knowledge and experience he already possesses. This principle involves that we shall compel the teacher to take the pupil's point of view¹ rather than the pupil the teacher's; give the student a choice of many directions in which he may study; let lectures be rather to instruct the student how to teach himself than to teach him; show the student how to learn through experiment, and how to use books, and, except for suggestion and help when asked for, leave him greatly to himself. If a teacher understands the principle he will have no difficulty in carrying it out with any class of students. I myself prefer to have students of very different qualifications and experience in one class because of the education that each gives to the others. Usually, however, except in evening classes, one has a set of boys coming from much the same kind of school, and, although perhaps differing considerably as to the places they might take in an ordinary examination, really all of much the same average intelligence. Perhaps I had better describe how the principle is carried out in one case—the sons of well-to-do parents such as now leave English schools at about fifteen years of age.

It was for such boys that the courses of instruction at the Finsbury Technical College (the City and Guilds of London Institute) were arranged twenty-two years ago. It was attempted to supply that kind of training which ought already to have been given at school, together with so much technical training as might enable a boy at the end of a two years' course to enter any kind of factory where applied science was important, with an observing eye, an understanding brain, and a fairly skilful hand. The system, in so far as it applies to various kinds of mechanical engineering, will be found described in one of a small collection of essays called 'England's Neglect of Science,' pp. 57-67.² I am sure that any engineer who reads that description will feel satisfied that it was the very best course imaginable for the average boy of the present time. A boy was taught how he must teach himself after he entered works. If after two or three years in the works he cared to go for a year or so to one of the greater colleges, or did not so care, it was assumed that he had had such a training as would enable him to choose the course which was really the best for him.

Old Finsbury students are to be found everywhere in important posts. The experiment has proved so successful that every London Polytechnic, every Municipal Technical School in the country, has adopted the system, and in the present state of our schools I feel sure that all important colleges ought to adopt the Finsbury system. It hardly seems appropriate to apply the word 'system' to what was so plastic and uncrystallised and had nothing to do with any kind of ritual.

¹ Usually it is assumed that there is only one line of study. In mathematics it is assumed that a boy has the knowledge and power and past experience and leisure of an Alexandrian philosopher. In mechanics we assume the boy to be fond of abstract reasoning, that he is a good geometrician who can do the most complex things in geometrical conics, but cannot possibly take in the simplest idea of the calculus.

² The ideas in this Address have been put forward many times by Professor Ayrton and myself. See the following, among other publications:—*England's Neglect of Science* (Fisher Unwin); *Practical Mechanics*, 1881 (Cassell); *Applied Mechanics*, 1897 (Cassell); *The Steam Engine, &c.*, 1898 (Macmillan); *The Calculus for Engineers*, 1897 (Arnold); Recent Syllabuses and Examination Papers of the Science and Art Department in Subjects I., VII., Vp, and XXII.; Summary of Lectures on Practical Mathematics (Board of Education); The Work of the City and Guilds Central Technical College (*Journal of the Society of Arts*, July 9, 1897); Inaugural Lecture at Finsbury, 1879; Address at the Coventry Technical Institute, February 1898; 'Education of an Electrical Engineer' (*Journal of the Society of Telegraph Engineers and of Electricians*, September 1882); Presidential Address, Institution of Electrical Engineers, January 1892; 'The Best Education for an Engineer' (*Nature*), October 12, 1899; Address at a Drawing-room Meeting, March 1887.

The Professors were given a free hand at Finsbury, and there were no outside examiners. I need not dwell upon the courses in Chemistry and Physics; some critics might call the subjects *Rational* Chemistry and Applied Physics; they were as different from all other courses of study in these subjects as the courses on Rational Mathematics and Mechanics differed from all courses elsewhere. The course on Mechanics was really one on Mechanical Engineering. There were workshops in wood and iron, not to teach trades, but rather to teach boys the properties of materials. There were a steam-engine and a gas-engine, and shafting and gearing of many kinds, and dynamos which advanced students in turn were allowed to look after under competent men. There was no machine which might not be experimented with occasionally. Elementary and advanced courses of lectures were given; there was an elaborate system of tutorial classes, where numerical and squared paper exercise work was done; there were classes in experimental plane and solid geometry, including much graphical calculation; boys were taught to make drawing-office drawings in pencil only, and tracings and blue prints, such as would be respected in the workshop, and not the ordinary drawing-class drawings, which cannot be respected anywhere; but the most important part of the training was in the Laboratory, in which every student worked, making quantitative experiments. An offer of a 100-ton testing-machine for that laboratory was made but refused; the advanced students usually had one opportunity given them of testing with a large machine, but not in their own laboratory. I consider that there is very little educational value in such a machine; the student thinks of the great machine,¹ and not of the tiny specimen. Junior students loaded wires and beams, or twisted things with very visible weights, and saw exactly what was happening, or they studied vibrating bodies. Many hours were devoted to experiments on a battered, rusty old screw-jack, or some other lifting-machine, its efficiency under many kinds of load being determined, and students studied their observations using squared paper, as intently as if nobody had ever made such experiments before. There was one piece of apparatus, an old fly-wheel bought at a rag-and-bone shop, to which kinetic energy was given by a falling weight, which, I remember, occupied the attention of four white-headed directors of Electric Companies in 1882 (evening students) for many weeks. A casual first measurement led on to corrections for friction and stiffness of a cord, and much else of a most interesting kind. At the end of six weeks these gentlemen had gained a most thorough computational acquaintance with every important principle of mechanics, a knowledge never to be forgotten. They had also had a revelation such as comes to the true experimenter—but that is too deep a subject.

Perhaps teachers in the greater colleges will smile in a superior way when they hear of this kind of experimental mechanics being called engineering laboratory work. True, it was elementary mechanics; but is not every principle which every engineer constantly needs called a mere elementary principle of mechanics by superior persons? I find that these elementary principles are very much

¹ These great testing-machines, so common in the larger colleges, seem to have destroyed all idea of scientific experiment. There is so much that the engineer wants to know, and yet laboratory people are persistently and lazily repeating old work suggested and begun by engineers of sixty years ago. For example, men like Fairbairn and Robert Napier would long ago have found out the behaviour of materials under combined stresses. We do not even know the condition of strength of iron or steel in a twisted shaft which is also a beam. The theory of strength of a gun or thick tube under hydraulic pressure is no clearer now than it was fifty years ago. The engineer asks for actual information derived from actual trial, and we offer him the 'could kail het again' stuff falsely called 'theoretical,' which is found in all the text-books (my own among others). These great colleges of university rank ought to recognise that it is their duty to increase knowledge through the work of their advanced students. The duty is not neglected in the electrical departments of some of the colleges. Perhaps the most instructive reference is to the work done at the Central Technical College of the City and Guilds Institute at South Kensington, as described by Professor Ayrton in some of the papers already referred to. I cannot imagine a better development of the Finsbury idea in the work of the highest kind of Engineering College.

unknown to men who have passed through elaborate mathematical studies of mechanics. Students found out in that laboratory the worth of formulæ; they gained courage in making calculations from formulæ, for they had found out the extent of their own ignorance and knowledge.

I have never approved of elaborate steam-engines got up for students' laboratory exercise-work. A professor who had devoted much thought for a year to the construction of such a four-cylinder engine showed a friend how any one or any two or any three or all four cylinders, with or without jacketing, could be used in all sorts of ways. The friend ventured to say: 'This engine will be used just once and never after.' The professor was angry, but his friend proved to be right. The professor made experiments with it once himself with a few good students. Unfortunately it was not a sufficiently elaborate investigation for publication. Afterwards he never had time personally to superintend such work; his assistants were busy at other things; his students could not be trusted with the engine by themselves, and to this day it stands in the laboratory a beautiful but useless piece of apparatus. At Finsbury there was an excellent one-cylinder engine with vaporising condenser. It drove the workshops and electric generators. On a field-day it drove an electric generator only, and perhaps thirty students made measurements. Each of them had already acted as stoker and engine-driver, as oiler and tester of the machinery, lighting fires, taking indicator diagrams, weighing coals, opening and closing cocks from seven in the morning to ten at night, so that everything was well known to him. They maintained three different steady loads for trials of three hours each. They divided into groups, one from each group ceasing to take a particular kind of observation every ten minutes and removing to another job. All watches were made to agree, and each student noted the time of each observation. These observations were:—Taking indicator diagrams, checking the speed indicator, taking temperature of feed-water, quantity of feed by meter (the meter had been carefully checked by gauge-notch, and every other instrument used by us had been tested weeks before), taking the actual horse-power passing through a dynamometer coupling on the shaft, taking boiler and valve-chest pressures and vacuum pressures on the roof and in the engine-room, weighing coals (the calorific value had already been tested), taking the horse-power given out by the dynamo, counting the electric lamps in use, and so on. Each student was well prepared beforehand. During the next week he reduced his own observations, and some of the results were gathered on one great table. One lesson that this taught could never be forgotten—how the energy of one pound of coal was disposed of. So much up the chimney or by radiation from boiler or steam-jacket and pipes; in condensation in the cylinder; to the condenser; in engine friction; in shaft friction, &c. I cannot imagine a more important lesson to a young engineer than this one taught through a common working engine. The students had the same sort of experience with a gas-engine. I need hardly say how important it was that the Professor himself should take charge of the whole work leading up to, during, and after such a field-day.

The difficulty about all laboratory exercise work worth the name is that of finding demonstrators and assistants who are wise and energetic. Through foolishness and laziness the most beautiful system becomes an unmeaning routine, and the more smoothly it works the less educational it is. In England just now the curse of all education is the small amount of money available for the wages of teachers—just enough to attract mediocre men. I have been told, and I can easily imagine, that such men have one talent over-developed, the talent for making their job softer and softer, until at length they just sit at a table, maintaining discipline merely by their presence, answering the questions of such students as are earnest enough to come and worry them. In such cases it is absolutely necessary to periodically upset their clockwork arrangements. After such an artificial earthquake one might be reminded of what occurred at the pool of Bethesda, whose waters had their healing property restored when the angel came down and troubled them. But for a permanently good arrangement there ought to be very much higher wages all round in the teaching profession.

No kind of engineering has developed so rapidly as the electrical. Why,

it was at the meeting here in Belfast twenty-eight years ago (I remember, for I was a Secretary of Section A that year, and took the machine to pieces afterwards in Lord Kelvin's laboratory) that there was exhibited for the first time in these islands a small Gramme machine. This handmaid of all kinds of engineering is now so important that every young engineer may be called uneducated who has not had a training in that kind of mechanical engineering which is called electrical engineering. Professor Ayrton's laboratory at Finsbury is the model copied by every other electrical engineering laboratory in the world. He and I had the same notions; we had both been students of Lord Kelvin; we had worked together in Japan since 1875; but whereas I was trying to make my system of teaching mechanical engineering replace an existing system, or want of system, there was no existing system for his to replace. Thus it will be found that in every electrical engineering laboratory the elementary principles are made part of a pupil's mental machinery by many quantitative experiments, and nobody suggests that it is mere elementary physics which is being taught—a suggestion often enough made about the work in my mechanical laboratory. When students know these elementary principles well, they can apply their mathematics to the subject. As they advance in knowledge they are allowed to find out by their own experiments how their simple theories must be made more complex in real machines. Their study may be very complete, but, however much mathematics and graphical calculation may come in, their designs of electrical machinery are really based upon the knowledge acquired by them in the electrical and mechanical laboratories.

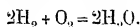
The electrical engineer has an enormous advantage over other engineers; everything lends itself to exact calculation, and a completed machine or any of its parts may be submitted to the most searching electrical and magnetic tests, since these tests, unlike those applied by other engineers, do not destroy the body tested. But for this very reason, as a finished product, the electrical engineer cannot have that training in the exercise of his judgment in actual practical work after he leaves a college that some other engineers must have. In tunnelling, earthwork, and building, in making railways and canals, the engineer is supremely dependent on the natural conditions provided for him, and these conditions are never twice the same. There are no simple laws known to us about the way in which sea and river currents will act upon sand and gravel, and engineers who have had to do with such problems are continually appealing to Nature, continually making observations and bringing to bear upon their work all the knowledge and habits of thought that all their past experience has given them. I do not know that there is any job which a good teacher would have greater pleasure in undertaking than the arrangement of a laboratory in which students might study for themselves such problems as come before railway, canal, river, harbour and coast-protection engineers; there is no such laboratory in existence at the present time, and in any case it could only be of use in the way of mere suggestion to an engineer who had already a good knowledge of his profession.

It was a curious illustration of mental inertia that the usual engineering visitor, even if he was a professor of engineering, always seemed to suppose that the work done at Finsbury was the same as that done in all the great engineering colleges. As a matter of fact no subject was taught there in the same manner as it was taught elsewhere.¹

Most of the students were preparing for electrical or mechanical engineering, and therefore we thought it important that nearly every professor or demonstrator or teacher should be an engineer. I know of nothing worse than that an engineering student should be taught mathematics or physics or chemistry by men who are ignorant of engineering, and yet nothing is more common in colleges of

¹ It is really ludicrous to see how all preachers on technical education are supposed by non-thinking people to hold the same doctrine. The people asking for reform in education differ from one another more than Erasmus and Luther, and John of Leyden and Ennippardoling.

applied science.¹ The usual courses are only suitable for men who are preparing to be mere mathematicians, or mere physicists, or mere chemists. Each subject is taken up in a stereotyped way, and it is thought quite natural that in one year a student shall have only a most elementary knowledge of what is to the teacher such a great subject. The young engineer never reaches the advanced parts which might be of use to him; he is not sufficiently grounded in general principles; his whole course is only a preliminary course to a more advanced one which there is no intention of allowing him to pursue, and, not being quite a fool, he soon sees how useless the thing is to him. The Professor of Chemistry ought to know that until a young engineer can calculate exactly by means of a principle, that principle is really unknown to him. For example, take the equation supposed to be known so well,



It is never understood by the ordinary elementary chemical student who writes it down so readily. Every one of the six cunning ways in which that equation conveys information ought to be as familiar to the young engineer as they are, or ought to be, to the most specialised chemist. Without this he cannot compute in connection with combustion in gas and oil engines and in furnaces. But I have no time to dwell on the importance of this kind of exact knowledge in the education of an engineer.

Mathematics and physics and chemistry are usually taught in watertight compartments, as if they had no connection with one another. In an engineering college this is particularly bad. Every subject ought to be taught through illustrations from the professional work in which a student is to be engaged. An engineer has been wasting his time if he is able to answer the questions of an ordinary examination paper in chemistry or pure mathematics. The usual mathematical teacher thinks most of those very parts of mathematics which to an ordinary man who wants to *use* mathematics are quite valueless, and those parts which would be altogether useful and easy enough to understand he never reaches; and as I have said, so it is also in chemistry. Luckily, the physics professor has usually some small knowledge of engineering; at all events he respects it. When the pure mathematician is compelled to leave the logical sequence which he loves to teach mechanics, he is apt scornfully to do what gives him least trouble: namely, to give as 'mechanics' that disguised pure mathematics which forms ninety per cent. of the pretence of theory to be found in so many French and German books on machinery. As pure mathematical exercise work it is even meaner than the stupid exercises in school algebras; as pretended engineering it does much harm because a student does not find out its futility until after he has gone through it, and his enthusiasm for mathematics applied to engineering problems is permanently hurt. But how is a poor mathematical professor who dislikes engineering, feeling like Pegasus harnessed to a common waggon—how is he to distinguish good from evil? He fails to see how worthless are some of the books on 'Theoretical Mechanics' written by mathematical coaches to enable students to pass examinations. An engineer teaching mathematics would avoid all futilities; he would base his reasoning on that experimental knowledge already possessed by a student; he would know that the finished engineer cannot hope to remember anything except a few general principles, but that he ought to be able to apply these, clumsily or not, to the solution of any problem whatsoever. Of course he would encourage some of his pupils to take up Thomson and Tait, or Rayleigh's 'Sound,' or some other classical treatise as an advanced study.²

¹ At the most important colleges the usual professor or tutor is often ignorant of all subjects except his own, and he generally seems rather proud of this; but surely in such a case a man cannot be said to know even his own subject.

² One sometimes finds a good mathematician brought up on academic lines taking to engineering problems. But he is usually *stale* and unwilling to go thoroughly into these practical matters, and what he publishes is particularly harmful, because it has such an honest appearance. When we do get, once in forty

Not only do I think that every teacher in an engineering college ought to have some acquaintance with engineering, but it seems to me equally important to allow a professor of engineering, who ought, above all things, to be a practical engineer, to keep in touch with his profession. A man who is not competing with other engineers in practical work very quickly becomes antiquated in his knowledge: the designing work in his drawing-office is altogether out of date; he lectures about old difficulties which are troubles no longer; his pupils have no enthusiasm in their work because it is merely academic and lifeless; even when he is a man distinguished for important work in the past his students have that kind of disrespect for his teaching which makes it useless to them. If there is fear that too much well-paid professional work will prevent efficiency in teaching, there is no great difficulty in applying a remedy.

One most important fact to be borne in mind is that efficient teachers cannot be obtained at such poor salaries as are now given. An efficient labourer is worthy of his hire; an inefficient labourer is not worthy of any hire, however small. Again, there is a necessity for three times as many teachers as are usually provided in England. The average man is in future to be really educated. This means very much more personal attention, and from thoughtful teachers. Is England prepared to face the problem of technical education in the only way which can lead to success, prepared to pay a proper price for the real article? If not, she must be prepared to see the average man remaining uneducated.

Advocacy of teaching of the kind that was given at Finsbury is often met by the opposition not only of pure mathematicians and academic teachers, but I am sorry to say also of engineers. The average engineer not merely looks askance at, he is really opposed to the college training of engineers, and I think, on the whole, that he has much justification for his views. University degrees in engineering science are often conferred upon students who follow an academic course, in which they learn little except how to pass examinations. The graduate of to-day, even, does not often possess the three powers to which I have referred. He is not fond of reading, and therefore he has no imagination, and the idea of an engineer without imagination is as absurd as Teufelsdröckh's notion of a cast-iron king. He cannot really compute, in spite of all his mathematics, and he is absurdly innocent of the methods of the true student of Nature. This kind of labelled scientific engineer is being manufactured now in bulk because there is a money value attached to a degree. He is not an engineer in any sense of the word, and does not care for engineering, but he sometimes gets employment in technical colleges. He is said to teach when he is really only impressing upon deluded pupils the importance of formulæ, and that whatever is printed in books must be true. The real young engineer, caught in this eddy, will no doubt find his way out of it, for the healthy experience of the workshop will bring back his common-sense. For the average pupil of such graduates there is no help. If he enters works, he knows but little more than if he had gone direct from school. He is still without the three qualifications which are absolutely necessary for a young engineer. He is fairly certain to be a nuisance in the works and to try another profession at the end of his pupilage. But if it is his father's business he can make a show of knowing something about it, and he is usually called an engineer.

Standardisation in an industry usually means easier and cheaper and better manufacture, and a certain amount of it must be good even in engineering, but when we see a great deal of it we know that in that industry the true engineer is disliked. I consider that in the scholastic industry there has been far too much

years, a mathematician (Osborne Reynolds or Dr. Hopkinson) who has common-sense notions about engineering things, or a fairly good engineer (Rankine or James Thomson) who has a common-sense command of mathematics, we have men who receive the greatest admiration from the engineering profession, and yet it seems to me that quite half of all the students leaving our technical colleges ought to be able to exercise these combined powers if mathematics were sensibly taught in school and college. We certainly have had enough of good mathematicians meddling with engineering theory and of engineers with no mathematics wasting their time in trying to add to our knowledge.

standardisation. Gymnasien and polytechnic systems are standardised in Germany, and there is a tendency to import them into England; but in my opinion we are very far indeed from knowing any system which deserves to be standardised, and the worst we can copy is what we find now in Germany and Switzerland. What we must strive for is the discovery of a British system suiting the British boy and man. The English boy may be called stupid so often that he actually believes himself to be stupid; but of one thing we may be sure, he will find in some way or other an escape from the stupefying kind of school work to which the German boy submits. And if it were possible to make the average English boy of nineteen pass such a silly school-leaving examination as the German boy,¹ and to pass through a polytechnic, I am quite sure that there would be little employment among common-sense English engineers for such a manufactured article. But is it possible that British boys could be manufactured into such obedient academic machines, without initiative or invention or individuality, by teachers who are none of them engineers? No, we must have a British system of education. We cannot go on much longer as we have done in the past without engineering education, and, furthermore, it must be such as to commend itself to employers. Of my Finsbury students I think I may say that not one failed to get into works on a two or three years' engagement, receiving some very small wage from the beginning, and without paying a premium. To obtain such employment was obviously one test of fitness to be an engineer, because experienced men thought it impossible. One test of the system was the greater ease with which new men obtained employment in shops which had already taken some of our students. It is certainly very difficult to convince an employer that a college man will not be a nuisance in the shops. In Germany and France, and to a less extent in America, there is among employers a belief in the value of technical education. In England there is still complete unbelief. I have known the subscribers of money to a large technical college in England (the members of its governing board) to laugh, all of them, at the idea that the college could be of any possible benefit to the industries of the town. They subscribed because just then there was a craze for technical education due to a recent panic. They were ignorant masters of works (sons of the men who had created the works), ignorant administrators of the college affairs, and ignorant critics of their mismanaged college. I feel sure that if the true meaning of technical education were understood, it would commend itself to Englishmen. Technical education is an education in the scientific and artistic principles which govern the ordinary operations in any industry. It is neither a science, nor an art, nor the teaching of a handicraft. It is that without which a master is an unskilled master; a foreman an unskilled foreman; a workman an unskilled workman; and a clerk or farmer an unskilled clerk or farmer. The cry for technical education is simply a protest against the existence of unskilled labour of all kinds.²

¹ The following is, I understand, a stock question at certain gymnasien: 'Write out all the trigonometrical formulæ you know.' I asked my young informant, 'Well, how many did you write?' 'Sixty-two' was the answer. This young man informed me that a boy could not pass this examination unless he knew 'all algebra and all trigonometry and all science.' Strassburg geese used to be fed in France; now they are fed in Germany. German education seems to be like smothering a fire with too much fuel or wet slack which has the appearance of fuel.

² I have pointed out how natural it is that business men should feel somewhat antagonistic to college training. Poorly paid unpractical teachers, with no ideas of their own, have in the past taught in the very stupidest way. They have called themselves 'scientific' and 'theoretical' till these words stink in the nostrils of an engineer. When I was an apprentice, and no doubt it is much the same now, if an apprentice was a poor workman with his hands he often took to some kind of study which he called the science of his trade. And in this way a pawkiness for science got to be the sign of a bad workman. But if workmen were so taught at school that they all really knew a little physical science, it would no longer be laughed at. When a civil or electrical engineer is unsuccessful because he has no business habits, he takes to calculation and the reading of so-called scientific books, because it is

To have any good general system the employers must co-operate. Much of the training is workshop practice, and it cannot be too often said that this is not to be given in any college. The workshop in a college serves a quite different purpose. Now how may the practice best be given? I must say that I like the Finsbury plan very much indeed, but there are others. When I attended this college in winter I was allowed to work in the Lagan Foundry in summer. In Japan the advanced students did the same thing; they had their winter courses at the college, and the summer was spent in the large Government workshops; the system worked very well indeed.¹ In Germany recently the great unions of manufacturers made facilities for giving a year of real factory work to the polytechnic students, but it seems to me that these men are much too old for entrance to works, and, besides, a year is too short a time if the finished product is to call itself a real engineer. Possibly the British solution may be quite different from any of these. A boy may enter works at fourteen on leaving a primary school or not later than sixteen on leaving a secondary school. In either case he must have the three powers to which I have already referred so often. It will be recognised as the duty of the owners of works to provide, either in one large works or near several works, in a well-equipped school following the Finsbury principle, all that training in the principles underlying the trade or profession which is necessary for the engineer.

No right-thinking engineer has been scared by the newspaper writers who tell us of our loss of supremacy in manufacture, but I think that every engineer sees the necessity for reform in many of our ways, and especially in this of education. People talk of the good done to our workmen's ideas by the strike of two years ago; it is to be hoped that the employers' ideas were also expanded by their having been forced to travel and to see that their shops were quite out of date.² In fact, we have all got to see that there is far too much unskilled labour among workmen and foremen and managers, and especially in owners. There may be some kinds of manufacture so standardised that everything goes like a wound-up clock, and no thought is needed anywhere; but certainly it is not in any branch of engineering. Many engineering things may be standardised, but not the engineer himself. Millions of money may build up trusts, but they will be wasted if the unskilled labour of mere clerks is expected to take the place of the thoughtful skilled labour of owners and managers. I go further, and say that no perfection in labour-saving tools will enable you to do without the skilled, educated, thoughtful, honest, faithful workman with brains. I laugh at the idea that any country has better workmen than ours, and I consider education of

very easy to get up a reputation for science. The man is a bad engineer in spite of his science, but people get to think that he is an unpractical man because of his scientific knowledge. I do believe that the unbelief in technical education so very general has this kind of illogical foundation. Four hundred years ago if a layman could read or write he was probably a useless person who, because he could not do well otherwise, took to learning. What a man learnt was clumsily learnt; usually he learnt little with great labour and made no use of it; therefore reading and writing seemed useless. Now that everybody is compelled to read and write, it is not a usual thing to say that it hurts a man to have these powers.

¹ It was the idea of Principal Henry Dyer.

² Not only is there an enormous improvement in the use of limit-gauges and checking and tools, and the careful calculation of rates of doing work by various tools and general shop arrangement, but attention is being paid to the comfort of workmen. There are basins and towels, and hot and cold water for them to wash in. In the old days it would have been called faddy philanthropy. Now, owners of works who scorn all softness of heart provide perfect water-closets for their men; their workshops are kept at a uniform temperature; the evil effect of a bad draught in producing colds, or a bad light in hurting the eyes, is carefully considered. In some of these works it is actually possible for a workman or a member of his family to get a luxurious hot bath for a penny. Will this really pay? Some clever hard-headed men of my acquaintance say they already see that it does pay very well indeed.

our workmen¹ to be the corner-stone of prosperity in all engineering manufacture. It is from him in countless ways that all hints leading to great inventions come. New countries like America and Germany have their chance just now; they are starting, without having to 'scrap' any old machinery or old ideas, with the latest machinery and the latest ideas. For them also the time will come when their machines will be getting out-of-date and the cost of 'scrapping' will loom large in their eyes. In the meantime they have taught us lessons, and this greatest of all lessons—that unless we look ahead with much judgment, unless we take reasonable precautions, unless we pay some regard to the fact that the cleverest people in several nations are hungry for our trade and jealous of our supremacy, we may for a time lose a little of that supremacy. In the last twenty-three years I have written a good deal about the harm done to England by the general dislike that there is among all classes for any kind of education. I do not say that this dislike is greater than it used to be in England; I complain that it is about as great.² But I have never spoken of the decadence of England. It is only that we have been too confident that those manufactures and that commerce and that skill in engineering, for which Napoleon sneered at us, would remain with us for ever. Many writers have long been pointing out the consequences of neglecting education: prophesying those very losses of trade, that very failure of engineers to keep their houses in order, which now alarms all newspaper writers. Panics are ridiculous, but there is nothing ridiculous in showing that we can take a hint. We have had a very strong hint given us that we cannot for ever go on with absolutely no education in the scientific principles which underlie all engineering. There is another important thing to remember. Should foreigners get the notion that we are decaying, we shall no longer have our industries kept up by an influx of clever Uitlanders, and we are much too much in the habit of forgetting what we owe to foreigners, Fleming and German, Hollander, Huguenot and Hebrew, for the development of our natural resources. Think of how much we sometimes owe to one foreigner like the late Sir William Siemens.

But I am going too far; there is after all not so very much of the foolishness of Ishbosheth among us, and I cannot help but feel hopeful as I think lovingly of what British engineers have done in the past. We who meet here have lived through the pioneering time of mechanical and electrical and various other kinds of engineering. Our days and nights have been delightful because we have had the feeling that we also were helping in the creation of a quite new thing never before known. It may be that our successors will have a better time, will see a more rapid development of some other applications of science. Who knows? In every laboratory of the world men are discovering more and more of Nature's secrets. The laboratory experiment of to-day gives rise to the engineering achievement of to-morrow. But I do say that, however great may be the growth of engineering, there can never be a time in the future history of the world, as there has never before been a time, when men will have more satisfaction in the growth of their profession than engineers have had during the reign of Queen Victoria.

And now I want to call your attention to a new phenomenon. Over and over again has attention been called to the fact that the engineer has created what is called 'modern civilisation,' has given luxuries of all kinds to the poorest people, has provided engines to do all the slave labour of the world, has given leisure and freedom from drudgery, and chances of refinement and high thought and high emotion to thousands instead of units. But it is doing things more striking still. Probably the most important of all things is that the yoke of superstitions of all kinds on the souls of men should be lifted. The study of natural science is alone able to do this, but education through natural science for the great mass of the people, even for the select few called the distinguished men of the country, has been quite impossible till recently. I say that it is to engineers that the world

¹ The old apprenticeship system of training men has broken down, and this is the cause of most of our industrial troubles. An apprenticeship system suited to modern conditions is described fully on pages 68-88 of *England's Neglect of Science*.

owes the possibility of this new study becoming general. In our country nearly all discoveries come from below. The leaders of science, the inventors, receive from a thousand obscure sources the germs of their great discoveries and inventions. When every unit of the population is familiar with scientific ideas our leaders will not only be more numerous, but they will be individually greater. And it is we, and not the schoolmasters, who are familiarising the people with a better knowledge of Nature. When men can hardly take a step without seeing steam engines and electro-motors and telegraphs and telephones and steamships, with drainage and water works, with railways and electric tramways and motor-cars; when every shop-window is filled with the products of engineering enterprise, it is getting rather difficult for people to have any belief in evil spirits and witchcraft.

All the heart-breaking preaching of enthusiasts in education would produce very little effect upon an old society like that of England if it were not for the engineer. He has produced peace. He is turning the brown desert lands of the earth into green pastures. He is producing that intense competition among nations which compels education. If England has always been the last to begin reform, she has always been the most thorough and steadfast of the nations on any reform when once she has started on it. Education, pedagogy, is a progressive science; and who am I that I should say that the system of education advocated by me is that which will be found best for England? In school education of the average boy or man England has as yet had practically no experience, for she has given no real thought to it. Yet when she does, I feel that although the Finsbury scheme for engineers may need great improvement, it contains the germ of that system which must be adopted by a race which has always learnt through trial and error, which has been led less by abstract principles or abstract methods of reasoning than any race known in history.

British Association for the Advancement of Science.

BELFAST, 1902.

ADDRESS

TO THE

PHYSIOLOGICAL SECTION

BY

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PRESIDENT OF THE SECTION.

The Present Position of Chemical Physiology.

AN engineer who desires to thoroughly understand how a machine works must necessarily know its construction. If the machine becomes erratic in its action, and he wishes to put it into proper working order, a preliminary acquaintance with its normal structure and function is an obvious necessity.

If we apply this to the more delicate machinery of the animal body we at once see how a knowledge of function (physiology and pathology) is impossible without a preliminary acquaintance with structure or anatomy.

It is therefore not surprising, it is indeed in the nature of things, that physiology originated with the great anatomists of the past. It was not until Vesalius and Harvey by tedious dissections laid bare the broad facts of structure that any theorising concerning the uses of the constituent organs of the body had any firm foundation.

Important and essential as the knowledge is that can be revealed by the scalpel, the introduction and use of the microscope furnished physiologists with a still more valuable instrument. By it much that was before unseen came into view, and microscopic anatomy and physiology grew in stature and knowledge simultaneously.

The weapons in the armoury of the modern physiologist are multitudinous in number and complex in construction, and enable him in the experimental investigation of his subject to accurately measure and record the workings of the different parts of the machinery he has to study. But pre-eminent among these instruments stands the test-tube and the chemical operations typified by that simple piece of glass.

Herein one sees at once a striking distinction between the mechanism of a living animal and that of a machine like a steam engine or a watch. It is quite possible to be an excellent watchmaker or to drive a steam engine intelligently without any chemical knowledge of the various metals that enter into its composition. In order to set the mechanism right if it goes wrong all the preliminary knowledge which is necessary is of an anatomical nature. The parts of which an engine is composed are stable; the oil that lubricates it and the fuel that feeds it never become integral parts of the machinery. But with the living engine all this is different. The parts of which it is made take up the nutriment or fuel and assimilate it, thus building up new living substance to replace that which is destroyed in the wear and tear associated with activity. This condition of unstable chemical equilibrium is usually designated metabolism, and metabolism is the great and essential attribute of a living as compared with a non-living thing.

It seems childish at the present day, and before such an audience as this, to point out how essential it is to know the chemical structure as well as the anatomical structure of the component parts of the body. But the early anatomists to whom I have alluded had no conception of the connection of the two sciences. Speaking of Vesalius, Sir Michael Foster says: 'The great anatomist would no doubt have made use of his bitterest sarcasms had someone assured him that the fantastic school which was busy with occult secrets and had hopes of turning dross into gold would one day join hands in the investigation of the problems of life with the exact and clear anatomy so dear to him.' Nor did Harvey, any more than Vesalius, pay heed to chemical learning. The scientific men of his time ignored and despised the beginning of that chemical knowledge which in later years was to become one of the foundations of physiology and the mainstay of the art of medicine.

The earliest to recognise this important connection was one whose name is usually associated more with charlatanry than with truth, namely, Paracelsus, and fifty years after the death of that remarkable and curious personality his doctrines were extended and developed by van Helmont. In spite, however, of van Helmont's remarkable insight into the processes of digestion and fermentation, his work was marred by the mysticism of the day which called in the aid of supernatural agencies to explain what could not otherwise be fully comprehended.

In the two hundred and fifty years that have intervened between the death of van Helmont and the present day alchemy became a more and more exact science, and changed its name to chemistry, and a few striking names stand out of men who were able to take the new facts of chemistry and apply them to physiological uses. Of these one may mention Mayow, Lower, Boerhaave, Réaumur, Borelli, Spallanzani, and Lavoisier. Mulder in Holland and Liebig in Germany bring us almost to the present time, and I think they may be said to share the honour of being regarded as the father of modern chemical physiology. This branch of science was first placed on a firm basis by Wöhler when he showed that organic compounds can be built out of their elements in the laboratory, and his first successful experiments in connection with the comparatively simple substance urea have been followed by numberless others, which have made organic chemistry the vast subject it is to-day.

Sir Michael Foster's book on the History of Physiology, from which I have already quoted, treats of the older workers who laid the foundations of our science, and whose names I have not done much more than barely mention. Those interested in the giants of the past should consult it. But what I propose to take up this morning is the work of those who have during more recent days been engaged in the later stages of the building. The edifice is far from completion even now. It is one of the charms of physiological endeavour that as the older areas yield their secrets to the explorers new ones are opened out which require equally careful investigation.

If even a superficial survey of modern physiological literature is taken, one is at once struck with the great preponderance of papers and books which have a chemical bearing. In this the physiological journals of to-day contrast very markedly with those of thirty, twenty, or even ten years ago. The sister science of chemical pathology is making similar rapid strides. In some universities the importance of biological chemistry is recognised by the foundation of chairs which deal with that subject alone; and though in the United Kingdom, owing mainly to lack of funds, this aspect of the advance of science is not very evident, there are signs that the date cannot be far distant when every well-equipped university or university college will follow the example set us at many seats of learning on the Continent and at Liverpool.

With these introductory remarks let me now proceed to describe what appear to me to be the main features of chemical physiology at the present time.

The first point to which I shall direct your attention is the rapid way in which chemical physiology is becoming an exact science. Though it is less than twenty years since I began to teach physiology, I can remember perfectly well a time when those who devoted their work to the chemical side of the science might

almost be counted on the fingers of one hand, and when chemists looked with scarcely veiled contempt on what was at that time called physiological chemistry: they stated that physiologists dealt with messes or impure materials, and therefore anything in the nature of correct knowledge was not possible. There was a good deal of truth in these statements, and if physiologists to-day cannot quite say that they have changed all that, they can at any rate assert with truth that they are changing it. This is due to a growing *rapprochement* between chemists and physiologists. Many of our younger physiologists now go through a thorough preliminary chemical training; and on the other hand there is a growing number of chemists—of whom Emil Fischer may be taken as a type—who are beginning to recognise the importance of a systematic study of substances of physiological interest. A very striking instance of this is seen in the progress of our knowledge of the carbohydrates, which has culminated in the actual synthesis of several members of the sugar group. Another instance is seen in the accurate information we now possess of the constitution of uric acid. When Miescher began his work on the chemical composition of the nuclei of cells, and separated from them the material he called nuclein, he little foresaw the wide practical application of his work. We now know that it is in the metabolism of cell-nuclei that we have to look for the oxidative formation of uric acid and other substances of the purine family. Already the chemical relationships of uric acid and nuclein have taught practical physicians some of the secrets that underlie the occurrence of gout and allied disorders.

With the time at my disposal, it would be impossible to discuss all the chemico-vital problems which the physiologists of the present day are attempting to solve, but there is one subject at which many of them are labouring which seems to me to be of supreme importance—I mean the chemical constitution of proteid or albuminous substances. Proteids are produced only in the living laboratory of plants and animals; proteid metabolism is the main chemical attribute of a living thing; proteid matter is the all-important material present in protoplasm. But in spite of the overwhelming importance of the subject chemists and physiologists alike have far too long fought shy of attempting to unravel the constitution of the proteid molecule. This molecule is the most complex that is known: it always contains five, and often six, or even seven elements. The task of thoroughly understanding its composition is necessarily vast, and advance slow. But little by little the puzzle is being solved, and this final conquest of organic chemistry, when it does arrive, will furnish physiologists with new light on many of the dark places of physiological science.

The revival of the vitalistic conception in physiological work appears to me a retrograde step. To explain anything we are not fully able to understand in the light of physics and chemistry by labelling it as vital or something we can never hope to understand is a confession of ignorance, and, what is still more harmful, a bar to progress. It may be that there is a special force in living things that distinguishes them from the inorganic world. If this is so, the laws that regulate this force must be discovered and measured, and I have no doubt that those laws when discovered will be found to be as immutable and regular as the force of gravitation. I am, however, hopeful that the scientific workers of the future will discover that this so-called vital force is due to certain physical or chemical properties of living matter which have not yet been brought into line with the known chemical and physical laws that operate in the inorganic world, but which as our knowledge of chemistry and physics increases will ultimately be found to be subservient to such laws.

Let me take as an example the subject of osmosis. The laws which regulate this phenomenon through dead membranes are fairly well known and can be experimentally verified; but in the living body there is some other manifestation of force which operates in such a way as to neutralise the known force of osmosis. Is it necessary to suppose that this force is a new one? May it not rather be that our much vaunted knowledge of osmosis is not yet complete? It is quite easy to understand why a dead and a living membrane should behave differently in relation to substances that are passing through them. The

molecules of the dead membrane are, comparatively speaking, passive and stable; the molecules in a membrane made of living cells are in a constant state of chemical integration and disintegration; they are the most unstable molecules we know. Is it to be expected that such molecules would allow water, or substances dissolved in water, to pass between them and remain entirely inactive? The probability appears to me to be all the other way; the substances passing, or attempting to pass, between the molecules will be called upon to participate in the chemical activities of the molecules themselves, and in the building up and breaking down of the compounds so formed there will be a transformation of chemical energy and a liberation of what looks like a new force. Before a physicist decides that his knowledge of osmosis is final, let him attempt to make a membrane of some material which is in a state of unstable chemical equilibrium, a state in some way comparable to what is called metabolism in living protoplasm. I cannot conceive that such a task is insuperable, and when accomplished, and the behaviour of such a membrane in an osmometer or dialyser is studied, I am convinced that we shall find that the laws of osmosis as formulated for such dead substances as we have hitherto used will be found to require revision.

Such an attitude in reference to vital problems appears to be infinitely preferable to that which too many adopt of passive content, saying the phenomenon is vital and there is an end of it.

When a scientific man says this, or that vital phenomenon cannot be explained by the laws of chemistry and physics, and therefore must be regulated by laws of some other nature, he most unjustifiably assumes that the laws of chemistry and physics have all been discovered. He forgets, for instance, that such an important detail as the constitution of the proteid molecule has still to be made out.

The recent history of science gives an emphatic denial to such a supposition. All my listeners have within the last few years seen the discovery of the Röntgen rays and the modern development of wireless telegraphy. On the chemical side we have witnessed the discovery of new elements in the atmosphere and the introduction of an entirely new branch of chemistry called physical chemistry. With such examples ready to our hands, who can say what further discoveries will not shortly be made, even in such well-worked fields as chemistry and physics?

The mention of physical chemistry brings me to what I may term the second head of my discourse, the second striking characteristic of modern chemical physiology: this is the increasing importance which physiologists recognise in a study of inorganic chemistry. The materials of which our bodies are composed are mainly organic compounds, among which the proteids stand out as pre-eminently important; but everyone knows there are many substances of the mineral or inorganic kingdom present in addition. I need hardly mention the importance of water, of the oxygen of the air, and of salts like sodium chloride and calcium phosphate.

The new branch of inorganic chemistry called physical chemistry has given us entirely new ideas of the nature of solutions, and the fact that electrolytes in solution are broken up into their constituent ions is one of fundamental importance. One of the many physiological aspects of this subject is seen in a study of the action of mineral salts in solution on living organisms and parts of organisms. Many years ago Dr. Ringer showed that contractile tissues (heart, cilia, &c.) continue to manifest their activity in certain saline solutions. Howell goes so far as to say, and probably correctly say, that the cause of the rhythmical action of the heart is the presence of these inorganic substances in the blood or lymph which usually bathes it. The subject has more recently been taken up by Loeb and his colleagues at Chicago: they confirm Ringer's original statements, but interpret them now as ionic action. Contractile tissues will not contract in pure solutions of non-electrolytes like sugar or albumin. But different contractile tissues differ in the nature of the ions which are their most favourable stimuli. An optimum salt solution is one in which stimulating ions, like those of sodium, are mixed with a certain small amount of those which like calcium restrain activity. Loeb considers that the ions act because they affect either the physical condition of the colloidal substances (proteid, &c.) in protoplasm or the rapidity of chemical processes.

Amœboid movement, ciliary movement, the contraction of muscle, cell division, and karyokinesis all fall into the same category as being mainly dependent on the stimulating action of ions.

Loeb has even gone so far as to consider that the process of fertilisation is mainly ionic action; he denies that the nuclein of the male cell is essential, but asserts that all it does is to act as the stimulus in the due adjustment of the proportions of the surrounding ions, and supports this view by numerous experiments on ova in which without the presence of spermatozoa he has produced larvæ by merely altering the saline constituents and so the osmotic pressure of the fluid that surrounds them. Whether such a sweeping and almost revolutionary notion will stand the test of further verification must be left to the future; so also must the equally important idea that nervous impulses are to be mainly explained on an electrolytic basis. But whether or not all the details of such work will stand the test of time, the experiments I have briefly alluded to are sufficient to show the importance of physical chemistry to the physiologist, and they also form a useful commentary on what I was saying just now about vitalism. Such eminently vital phenomena as movement and fertilisation are to be explained in whole or in part as due to the physical action of inorganic substances. Are not such suggestions indications of the undesirability of postulating the existence of any special mystic vital force?

I have spoken up to this point of physical chemistry as a branch of inorganic chemistry; there are already indications of its importance also in relation to organic chemistry. Many eminent chemists consider that the future advance of organic chemistry will be on the new physical lines. It is impossible to forecast where this will lead us; suffice it to say that not only physiology, but also pathology, pharmacology, and even therapeutics, will receive new accessions to knowledge the importance of which will be enormous.

I have now briefly sketched what appear to me to be the two main features of the chemical physiology of to-day, and the two lines, organic and inorganic, along which I believe it will progress in the future.

Let me now press upon you the importance in physiology, as in all experimental sciences, of the necessity first of bold experimentation, and secondly of bold theorising from experimental data. Without experiment all theorising is futile; the discovery of gravitation would never have seen the light if laborious years of work had not convinced Newton that it could be deduced from his observations. The Darwinian theory was similarly based upon data, and experiments which occupied the greater part of its author's lifetime to collect and perform. Pasteur in France and Virchow in Germany supply other instances of the same devotion to work which was followed by the promulgation of wide-sweeping generalisations.

And after all it is the general law which is the main object of research; isolated facts may be interesting and are often of value, but it is not until facts are correlated and the discoverers ascertain their interrelationships that anything of epoch-making importance is given to the world.

It is, however, frequently the case that a thinker with keen insight can see the general law even before the facts upon which it rests are fully worked out. Often such bold theorists are right, but even if they ultimately turn out to be wrong, or only partly right, they have given to their fellows some general idea on which to work; if the general idea is incorrect, it is important to prove it to be so in order to discover what is right later on. No one has ever seen an atom or a molecule, yet who can doubt that the atomic theory is the sheet anchor of chemistry? Mendeleeff formulated his periodic law before many of the elements were discovered; yet the accuracy of this great generalisation has been such that it has actually led to the discovery of some of the missing elements.

I purpose to illustrate these general remarks by a brief allusion to two typical sets of researches carried out during recent years in the region of chemical physiology. I do not pretend that either of them has the same overwhelming importance as the great discoveries I have alluded to, but I am inclined to think that one of them comes very near to that standard. The investigations in question are those of Ehrlich and of Pawlow. The work of Ehrlich mainly illustrates the useful

part played by bold theorising, the work of Pawlow that played by the introduction of new and bold methods of experiment.

I will take Pawlow first. This energetic and original Russian physiologist has by his new methods succeeded in throwing an entirely new light on the processes of digestion. Ingeniously devised surgical operations have enabled him to obtain the various digestive juices in a state of absolute purity and in large quantity. Their composition and their actions on the various foodstuffs have thus been ascertained in a manner never before accomplished; an apparently unfailing resourcefulness in devising and adapting experimental methods has enabled him and his fellow workers to discover the paths of the various nerve impulses by which secretion in the alimentary canal is regulated and controlled. The importance of the psychological element in the process of digestion has been experimentally verified. If I were asked to point out what I considered to be the most important outcome of all this painstaking work, I should begin my answer by a number of negatives, and would say, not the discovery of the secretory nerves of the stomach or pancreas; not the correct analysis of the gastric juice, nor the fact that the intestinal juice has most useful digestive functions; all of these are discoveries of which anyone might have been rightly proud; but after all they are more or less isolated facts. The main thing that Pawlow has shown is that digestion is not a succession of isolated acts, but each one is related to its predecessor and to that which follows it; the process of digestion is thus a continuous whole; for example, the acidity of the gastric juice provides for a delivery of pancreatic juice in proper quantity into the intestine; the intestinal juice acts upon the pancreatic, and so enables the latter to perform its powerful actions. I am afraid this example, as I have tersely stated it, presents the subject rather inadequately, but it will serve to show what I mean. Further, the composition of the various juices is admirably adjusted to the needs of the organism; when there is much proteid to be digested, the proteolytic activity of the juices secreted is correspondingly high, and the same is true for the other constituents of the food. It is such general conclusions as these, the correlation of isolated facts leading to the formulation of the law that the digestive process is continuous in the sense I have indicated and adapted to the needs of the work to be done, that constitute the great value of the work from the Russian laboratory. Work of this sort is sure to stimulate others to fill in the gaps and complete the picture, and already has borne fruit in this direction. It has, for instance, in Starling's hands led to the discovery of a chemical stimulus to pancreatic secretion. This is formed in the intestine as the result of the action of the gastric acid, and taken by the blood-stream to the pancreas. Whether this *secretin* as it is called may be one of a group of similar chemical stimuli which operate in other parts of the body has still to be found out.

The other series of researches to which I referred are those of Ehrlich and his colleagues and followers on the subject of immunity. This subject is one of such importance to every one of us that I am inclined to place the discovery on a level with those great discoveries of natural laws to which I alluded at the outset of this portion of my Address. I hesitate to do so yet because many of the details of the theory still await verification. But up to the present all is working in that direction, and Ehrlich's ideas illustrate the value of bold theorising in the hands of clear-sighted and far-seeing individuals.

But when I say that the doctrine is bold, I do not mean to infer that the experimental facts are scanty; they are just the reverse. But in the same way that a chemist has never seen an atom, and yet he believes atoms exist, so no one has yet ever seen a toxin or antitoxin in a state of purity, and yet we know they exist, and this knowledge promises to be of incalculable benefit to suffering humanity.

It may not be uninteresting to state briefly, for the benefit of those to whom the subject is new, the main facts and an outline of the theory which is based upon them.

We are all aware that one attack of many infective maladies protects us against another attack of the same disease. The person is said to be *immune* either partially or completely against that disease. Vaccination produces in a

patient an attack of cowpox or vaccinia. This disease is related to smallpox, and some still hold that it is smallpox modified and rendered less malignant by passing through the body of a calf. At any rate an attack of vaccinia renders a person immune to smallpox, or variola, for a certain number of years. Vaccination is an instance of what is called *protective inoculation*, which is now practised with more or less success in reference to other diseases like plague and typhoid fever. The study of immunity has also rendered possible what may be called *curative inoculation*, or the injection of antitoxic material as a cure for diphtheria, tetanus, snake poisoning, &c.

The power the blood possesses of slaying bacteria was first discovered when the effort was made to grow various kinds of bacteria in it; it was looked upon as probable that blood would prove a suitable soil or medium for this purpose. It was found in some instances to have exactly the opposite effect. The chemical characters of the substances which kill the bacteria are not fully known; indeed, the same is true for most of the substances we have to speak of in this connection. Absence of knowledge on this particular point has not, however, prevented important discoveries from being made.

So far as is known at present, the substances in question are proteid in nature. The bactericidal powers of blood are destroyed by heating it for an hour to 56° C. Whether the substances are enzymes is a disputed point. So also is the question whether they are derived from the leucocytes; the balance of evidence appears to me to be in favour of this view in many cases at any rate, and phagocytosis becomes more intelligible if this view is accepted. The substances, whatever be their source or their chemical nature, are sometimes called alexins, but the more usual name now applied to them is that of *bacterio-lysins*.

Closely allied to the bactericidal power of blood, or blood-serum, is its globulicidal power. By this one means that the blood-serum of one animal has the power of dissolving the red blood-corpuscles of another species. If the serum of one animal is injected into the blood-stream of an animal of another species, the result is a destruction of its red corpuscles, which may be so excessive as to lead to the passing of the liberated hæmoglobin into the urine (hæmoglobinuria). The substance or substances in the serum that possess this property are called *hemolysins*, and though there is some doubt whether bacterio-lysins and hæmolysins are absolutely identical, there is no doubt that they are closely related substances.

Another interesting chemical point in this connection is the fact that the bactericidal power of the blood is closely related to its alkalinity. Increase of alkalinity means increase of bactericidal power. Venous blood contains more diffusible alkali than arterial blood and is more bactericidal; dropsical effusions are more alkaline than normal lymph and kill bacteria more easily. In a condition like diabetes, when the blood is less alkaline than it should be, the susceptibility to infectious diseases is increased. Alkalinity is probably beneficial because it favours those oxidative processes in the cells of the body which are so essential for the maintenance of healthy life.

Normal blood possesses a certain amount of substances which are inimical to the life of our bacterial foes. But suppose a person gets run down; everyone knows he is then liable to 'catch anything.' This coincides with a diminution in the bactericidal power of his blood. But even a perfectly healthy person has not an unlimited supply of bacterio-lysin, and if the bacteria are sufficiently numerous he will fall a victim to the disease they produce. Here, however, comes in the remarkable part of the defence. In the struggle he will produce more and more bacterio-lysin, and if he gets well it means that the bacteria are finally vanquished, and his blood remains rich in the particular bacterio-lysin he has produced, and so will render him immune to further attacks from that particular species of bacterium. Every bacterium seems to cause the development of a specific bacterio-lysin.

Immunity can more conveniently be produced gradually in animals, and this applies, not only to the bacteria, but also to the toxins they form. If, for instance, the bacilli which produce diphtheria are grown in a suitable medium, they produce the diphtheria poison, or toxin, much in the same way that yeast-cells will produce

alcohol when grown in a solution of sugar. Diphtheria toxin is associated with a proteose, as is also the case with the poison of snake venom. If a certain small dose called a 'lethal dose' is injected into a guinea-pig the result is death. But if the guinea-pig receives a smaller dose it will recover; a few days after it will stand a rather larger dose; and this may be continued until after many successive gradually increasing doses it will finally stand an amount equal to many lethal doses without any ill effects. The gradual introduction of the toxin has called forth the production of an antitoxin. If this is done in the horse instead of the guinea-pig the production of antitoxin is still more marked, and the serum obtained from the blood of an immunised horse may be used for injecting into human beings suffering from diphtheria, and rapidly cures the disease. The two actions of the blood, antitoxic and antibacterial, are frequently associated, but may be entirely distinct.

The antitoxin is also a proteid probably of the nature of a globulin; at any rate it is a proteid of larger molecular weight than a proteose. This suggests a practical point. In the case of snake-bite the poison gets into the blood rapidly owing to the comparative ease with which it diffuses, and so it is quickly carried all over the body. In treatment with the antitoxin or antivenin, speed is everything if life is to be saved; injection of this material under the skin is not much good, for the diffusion into the blood is too slow. It should be injected straight away into a blood-vessel.

There is no doubt that in these cases the antitoxin neutralises the toxin much in the same way that an acid neutralises an alkali. If the toxin and antitoxin are mixed in a test-tube, and time allowed for the interaction to occur, the result is an innocuous mixture. The toxin, however, is merely neutralised, not destroyed: for if the mixture in the test-tube is heated to 68° C. the antitoxin is coagulated and destroyed and the toxin remains as poisonous as ever.

Immunity is distinguished into *active* and *passive*. Active immunity is produced by the development of protective substances in the body; passive immunity by the injection of a protective serum. Of the two the former is the more permanent.

Ricin, the poisonous proteid of castor-oil seeds, and *abrin*, that of the Jequirity bean, also produce when gradually given to animals an immunity, due to the production of antiricin and antiabrin respectively.

Ehrlich's hypothesis to explain such facts is usually spoken of as the *side-chain theory* of immunity. He considers that the toxins are capable of uniting with the protoplasm of living cells by possessing groups of atoms like those by which nutritive proteids are united to cells during normal assimilation. He terms these *haptophor* groups, and the groups to which these are attached in the cells he terms *receptor* groups. The introduction of a toxin stimulates an excessive production of receptors, which are finally thrown out into the circulation, and the free circulating receptors constitute the antitoxin. The comparison of the process to assimilation is justified by the fact that non-toxic substances like milk introduced gradually by successive doses into the blood-stream cause the formation of anti-substances capable of coagulating them.

Up to this point I have spoken only of the blood, but month by month workers are bringing forward evidence to show that other cells of the body may by similar measures be rendered capable of producing a corresponding protective mechanism.

One further development of the theory I must mention. At least two different substances are necessary to render a serum bactericidal or globulicidal. The bacterio-lysin or hæmolyisin consists of these two substances. One of these is called the *immune body*, the other the *complement*. We may illustrate the use of these terms by an example. The repeated injection of the blood of one animal (*e.g.*, the goat) into the blood of another animal (*e.g.*, a sheep) after a time renders the latter animal immune to further injections, and at the same time causes the production of a serum which dissolves readily the red blood-corpuscles of the first animal. The sheep's serum is thus hæmolytic towards goat's blood-corpuscles. This power is destroyed by heating to 56° C. for half an hour, but returns when fresh goat's serum is added. The specific immunising substance formed in the

sheep is called the immune body; the ferment-like substance destroyed by heat is the complement. The latter is not specific, since it is furnished by the blood of non-immunised animals, but it is nevertheless essential for hæmolysis. Ehrlich believes that the immune body has two side groups—one which connects with the receptor of the red corpuscles and one which unites with the haptophor group of the complement, and thus renders possible the ferment-like action of the complement on the red corpuscles. Various antibacterial serums which have not been the success in treating disease they were expected to be are probably too poor in complement, though they may contain plenty of the immune body.

Quite distinct from the bactericidal, globulicidal, and antitoxic properties of blood is its agglutinating action. This is another result of infection with many kinds of bacteria or their toxins. The blood acquires the property of rendering immobile and clumping together the specific bacteria used in the infection. The test applied to the blood in cases of typhoid fever, and generally called *Widal's reaction*, depends on this fact.

The substances that produce this effect are called *agglutinins*. They also are probably proteid-like in nature, but are more resistant to heat than the lysins. Prolonged heating to over 60° C. is necessary to destroy their activity.

Lastly, we come to a question which more directly appeals to the physiologist than the preceding, because experiments in relation to immunity have furnished us with what has hitherto been lacking, a means of distinguishing human blood from the blood of other animals.

The discovery was made by Tchistovitch (1899), and his original experiment was as follows:—Rabbits, dogs, goats, and guinea-pigs were inoculated with eel-serum, which is toxic: he thereby obtained from these animals an antitoxic serum. But the serum was not only antitoxic, but produced a precipitate when added to eel-serum, but not when added to the serum of any other animal. In other words, not only has a specific antitoxin been produced, but also a specific *precipitin*. Numerous observers have since found that this is a general rule throughout the animal kingdom, including man. If, for instance, a rabbit is treated with human blood, the serum ultimately obtained from the rabbit contains a specific precipitin for human blood; that is to say, a precipitate is formed on adding such a rabbit's serum to human blood, but not when added to the blood of any other animal.¹ The great value of the test is its delicacy: it will detect the specific blood when it is greatly diluted, after it has been dried for weeks, or even when it is mixed with the blood of other animals.

I have entered into this subject at some length because it so admirably illustrates the kind of research which is now in progress; it is also of interest to others than mere physiologists. I have not by any means exhausted the subject, but for fear I may exhaust my audience let me hasten to a conclusion. I began by eulogising the progress of the branch of science on which I have elected to speak to you. Let me conclude with a word of warning on the danger of over-specialisation. The ultra-specialist is apt to become narrow, to confine himself so closely to his own groove that he forgets to notice what is occurring in the parallel and intercrossing grooves of others. But those who devote themselves to the chemical side of physiology run but little danger of this evil. The subject cannot be studied apart from other branches of physiology, so closely are both branches and roots intertwined. As an illustration of this may I be permitted to speak of some of my own work? During the past few years the energies of my laboratory have been devoted to investigations on the chemical side of nervous activity, and I have had the advantage of co-operating to this end with a number of investigators, of whom I may particularly mention Dr. Mott and Dr. T. G. Brodie. But we soon found that any narrow investigation of the chemical properties of nervous matter and the changes this undergoes during life and after death was impossible. Our work extended in a pathological direction so as to investigate the matter in the brains of those suffering from nervous disease; it

¹ There may be a slight reaction with the blood of allied animals; for instance, with monkey's blood in the case of man.

extended in a histological direction so as to determine the chemical meaning of various staining reactions presented by normal and abnormal structures in the brain and spinal cord; it extended in an experimental direction in the elucidation of the phenomena of fatigue, and to ascertain whether there was any difference in medullated and non-medullated nerve fibres in this respect; it extended into what one may call a pharmacological direction in the investigation of the action of the poisonous products of the breakdown of nervous tissues. I think I have said enough to show you how intimate are the connections of the chemical with the other aspects of physiology, and although I have given you but one instance, that which is freshest to my mind, the same could be said for almost any other well-planned piece of research work of a bio-chemical nature.

We have now before us the real work of the Section, the reading, hearing, and seeing the researches which will be brought forward by members of the Association, and I must, in thanking you for your attention, apologise for the length of time I have kept you from these more important matters.

British Association for the Advancement of Sci

BELFAST, 1902.

ADDRESS

TO THE

ANTHROPOLOGICAL SECTION

BY

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So much has been written of late on totemism that I feel some diffidence in burdening still further the literature of the subject. But I may plead a slight claim on your attention, as I happen to be an unworthy member of the Crocodile kin of the Western tribe of Torres Straits, and I have been recognised as such in another island than the one where I changed names with Maino, the chief of Tutu, and thereby became a member of his kin.

I do not intend to discuss the many theories about totemism, as this would occupy too much time; nor can I profess to be able to throw much light upon the problems connected with it; but I chiefly desire to place before you the main issues in as clear a manner as may be, and I venture to offer for your consideration one way in and some ways out of totemism.

A few years ago M. Marillier wrote¹ that 'totemism is one of the rare forms of culture: it is incapable of evolution and transformation, and is intelligible only in its relations with certain types of social organisation. When these disappear it also disappears. Totemism in its complete development is antagonistic alike to transformation or progress.' In due course I shall describe how one people at least is emerging from totemism. At the outset I wish it to be distinctly understood that I do not regard this as the only way out; doubtless there have been several transformations, but a record of what appears to be taking place appeals more to most students than a guess as to what may have happened.

What is most needed at the present time is fresh investigation in the field. Those who are familiar with the literature of the subject are only too well aware of the imperfection of the available records. There are several reasons which account for this. Some of the customs and beliefs associated with totemism have a sacred significance, and the average savage is too reverent to speak lightly of what touches him so deeply. Natives cannot explain their mysteries any more than the adherents of more civilised religions can fully explain theirs. Further, they particularly dislike the unsympathetic attitude of most inquirers, and nothing shuts up a native more effectually than the fear of ridicule.

Language is another difficulty. Even supposing the white man has acquired the language, the vocabulary of the native is not sufficiently full or precise to explain those distinctions which appeal to us, but which are immaterial to him.

Granting the willingness of the native to communicate his ideas, and that the hindrance of language has been overcome, there remains the difficulty of the native understanding what it is the white man wishes to learn. If there is a practically

insuperable difficulty in the investigator putting himself into the mental attitude of the savage, there is also the reciprocal source of error.

‘Oh, East is East, and West is West,
And never the twain shall meet.’

If Kipling is right for the civilised Oriental, how about those of lower stages of culture and more primitive modes of thought?

We must not overlook the fact that the majority of white men who mix with primitive folk are either untrained observers or their training is such that it renders them yet more unsympathetic—one might say antagonistic—to the native point of view. The ignorance and prejudice of the white man are great hindrances to the understanding of native thought.

When students at home sift, tabulate, and compare the available records they get a wider view of the problems concerned than the investigator in the field is apt to attain. Generalisations and suggestions crystallise out which may or may not be true, but which require further evidence to test them. So the student asks for fresh observations and sends the investigator back to his field.

The term ‘totemic’ has been used to cover so many customs and beliefs that it is necessary to define the connotation which is here employed.

It appears from Major J. W. Powell’s recent account of totemism¹ that the Algonkin use of the term ‘totem’ is so wide as to include the representation of the animal that is honoured (but he does not state that the animal itself is called a totem), the clay with which the person was painted, the name of the clan,² and that of the gens,³ the tribal name, the names of shamanistic societies, the new name assumed at puberty, as well as the name of the object from which the individual is named. He distinctly states, ‘We use the term “totemism” to signify the system and doctrine of naming.’ I must confess to feeling a little bewildered by this terminology, and I venture to think it will not prove of much service in advancing our knowledge. It looks as if there had been some misunderstanding, or that the Algonkins employed the word ‘totem’ to cover several different ideas because they had not definite terms with which to express them. Major Powell’s definitions practically exclude those cults which are practised in various parts of the world, and which by the common consent of other writers are described as totemic.

Professor E. B. Tylor has given⁴ the following clear exposition of his interpretation of the American evidence: ‘It is a pity that the word “totem” came over to Europe from the Ojibwas through an English interpreter who was so ignorant as to confuse it with the Indian hunter’s patron genius, his *manitu*, or “medicine.” The one is no more like the other than a coat of arms is like a saint’s picture. Those who knew the Algonkin tribes better made it clear that totems were the animal signs, or, as it were, crests, distinguishing exogamous clans; that is, clans bound to marry out of, not into, their own clan. But the original sin of the mistake of Long the interpreter has held on ever since bringing the intelligible institution of the totem clan into such confusion that it has become possible to write about “sex totems” and “individual totems,” each of which terms is a self-contradiction. . . . Totems are the signs of intermarrying clans.’

A reviewer in ‘L’Année Sociologique,’ ii. 1899, says (p. 202): ‘One must avoid giving to a genus the name of a species. It will be said these are merely verbal quibbles; but does not the progress of a science consist in the improvement of its nomenclature and in the classification of its concepts?’

Totemism, as Dr. Frazer and as I understand it, in its fully developed condition implies the division of a people into several totem kins (or, as they are usually termed, totem clans), each of which has one, or sometimes more than one, totem. The totem is usually a species of animal, sometimes a species of plant, occasionally a natural object or phenomenon, very rarely a manufactured object. Totemism

¹ *Man*, 1902, No. 75.

² A group that reckons descent only through the mother.

³ A group that reckons descent only through the father.

⁴ *Man*, 1902, No. 1; cf. *Journ. Anthropol. Inst.*, xxviii. 1898, p. 138.

also involves the rule of exogamy, forbidding marriage within the kin, and necessitating intermarriage between the kins. It is essentially connected with the matriarchal stage of culture (mother-right), though it passes over into the patriarchal stage (father-right). The totems are regarded as kinsfolk and protectors or benefactors of the kinsmen, who respect them and abstain from killing and eating them. There is thus a recognition of mutual rights and obligations between the members of the kin and their totem. The totem is the crest, or symbol of the kin.

Sometimes all the kins are classified into two or more groups; for example, in Mabuiag, in Torres Straits, there is a dual grouping of the kins, the totems of which are respectively land and water animals; and in speaking of the latter group my informant volunteered the remark, 'They all belong to the water; they are all friends.' On the mainland of New Guinea also I found that one group of the totems 'stop ashore,' while the other 'stop in water.' When no member of a group of kins in a community can marry another member of that same group, that group is termed a phratry. An Australian tribe is generally divided into two exogamous phratries.

North America is the home of the term 'totem,' and though typical totemism does occur there, it is often modified by other customs. In Australia we find true totemism rampant, and it occurs in Africa, where also it is subject to much modification. Quite recently the Rev. J. Roscoe has published an important paper¹ on the Baganda, in which he describes a perfectly typical case of totemism. Among the Baganda there are a number of kins each of which has a totem, *muziro*. The kin, *kika*, is called after its totem; no member of a kin may kill or eat his totem, though one of another kin may do so with impunity. No one mentions his totem. Old people affirm their fathers found some things injurious to them either as food or to their personal safety, and made their children promise not to kill or eat that particular thing. No man may marry into his mother's kin, because all the members of it are looked upon as sisters of his mother; nor may he marry into his father's kin except in the case of two very large kins. In Uganda royalty follows the totem of the mother, whilst the common people follow the paternal totem. Each kin has its own special part of the country where the dead are always buried. For sympathy or assistance the member of a kin always turns to his particular kin. From what Mr. Roscoe says about the married women of the Green Locust kin, it is evident that the magical aspect of totemism is present as it is in Australia and Torres Straits. The Baganda are thus a true totemic people who are in an interesting transitional condition between matriarchy and patriarchy. Totemic practices also occur in various parts of Asia.

To put the matter briefly, totemism consists of the following five elements:—

1. Social organisation with totem kinsmen and totem symbols.
2. Reciprocal responsibilities between the kin and the totem.
3. Magical increase² or repression of the totem by the kinsmen.
4. Social duties of the kinsmen.
5. Myths of explanation.

Totemism is only one of several animal cults, and it is now necessary to consider certain cults that have been termed totemic before I proceed with the main object of this Address.

¹ *Journ. Anthropol. Inst.*, xxxii. 1902, p. 25.

² The first intimation of this aspect of totemism is entirely due to the researches of Messrs. Spencer and Gillen (*The Native Tribes of Central Australia*, 1899). Dr. J. G. Frazer, appreciating the value of these observations, extended the conception to totemism generally, *Journ. Anthropol. Inst.*, xxviii. 1899, p. 285, read December 14, 1898; the *Fortnightly Review*, April 1899, pp. 664, 665; cf. also 'Israel and Totemism,' by S. A. Cook, *Jewish Quart. Review*, April 1902, pp. 25, 26 of reprint.

Manitu (Guardian Spirit).

Very widely spread in North America was the belief in guardian spirits which appeared to young men in visions after prayer and fasting. It then became the duty of the youth to seek until he should find the animal he had seen in his trance; when found he must slay and preserve some part of it. In cases when the vision had been of no concrete form a symbol was taken to represent it: this memento was ever after to be the sign of his vision, the most sacred thing he could ever possess, for by it his natural powers were so to be reinforced as to give him success as a hunter, victory as a warrior, and even power to see into the future.

The guardian spirit was obtained in various ways by different American tribes, but the dream apparition was the most widely spread. Dr. Frazer¹ calls it 'individual totem'; Miss Fletcher speaks of the object dreamed of (the *wahube* of the Omaha) as the 'personal totem' or simply as the 'totem'; it is termed by the Algonkin *manitu*, by the Huron *okki*, by the Salish Indians *sulia*, and *nagual* in Mexico. Perhaps it would be best to adopt either *wahube* or *manitu* to express the guardian spirit.

Miss Alice C. Fletcher finds that among the Omaha² those who have received similar visions, that is, those who have the same *wahube*, formed brotherhoods which gradually developed a classified membership with initiatory rites and other rituals. These religious societies acquired great power; still later, according to this observer, an artificial social structure, the 'gens,' was organised on the lines of the earlier religious societies. Each 'gens' had its particular name, which referred directly or symbolically to its totem, and its members practised exogamy and traced their descent only through the father. 'As totems could be obtained in but one way—through the rite of vision—the totem of a "gens" must have come into existence in that manner, and must have represented the manifestation of an ancestor's vision, that of a man whose ability and opportunity served to make him the founder of a family.' Mr. C. Hill-Tout,³ in discussing the origin of the totemism of the aborigines of British Columbia, states:—'There is little room for doubt that our clan totems are a development of the personal or individual totem or tutelary spirit, as this is in turn a development of an earlier fetishism.'

Dr. F. Boas points out⁴ that the tribes of the northern portion of the North Pacific group of peoples, such as the Tlingit, Haida, and Tsimshian, have a maternal organisation with animal totems: the clans bear the names of their respective totems and are exogamous. The central tribes, particularly the Kwakiutl, show a peculiar transitional stage. The southern tribes have a purely paternal organisation, and their groups are simple village communities which are often exogamic.

Dr. Boas distinctly asserts⁵ that 'the natives do not consider themselves descendants of the totems; all endeavours to obtain information regarding the supposed origin of the relation between man and animal invariably led to the telling of a myth in which it is stated how a certain ancestor of the clan in question obtained his totem. . . . It is evident that legends of this character correspond almost exactly to the tales of the acquisition of manitows among the eastern Indians, and they are evidence that the totem of this group of tribes is in the main the hereditary manitow of a family.' This analogy becomes still

¹ *Totemism*, 1887, pp. 2, 53.

² 'The Import of the Totem,' *Amer. Assoc. Adv. Sci.*, Detroit Meeting, August 1897.

³ *Trans. Roy. Soc. Canada* (2nd ser.), vii., sect. 2, 1901, p. 6.

⁴ *Report U.S. Nat. Mus.*, 1895 (1897), pp. 322, 323, 334.

⁵ *L.c.*, p. 323.

⁶ But Mr. E. S. Hartland points out (*Folk-lore*, xi. 1900, p. 61) that we have clear evidence from the legends of the descent at all events of some of the clans from non-human ancestors; and Mr. Hill-Tout says: 'Among the Salish tribes it is uniformly believed that in the early days, before the time of the tribal heroes or great transformers, the beings who then inhabited the world partook of the character of both men and animals, assuming the form of either apparently at will.'

clearer when we consider that each man among these tribes acquires a guardian spirit, but that he can acquire only such as belong to his clan. Thus a person may have the general crest of his clan, and besides use as his personal crest such guardian spirits as he has acquired. This accounts partly for the great multiplicity of combinations of crests on the carvings of these people.'

Throughout a considerable portion of North America there appears to be a mixture of variously developed cults of the totem and of the *manitu*. It is not perhaps possible at present to dogmatise as to the relative chronology of these two cults. Personally I am in favour of the superior antiquity of the totem cult, as the conception of an individual spirit-helper appears to me to be of a higher grade than the ideas generally expressed by purely totemic peoples, or what may be gathered by implication from a study of their ceremonies.

The social organisation appears to be very weak in some Californian tribes; our knowledge of the Seri in this respect is very meagre, but Dr. Dixon definitely denies¹ the existence of totemic grouping among the Maidu.

Accepting then for the present the priority of the totem cult, we find a substratum of totemism underlying many of the social organisations in North America. Religious societies are a noticeable feature of the social life of North-west America; those societies have the guardian spirit (*manitu*) as their central idea, but it appears as if the organisation is rooted in a clan² system which has been smothered and virtually destroyed by the parasitic growth. The problems to be solved in North-west America are very complicated, and we must await with patience further researches. It is perfectly evident from the researches of Boas, Nelson, Hill-Tout, and others that comparatively recent great changes have taken place. Dr. Boas indeed states that 'the present system of tribes and clans (of the Kwakiutl) is of recent growth and has undergone considerable changes.'³ An interesting illustration of this is found in the alteration in the organisation of the (Kwakiutl) tribe during the season of the winter ceremonial. 'During this period the place of the clans is taken by a number of societies, namely, the groups of all those individuals upon whom the same or almost the same power or secret has been bestowed by one of the spirits.'⁴ The characteristic North American idea of the acquisition of the *manitu* was evidently also fundamental among the Kwakiutl, as all their tales refer to it, and the whole winter ceremonial is based on it.

I agree in the main with Mr. Hartland⁵ in thinking that, 'whether or no totemism was anciently a part of the tribal organisation, the *manitu* conception is of modern date. It is part of the individualism which is tending, not among these tribes only, to obscure the older communistic traditions.'

Nyarong.

Allied to the *manitu* of North America is the *nyarong*, or spirit helper, of the Iban (Sea Dayaks) of Sarawak. The Iban believe that the spirit of some ancestor or dead relative may come to them in a dream, and this *nyarong* becomes the special protector of the individual. An Iban youth will often retire to some lonely spot or mountain-top and live for days on a very restricted diet in his anxiety to obtain a vision. This custom is called *mampok*. On the following day the dreamer searches for the outward and visible form of the *nyarong*, which may be anything from a curious natural object to some one animal. In such cases the *nyarong* hardly differs from a fetish. In other cases, as the man is unable to distinguish the particular animal which he believes to be animated by his *nyarong*, he extends his regard and gratitude to the whole species. In some instances all the members of a man's family and all his immediate descendants, and if he be a chief all the members of the community over which he rules, may come to share the benefits conferred by the *nyarong* and pay respect to the species of animal in one individual of which it is supposed to reside. 'In such cases,' Drs. Hose and

¹ *Bull. Amer. Mus. Nat. Hist.*, xvii., pt. 2, 1902, p. 35.

² Matriarchal totemic kin.

³ *L.c.*, p. 333.

⁴ *L.c.*, p. 418.

⁵ *Folk-lore*, xi. p. 68.

McDougall remark,¹ 'the species approaches very closely the clan totem in some of its varieties.' Here we have a parallel to the North American custom, but the later stages are not carried as far.

Personally I concur in the opinion expressed by Drs. Hose and McDougall that there is no proof that the peculiar regard paid in Sarawak to animals, the sacrifice of animals to gods or spirits, the ceremonial use of the blood of these sacrificed animals are survivals of a fully developed system of totem worship now fallen into decay. It is very significant that the magical and social aspects of totemism are entirely lacking.

Those who have read Miss Alice Fletcher's sympathetic account of 'The Import of the Totem'² can scarcely fail to recognise that the moral support due to a belief in the guidance and protection of a *wakube* ('personal totem') is of great importance to the individual, and would nerve him in difficulty and danger, and thus proving a very present help in time of need it would surely justify its existence in a most practical manner, and consequently be of real utility in the struggle for existence—a struggle which in man has a psychical as well as a material aspect.

The advantages of totemism are many, but most of them are social and benefit the special groups or the community at large. The hold that the *manitu* has on the individual consists in its personal relation: the man feels that he himself is helped, and I suspect this is the main reason why it supplants totemism. I believe Mr. Lang some years ago suggested the term *manitism* for this cult. If this name be not accepted I venture to propose the revival of the word 'daimon' (*δαίμων*) to include the *manitu*, *nyarong*, and similar spirit helpers, and 'daimonism' as the name of the cult.

Theriomorphic Ancestor Worship.

Dr. Frazer calls attention³ to a publication by Dr. G. McCall Theal⁴ in which he describes the tribal veneration for certain animals, *siboko*. The Bantu believed that the spirits of the dead visited their friends and descendants in the form of animals. Each tribe regarded some particular animal as the one selected by the ghost of its kindred, and therefore looked upon it as sacred. Dr. Frazer says: 'Thus the totemism of the Bantu tribes of South Africa resolves itself into a particular species of the worship of the dead; the totem animals are revered as incarnations of the souls of dead ancestors. This entirely agrees with the general theory of totemism suggested by the late S. G. A. Wilken, and recently advocated by Professor E. B. Tylor.'⁵ But is this totemism? The *siboko* are the residences of the ancestral spirits of the tribe, not of a clan; there is no mention of *siboko* exogamy. Is this anything more than theriomorphic ancestor worship? There can, however, be little doubt that true totemism did occur, and probably universally so, among the Bantu people; but some of the tribes appear to be in a transitional state, and others have doubtless passed beyond typical totemism. The decay of the Bantu totemism in South Africa appears to have been mainly due to a patriarchal organisation combined with a pastoral life.⁶

In describing Dr. Wilken's theory that the doctrine of the transmigration of souls affords the link which connects totemism with ancestor worship, Professor Tylor concludes as follows: 'By thus finding in the world-wide doctrine of soul-transference an actual cause producing the two collateral lines of man and beast which constitute the necessary framework of totemism, we seem to reach at least something analogous to its real cause.' I have already expressed my belief that the animal cults of the Malay Archipelago, so far as they are known at present,

¹ *Journ. Anthropol. Inst.*, xxxi. 1901, p. 210.

² *Amer. Assoc. Adv. Sci.*, Section Anthropology, Detroit Meeting, August 1897.

³ *Man*, 1901, No. 3.

⁴ *Records of South-eastern Africa*, vii. 1901.

⁵ *Journ. Anthropol. Inst.*, xxviii. p. 146.

⁶ E. Durkheim, *L'Année Sociologique*, v. 1902, p. 330; cf. also F. B. Jevons, *Introduction to the History of Religion*, 1902, pp. 155, 158.

cannot be logically described as totemism, and the majority of the peoples of this area have so long passed out of savagery that we are hardly likely to find here an unequivocal clue to the actual origin of totemism.

The reverence paid to particular animals or plants by certain groups of people in Fiji may, as Mr. Lorimer Fison says,¹ 'look like reminiscences' of totemism, but he has 'no direct evidence.' It surely belongs to the same category as the Samoan custom of which Dr. George Brown writes: ² 'In Samoa every principal family had some animal which they did not eat, and I have always regarded this as meaning, not that they thought the animal divine, or an object of worship, but that it was the "shrine" in which their ancestral god had dwelt, or which was associated with some fact in their past history which had led them to adopt it as their totem.' An opinion which Professor Tylor has independently expressed,³ but he naturally dissents from the incarnate god being termed a 'totem.'

I agree with Dr. Codrington⁴ in doubting whether the evidence warrants a belief in totemism as an existing institution in the Southern Solomon Islands. I suspect that totemism has been destroyed over a considerable portion of Melanesia by the growth of secret societies as well as by theriomorphic ancestor worship. Herr R. Parkinson,⁵ however, proves true totemism in the Northern Solomon Islands as the Rev. B. Danks had previously done⁶ for New Britain, Duke of York Island, and New Ireland.

The more one looks into the evidence the more difficult is it to find cases of typical totemism; almost everywhere considerable modification has taken place, often so much so that the communities cannot logically be called totemistic. The magical increase of the totem by the clansmen does not appear to be common, but that may be due to its having been overlooked; on the other hand, magic may be performed against the totems to prevent them from injuring the crops, as in the case of the 'Reptile people' of the Omaha.⁷

Animal Brethren.

Throughout South-eastern Australia and probably elsewhere in that continent there is a peculiar association of a species of animal, usually a bird, with each sex. To take two examples given by Mr. A. W. Howitt,⁸ 'the bird totems of the Kurnai are the Emu, Wren, and the Superb Warbler, which are respectively the "man's brother" and "woman's sister." . . . When we turn to the Kulin we find both the Kurnai totems in just the same position. In addition there are also a second male and female totem, namely, the Bat and the small Night Jar.' Mr. Howitt is careful to point out, 'They are not true totems in the sense that these represent subdivisions of the primary classes; yet they are true totems in so far that they are regarded as being the "brothers" and "sisters" of the human beings who bear their names.' Mr. A. L. P. Cameron⁹ also states that these are 'something different from ordinary totems.' Later Mr. Howitt¹⁰ says: 'Among the Wotjobaluk tribe which have a true totemic system these were real totems although of a peculiar kind. They were called *yaur*, or "flesh," or *ngirabûl*, or *mir*, just as were the totems proper. The only difference was that the Bat was the brother of all the men, while any one totem was the brother only of the men who bore it as their totem. . . . It is evident that the institution of the "man's brother" and the "woman's sister" as totems is very widespread throughout Australia. I have traced it over an extent of about a thousand miles and in tribes having marked differences in language and in social organisation. It seems to be very persistent and enduring, for it remained among the Kurnai in full force

¹ *Ann. Rep. Brit. New Guinea*, 1897-98, p. 136.

² *Ibid.*, p. 137.

³ *Journ. Anthropol. Inst.*, xxviii, p. 142.

⁴ *The Melanesians*, 1891, p. 32.

⁵ *Abh. Ber. k. Zool. Anth. Mus. Dresden*, vii. 1899, Nr. 6.

⁶ *Journ. Anthropol. Inst.*, xviii. 1889, p. 281.

⁷ J. O. Dorsey, *Ann. Rep. Bureau Ethnol.*, 1881-82 (1884), p. 248.

⁸ *Journ. Anthropol. Inst.*, xv. 1886, p. 416.

⁹ *Ibid.*, xiv. 1885, p. 350.

¹⁰ *Ibid.*, xviii. 1888, pp. 57, 59.

after the ordinary social organisation in class divisions and totems had become extinct.' Mr. Howitt speaks of these as 'abnormal totems,' and Dr. Frazer¹ calls them 'sex totems.' As it appears most desirable to distinguish between this cult, which is confined to Australia, and true totemism I propose, in default of a distinctive native term, to call these revered animals 'animal brethren.' Although the natives do not appear to distinguish nominally between these animal brethren and ordinary totems, it does not follow they are to be considered as the same. I am calling attention to an analogous confusion of terms in the totemism of Torres Straits.

I must now pass on to a further consideration of true totemism as understood by Tylor, Frazer, Lang, Hartland, Jevons, Durkheim, and others, as it is impossible within the limits of an Address to give an account of all the varieties of pseudo-totemism.

A Suggestion concerning the Origin of Totemism.

I take this opportunity to hazard a suggestion for a possible origin of one aspect of totemism. Primitive human groups, judging from analogy, could never have been large, and the individuals comprising each group must have been closely related. In favourable areas each group would have a tendency to occupy a restricted range owing to the disagreeable results which arose from encroaching on the territory over which another group wandered. Thus it would inevitably come about that a certain animal or plant, or group of animals or plants, would be more abundant in the territory of one group than in that of another. To take a clear example, the shore-folk and the river-folk would live mainly on different food from each other and both would have other specialities than fell to the lot of the jungle-folk. The groups that lived on the seashore would doubtless have some natural vegetable products to supplement their animal diet, but the supply would probably be limited alike in quantity and variety. Even they would scarcely have unlimited range of a shore line, and there would be one group of shore-folk that had a speciality in crabs, another would have shell-beds, while a third would own sandy shores which were frequented by turtle. A similar natural grouping would occur among the jungle-folk: sago flourishes in swampy land, certain animals frequent grassy plains, others inhabit the dense scrub, bamboos grow in one locality, various kinds of fruit trees thrive best in different soils; the coastal plains, the foot hills, the mountains, each has its characteristic flora and fauna. There is thus no difficulty in accounting for numerous small human groups each of which would be largely dependent upon a distinctive food supply the superfluity of which could be bartered² for the superfluities of other groups. These specialities were not confined to food alone; for example, the shore-folk would exchange the shells they collected for the feathers obtained from the jungle-folk.

It may be objected that in the great prairies and steppes of America, Eurasia, and Australia the natural products are very uniform; but these areas are not thickly populated, and in most cases they probably were only inhabited when the pressure of population in the localities with more varied features forced migration into the open. Certainly these were never the primitive homes of man.

In a recent paper read before the Folklore Society Mr. Andrew Lang put forward the hypothesis that while each primitive human group called itself 'the men' they named the surrounding groups from the names of animals or plants, and hence arose totemism. The idea that there was an intimate connection between the group and the object from which they were nicknamed would soon be developed, and myths of origin would spring up to account for the name. Mr. Lang's theory, still unpublished, regards totem names as given from without for a variety of reasons, amongst which, I understand, he includes my own suggestion.

¹ *Totemism*, p. 51; *The Golden Bough*, iii. p. 416.

² It may be objected that the idea of barter is by no means primitive; but as I believe that sociability was a fundamental characteristic of primitive man I can see no reason why it should not have occurred quite early in a rudimentary sort of way.

His conjecture is based on the similar names, or sobriquets, of villages in the folklore, or *blason populaire*, of France and England, which, again, is almost identical with the extant names of Red Indian totem kindred now counting descent in the male line. Similar phenomena occur in Melanesia with female kin. Mr. Lang is rather indifferent to the causes of the name-giving so long as the name-giving comes from without and applies to groups, not to individuals.

To return to my suggestion. Among the shore-folk the group that lived mainly on crabs and occasionally traded in crabs might well be spoken of as 'the crab-men' by all the groups with whom they came in direct or indirect contact. The same would hold good for the group that dealt in clams or in turtle, and reciprocally there might be sago-men, bamboo-men, and so forth. It is obvious that men who persistently collected or hunted a particular group of animals would understand the habits of those animals better than other people, and a personal regard for these animals would naturally arise. Thus from the very beginning there would be a distinct relationship between a group of individuals and a group of animals or plants, a relationship that primitively was based, not on even the most elementary of psychic concepts, but on the most deeply seated and urgent of human claims, hunger.

There is scarcely any need to point out that the association of human groups with fearsome animals would arise by analogy very early. Hence tiger-man and crocodile-man would restrain the ravages of those beasts (Dr. Frazer¹ describes this as the negative or remedial side of totemic magic); but I take it this was not as primitive as the nutritive alliances. The relation between groups of men and the elements has a purely economic basis; for example, rain is rarely required for itself, but as a means for the increase of vegetable food; similarly the fisherman wants a wind to enable him to get to and from his fishing grounds.

The next phase is reached when man arrived at elementary metaphysical conceptions and endeavoured by sympathetic or symbolic magic to increase his food supply. Naturally the food or product that each group would endeavour to multiply would be the speciality or specialities of that group, and for this practice we now have demonstrative evidence. Though this may be an early phase of totemism I do not consider it the earliest: it can scarcely be the origin of totemism, but it doubtless helped to establish and organise the system.

The essential difference between the view advocated by Dr. Frazer,² and that here suggested is that according to him totemism 'is primarily an organised and co-operative system of magic designed to secure for the members of the community, on the one hand, a plentiful supply of all the commodities of which they stand in need, and, on the other hand, immunity from all the perils and dangers to which man is exposed in his struggle with nature. Each totem group, on this theory, was charged with the superintendence and control of some department of nature from which it took its name, and with which it sought, as far as possible, to identify itself.' Whereas I suggest that the association between a group of men and a species of animals or plants was the natural result of local causes, and that departments of nature were not 'assigned to a particular group' of men. I think it is scarcely probable 'that in very ancient times communities of men should have organised themselves more or less deliberately for the purpose of attaining objects so natural by means that seemed to them so simple and easy.' I suspect that if there was any deliberate organisation it was in order to regulate already existing practices.

To us it might appear that these magical practices could be undertaken by anyone, but this does not seem to have been an early conception. As far as we can penetrate the mind of existing backward man there is a definite acknowledgment of the limit of his own powers. The members of one group can perform a certain number of actions; there are others that they cannot undertake. One group of men, for example, may ensure the abundance of a certain kind of animal, but another will have power over the rain. An interesting example of this limitation is afforded at Port Moresby, in British New Guinea, where the

Motu immigrants have to buy fine weather for their trading voyages from the sorcerers of the indigenous agricultural Koitapu.¹

The remarkable researches of Messrs. Spencer and Gillen in Central Australia prove that it is the function of the kinsmen of a particular totem to perform what are known as *intichiuma* ceremonies, the object of which is to cause the abundance of the species of animal or plant which is the totem of that kin. The descriptions of these ceremonies are well known to students.² I have adduced further evidence of a like nature,³ and from what Mr. Roscoe has found in Uganda we may expect other examples from Africa.

It may be that in some, possibly in all, of the instances of sympathetic and symbolic magic there is a belief that wind or sun, animal or plant, or whatever the objects may be, are animated by spirits akin to those of humankind; but even so, as Dr. Frazer⁴ points out, the action of the magician is a direct one: it does not imply the assistance of other powers who can control the body or spirit of those objects. The data from Australia and Torres Straits point to the conclusion that there is a magical aspect of totemism, which is of great economic importance, and there is no evidence that the officiators at these ceremonies acknowledge the assistance of spiritual powers resident either within the objects themselves or in the form of independent, more or less supreme beings. The existing data do not deny their existence, they simply ignore them in the ceremonies, and so far they are practically non-existent.

According to the suggestion I have ventured to make, the primitive totemic groups ate their associated animals or plants; indeed these were their chief articles of diet. Messrs. Spencer and Gillen point out⁵ that while amongst most Australian tribes a man may not eat his totem, amongst the Arunta and other tribes in the centre of the continent there is no restriction according to which a man is altogether forbidden to eat his totem. On the other hand, though he may, only under ordinary circumstances, eat very sparingly of it, there are certain special occasions on which he is obliged by custom to eat a small portion of it, or otherwise the supply would fail. The Arunta are a peculiar people, while they may be primitive in some respects; in others they are not so, as also has been pointed out by Durkheim.⁶ According to the strict definition of the term, they are not even a totemic people. Judging from the evidence of the legends of the Alcheringa time and the traces of group marriage and mother right, Mr. Hartland⁷ is of opinion that the present disregard by the Arunta of the totem in marriage is a stage in the sloughing of totemism altogether, whereas the *engwura*, or final initiation ceremonies, indicate that the organisation is undergoing a slow transformation into something more like the so-called secret societies of the British Columbian tribes.⁸

The eating of what are evidently the totem animals by the Arunta may possibly be a persistence from an earlier phase, but, without doubt, the totem taboo is characteristic of totemism in full sway.⁹ We have evidence to show that under certain conditions the totem taboo may break down, but this is a later transformation, and indicates a breaking up of the rigid observance of totemism.

Mr. Lang⁹ has made a simple suggestion to account for the origin of the totem taboo. He says: 'These men therefore would work the magic for propagating their kindred in the animal and vegetable world. But the existence of

¹ J. Chalmers, *Pioneering in New Guinea*, 1887, p. 14.

² Baldwin Spencer and F. J. Gillen, *The Native Tribes of Central Australia*, 1899; cf. also J. G. Frazer, *Fortnightly Review*, 1899, pp. 648, 835.

³ *Folk-lore*, xii. 1901, p. 230, and *Report Camb. Anthropol. Expedition to Torres Straits*, vol. v. (in the press).

⁴ *Loc. cit.*, 1899, p. 657.

⁵ *Loc. cit.*, pp. 73, 167.

⁶ *L'Année Sociologique*, v. 1902.

⁷ *Folk-lore*, xi. 1900, pp. 73-75.

⁸ I am fully aware that this appears to cut the ground from under my suggestion; but the latter deals with incipient totemism, and I do not see why the totem taboo should not have arisen from several causes.

⁹ *Magic and Religion*, 1901, pp. 264, 265.

this connection would also suggest that, in common decency, a man should not kill and eat his animal or vegetable relations. In most parts of the world he abstains from this uncousinly behaviour; among the Arunta he may eat sparingly of his totem, and must do so at the end of the close-time or beginning of the season. He thus, as a near relation of the actual kangaroo or grubs, declares the season is open, now his neighbours may begin to eat grubs or kangaroos: the taboo is off.' Dr. Frazer puts forth two suggestions: 'the one is that as animals do not eat their own kind, so man thought it inconsistent to eat his totem kin; the other is a hypothetical idea of conciliation.

I have barely touched upon the relation of social organisation, with its marriage taboo, to totemism. It is by no means certain that the social regulations and customs, which are so much in evidence in a fully developed totemic society, were primitively connected with totemism. So far as the Arunta are concerned, Messrs. Spencer and Gillen believe² the 'totemism appears to be a primary, and exogamy a secondary, feature . . . and that exogamic groups were deliberately introduced so as to regulate marital relations.' But is this primitive?

If one admits that mankind was originally distributed in small groups, which must have consisted of near kin, it does not seem difficult to imagine that marriage would more likely take place between members of contiguous groups rather than within the groups themselves. The attraction for novelty must always have operated, and in the struggle for existence there was always one advantage to be gained by alliances between neighbouring groups, not only from a commissariat point of view, but for offensive and defensive purposes. There is, of course, the converse of this, as wife-stealing would lead to feuds; perhaps daughter-abduction was more frequent, and this probably was not regarded as an offence so serious that a mild scrimmage would not set matters right. It would not take long for wont to crystallise into rigid custom, and custom is always supported by public opinion.

Social regulations must be later than social conditions, and I suspect that the privileges and taboos which run through the social aspect of totemism first arose when totemic groups were in process of aggregation into more complex communities, and afterwards gradually became fixed into a system.

Hero-cults.

The facts to which I have hitherto directed your attention fall well within the sphere of totemism, but I wish now to indicate two interesting departures from typical totemism, both of which occur among the Western tribe of Torres Straits.

I have alluded to the dual grouping of the totem kins at Mabuiag, and an analogous arrangement occurred in the other islands; I propose to speak of each group of kins as a phratry. Strictly speaking, a phratry is a group of exogamous kins within a community; that is, no member of a group of kins (or phratry) could marry another person belonging to the same phratry. The evidence that this is or was the case in the Western tribe of Torres Straits is strong, but it is not absolutely proven.

In Yam, as in the other islands, there is at least one *kwod*, or taboo ground, where sacred ceremonies were held. In the principal *kwod* in Yam there was formerly a low fence surrounding a space about thirty-five feet square in which were the shrines of the two great totems of the island. All that now remains is several heaps of great *Fusus* shells.

Two of the heaps are about twenty-five feet in length. Formerly at the southerly end of each long row of shells was a large turtle-shell (tortoise-shell) mask representing respectively a crocodile and a hammer-headed shark. These were decorated in various ways, and under each was a stone in which the life of the totem resided; stretching from the front end of each mask was a cord to which numerous human lower jaw-bones were fastened, and its other end was

¹ *Fortnightly Review*, 1899, pp. 838-40.

² *Journ. Anthropol. Inst.*, xxviii. 1899, pp. 277, 278.

attached to a human skull, which rested on a stone. Beside the shrine of the hammer-headed shark was a small heap of shells which was the shrine of a sea-snake, which was supposed to have originated from the shark. These shrines were formerly covered over by long low huts, which like the fence were decorated with large *Fusus* shells.

Outside the fence were two heaps of shells which had a mystical connection with the shrine: they were called the 'navels of the totems.'

I have referred to the *intichiuma* ceremonies of the Arunta tribe of Central Australia as being magical rites undertaken by certain kinsmen for the multiplication of the totems. In some cases, apparently, the ceremonies may take place wherever the men happen to be camping; in other cases there are definite localities where they must be performed, as there are in these places certain stones, rocks, or trees which are intimately connected with the magical rites. These spots may be spoken of as shrines. In the island of Mabuiag the magical ceremony for the alluring of the dugong was performed by the men of that kin in their own *kwod*, which was a fixed spot; and doubtless this was the case in the other islands of Torres Straits, for even in the small islands there was a tendency to a territorial grouping of the kins. This localisation of a totem cult has proceeded one step further in Yam Island. Here we have a dual synthesis. The chief totem of each group of kins is practically alone recognised; in other words, the various lesser totems are being absorbed by two more important totems. Each totem has a distinct shrine, and the totem itself, instead of being a whole species, is visualised in the form of a representation of an individual animal, and this image was spoken of as the totem (*augud*). Indeed, the tendency to concretism had gone so far that the life of the *augud* was supposed to reside in the stone that lay beneath the image, and certain heaps of shells were the navels of the totems, a further linkage of the totem to that spot of ground.

A suggestion as to the significance of this transformation is not lacking. There are various folk-tales concerning a family of brothers who wandered from west to east across Torres Straits. Some of them were, in a mysterious way, sharks as well as men. The two brothers who went to Yam were called Sigai and Maiau, and each became associated, in his animal form, with one of the two phratries. The shrines in the *kwod* were so sacred that no women might visit them, nor did the women know what the totems were like. They were aware of Sigai and Maiau, but they did not know that the former was the hammer-headed shark and the latter was the crocodile; this mystery was too sacred to be imparted to the uninitiated. When the totems were addressed it was always by their hero names, and not by their animal or totem names.

Malu, another of these brothers, introduced the cult that bears his name to the Murray Islanders, who form part of the Eastern tribe. He also was identified with a hammer-headed shark. Totemism, as such, had practically disappeared from Murray Island before the advent of the white man, and the great ceremonies at the initiation of the lads into the Malu fraternity were a main feature of the religion of these people.

In Yam totemism was merging into a hero cult; in Murray Island the transformation was accomplished; the one had replaced the other.

In Mabuiag, one of the Western Islands, there was a local hero named Kwoiam whose deeds are narrated in a prose epic. Kwoiam made two crescentic ornaments of turtle-shell, which blazed with light when he wore them at nighttime, and which he nourished with the savour of cooked fish. These ornaments were called totems (*augud*)—presumably because the natives did not know by what other sacred name to call them—and they became the insignia of the two groups of kins of Mabuiag. The crescent which was worn above Kwoiam's mouth was regarded as the more important, and those kins which had land animals for their totems

¹ For the keeping of a soul in an external receptacle, and for Dr. Frazer's views on its bearing on totemism, cf. *Fortnightly Review*, May 1899, p. 844; *The Golden Bough*, iii. 1900, pp. 418-422; and S. A. Cook, *Jewish Quart. Review*, 1902, p. 34 of reprint.

were called from it 'the children of the great totem,' but the water group were called 'the children of the little totem.' There is reason to believe that the dual grouping of the kins is ancient. The erecting Kwoiam's emblems as the head totems of the two groups of kins must be comparatively recent. Here again the primitive association of a group of men with a group of natural objects obtains in the small groups or totem-kins, but in the larger synthesis a manufactured object replaces a group of animals, and this object possesses definite magical powers. There were two navel-shrines connected with the cult of Kwoiam, which were constructed to show that the two *augud* were born there. When it was deemed necessary to fortify the *augud*—that is, the emblems—they were placed on their respective navel-shrines. Further, in Muralug and the adjacent islands Kwoiam himself was a totem (*augud*). Thus in the westernmost islands of the Western tribe the transition from totemism to hero-worship was in process of evolution till it was arrested by the coming of the white man.

To what was this transformation due? It is not very easy to answer this question. We have evidence that in comparatively recent times a change took place in the social organisation of the people, and that the former matriarchal conditions had been replaced by patriarchal. Although superficially the marriage system of the Western tribe appears to be regulated by totemism, Dr. Rivers has found¹ that it is really a relationship system, and that descent, rather than totemism, is the regulating factor. The Eastern tribe, as represented by the Murray Islanders, had progressed further along this road than had the Western tribe. Such a change as this could not fail to have a disturbing effect upon other old customs.

The folk-tales that I collected clearly indicate a migration of culture from New Guinea to the Western tribe, and from the Western tribe to the Eastern tribe. I believe I can demonstrate the migration from New Guinea of a somewhat broad-headed people that spread over the Western Islands but barely reached Murray Island. It is conceivable that the culture myths have reference to this migration, and that the gradual substitution of a hero cult for totemism may be part of the same movement; but, on the other hand, this social and religious change is most thorough in Murray Island, where, I imagine, the racial movement has been least felt. The isolation of Murray Island from outside disturbing factors is very complete, and, being but a small island, a change once started might take place both rapidly and effectively.

It is interesting to note that the totem heroes of the Western tribe were invoked when their votaries were preparing to go to war. I obtained the following prayer in Yam Island:—'O *Augud* Sigai and O *Augud* Maiau, both of you close the eyes of those men so that they cannot see us,' which had for its intent the slaughtering of the enemy without their being able to make a defence. I was informed that when the Yam warriors were fighting they would also call on the name of Kwoiam, who belonged to another group of islands, and on Yadzebug, a local warrior. Yadzebug was always described as 'a man,' whereas Kwoiam and Sigai were relegated to a 'long time' back. From the folk-tales it is evident that Sigai and Maiau are more mythical or mysterious than Kwoiam. We thus have an instructive series: Yadzebug, the local famous man; Kwoiam, the hero, who was also a totem to other people; and Sigai and Maiau, the local totem heroes whose cult was visualised in turtle-shell images, and the life of each of whom resided in a particular stone. Perhaps it would be more correct to speak of this as the grafting of a new cult on totemism rather than to describe it as an evolution of totemism. A transformation has certainly occurred, but it does not appear to me to be a gradual growth—a metamorphosis in the natural history sense of the term—so much as the pouring of new wine into old bottles.

I hope on another occasion to deal with the question of religious and secret societies, as the growth of these has invariably disintegrated whatever antecedent totemism there may have been.

¹ *Reports Camb. Anthrop. Expedition to Torres Straits*, v. 'Kinship' (in the press).

It is highly probable that something like what was taking place in Torres Straits has occurred elsewhere, but I cannot now enter into a comparative study of the rise of hero cults.

Local or Village Exogamy.

I have more than once¹ called attention to the fact that among some Papuans marriage restrictions are territorial and not totemic. Dr. Rivers² has shown that in Murray Island, Eastern tribe of Torres Straits, marriages are regulated by the places to which natives belong. A man cannot marry a woman of his own village or of certain other villages. The totemic system which probably at one time existed in this island appears to have been replaced by what may be called a territorial system. A similar custom occurs in the Mekeo district of British New Guinea, and it is probably still more widely distributed.

I was informed by a member of the Yaraikanna tribe of Cape York, North Queensland, that children must take the 'land' or 'country' of their mother; all who belong to the same place are brothers and sisters, a wife must be taken from another 'country';³ thus it appears their marriage restrictions are territorial and not totemic. The same is found amongst the Kurnai and the Coast Murring tribe in New South Wales.⁴

At Kiwai, in the delta of the Fly River, B.N.G., all the members of a totemic group live together in a long house which is confined to that group. I have also collected evidence which proves there was a territorial grouping of totemic clans among the Western tribe of Torres Straits.⁵

Within a comparatively small area we have the following conditions:—

- (1) A typical totemic community with totem-kin houses (Kiwai).
- (2) A typical totemic community with territorial grouping of the kins. Although there is totem exogamy, the marriage restrictions are regulated by relationship. The former mother-right has comparatively recently been replaced by father-right, but there are many survivals from matriarchy (Western tribe, Torres Straits).
- (3) A community in which totemism has practically lapsed, with village exogamy and marriage restrictions regulated by relationship, patriarchy with survivals from matriarchy (Eastern tribe, Torres Straits).
- (4) Total absence of totemism (?), village exogamy (Mekeo).

I do not assert this is a natural sequence, but it looks like one, and it appears to indicate another of the ways out of totemism. It is suggestive that this order also indicates the application of the several peoples to agriculture: the people of Kiwai are semi-nomadic, those of the Mekeo district are firmly attached to the land. This constraint of the soil must have operated in a similar manner elsewhere.⁶ The territorial exogamy occasionally found in Australia cannot be explained as being due to agriculture; a rigid limitation of hunting grounds may here have had a similar effect.

In offering these remarks to-day I desire, above all, to impress on you the need there is for more work in the field. When one surveys the fairly extensive literature of totemism one is struck with the very general insufficiency of the evidence; as a matter of fact full and precise information is lamentably lacking. The foundations upon which students at home have to build their superstructures of generalisation and theory are usually of too slight a character to support these

¹ *Folk-lore*, xii. 1901, p. 233; *Head-hunters, Black, White, and Brown*, 1901, p. 258.

² *Journ. Anthropol. Inst.*, xxx. 1900, p. 78.

³ *Brit. Assoc. Report*, Dover, 1899, p. 585

⁴ *Frazer, Totemism*, p. 90.

⁵ *Reports Camb. Anthropol. Expedition to Torres Straits*, v. (in the press).

⁶ Cf. *L'Année Sociologique*, v. 1902, pp. 330, 333.

erections with much chance of their permanence. There is only one remedy for this, and that is more extensive and more thorough field work. The problems connected with totemism bear upon many of the most important phases in the social and religious evolution of man, the solution of which can only be obtained within the space of a few years. The delay of each year in the investigation of primitive peoples means that so much less information is possible to be obtained. There is no exaggeration in this. Those who have a practical experience of backward man and who have travelled in out-of-the-way places can testify as to the surprising rapidity with which the old order changeth. In sober earnestness I appeal to all those who are interested in the history and character of man, whether they be theologians, historians, sociologists, psychologists, or anthropologists, to face the plain fact that the only available data for the solution of many problems of the highest interest are daily slipping away beyond recovery.

British Association for the Advancement of Science.

BELFAST, 1902.

ADDRESS TO THE BOTANICAL SECTION

BY

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PRESIDENT OF THE SECTION.

THE visits of the British Association to a particular city recur with a certain irregular frequency and bring with them a temptation to the President of a Section to dwell in his opening Address on the progress made in the science associated with that Section during the interval between such consecutive visits. This course possesses a certain fascination of its own, for it enables us to realise how far the patient investigations of years have ultimately led to definite advances in knowledge and to appreciate the difficulties that have involved disappointments, and that still have to be surmounted. We like to look back upon the struggles, to record the triumphs, to deplore the failures, and to brace ourselves for new efforts. The opportunity afforded hereby for criticism of methods, for reconsideration of what have been held to be fundamental principles, for the laying down of new lines of work based upon longer experience, shows us how desirable such a periodical retrospect may be.

Standing as we do almost at the threshold of a new century, it seems particularly advisable that we shall occupy our thoughts with some such considerations to-day. I do not wish, however, so much to dwell upon the past and to lead my hearers to rest in any way satisfied with the achievements of the last century, phenomenal as they have been, as to direct attention to the future and to place before you some of those problems which at the opening of the twentieth century we find awaiting investigation, if not solution.

I can only attempt to deal with a small portion of the botanical field. These are the days of specialisation, and when anyone is said to be a botanist, the question which arises at once is, Which particular section of botany is he associated with? The same principle of subdivision which cut up the old subject of Natural History into Zoology, Botany, and Geology has now gone further as knowledge has increased, and three or perhaps four departments of botany must be recognised, each demanding as much study as the whole subject seemed to only fifty years ago. I shall therefore confine my remarks to-day to the field of vegetable physiology.

I should like at the outset to recommend this section of botanical work to those of the younger school of botanists who are contemplating original research. To my mind the possibilities of the living organism as such present a fascination which is not afforded by the dry bones of morphology or histology; valuable as researches into the latter are, they seem to me to derive their importance very largely from the past, from the possibility of indicating or ascertaining the line of descent of living forms and the relation of the latter to their remote ancestors. The interest thus excited seems to me to be rather of an academic character

when compared with the actual problems of present-day life, its struggles, triumphs, and defeats in the conflict for existence waged to-day by every living organism. The importance of the study of physiology as bearing upon the problems of the morphologists has, I need hardly say, been fully recognised by the workers in that field. I may quote here a sentence or two from the Address of one of my distinguished predecessors, who said at Liverpool, 'There is a close relation between these two branches of biology, at any rate to those who maintain the Darwinian position, for from that point of view we see that all the characters which the morphologist has to compare are, or have been, adaptive. Hence it is impossible for the morphologist to ignore the functions of those organs of which he is studying the homologues. To those who accept the origin of species by variation and natural selection there are no such things as morphological characters pure and simple. There are not two distinct categories of characters—a morphological and a physiological category—for all characters alike are physiological.'

But apart from the considerations of the claims of vegetable physiology based upon its own intrinsic scientific value and the interest which its problems possess for the worker himself, and upon the place accorded to it as its relationship to morphology, it must, I think, be recognised as being of fundamental economic importance, especially in these times of agricultural depression. For many years now it has been recognised that agriculture is based upon science; that it involves indeed properly the application of scientific principles to the cultivation of the soil. But when we look back upon what has passed for agricultural science since the alliance between the two has been admitted, we cannot but recognise how lamentably deficient in breadth it has been. The chemical composition of the soil and subsoil has been investigated with some thoroughness in many districts of the country. The effect of its various constituents on the weight and quality of the crops cultivated in it has been exhaustively inquired into, and a considerable amount of information as to what minerals are advantageously applied to the soil in which particular plants are to be sown has been acquired. A kind of empirical knowledge is thus in our possession, in some respects a very detailed one, quantitative as well as qualitative records being available to the inquirer. But elaborate as have been the researches in these directions, and costly and troublesome as the investigations have been, they have been hardly, if at all, more than empirical. Till quite recently the physiological idiosyncrasies of the plants round which all these inquiries centred were almost entirely ignored. No serious attempt was made to ascertain the way in which a plant benefited by or suffered from the presence of a particular constituent of the soil. What influence, for instance, has potassium or any of its compounds upon the general metabolism of the plant? Does it affect all its normal nutritive processes, or does it specially associate itself with some particular one? If so which one, and how does the plant respond to its presence or absence by modifying its behaviour? So with phosphorus again; hardly any investigation can be made into the nutritive processes of a plant without this element becoming more or less prominent. In some cases the empirical results already referred to show an enormous influence on the crop exerted by soluble phosphates in the soil or the manure applied to it. But what can yet be said as to the rôle played by phosphorus or by phosphates in the metabolic processes in the plant? Further, how do different plants show different peculiarities in their reactions to these various constituents of the soil? For the advance of agriculture the study of the plant itself must now be added to the study of the soil. The fact that it is a living organism possessing a certain variable and delicate constitution, responding in particular ways to differences of environment, capable of adapting itself to a certain extent to its conditions of life, dealing in particular ways with different nutritive substances, must not only be recognised, but must be the basis for the researches of the future, which will thus supplement and enlarge the conclusions derived from those of the past, in some respects correcting them, in others establishing them on a firmer basis.

In pressing upon the younger school of botanists the importance of this

line of research, I do not wish to minimise the difficulties that accompany it. Difficulties of method assume considerable magnitude, for we have here no question of section cutting and microscopic examination. Vegetable physiology is allied very closely to other sciences, and research into its mysteries involves more than a preliminary acquaintance with them. Especially must one point out the importance, indeed the necessity, of acquaintance with a certain range of organic chemistry and with chemical methods of work. In certain directions, too, physics are as much involved as chemistry in others. The bearing of these sciences in particular directions will be referred to later.

I fear another obstacle stands at the threshold of research which looks sufficiently formidable. The so-called fundamental facts of vegetable physiology have been laid down with sufficient dogmatism in text-books by many writers whose names carry with them such weight that it appears almost heresy to question their statements. We have been content to accept many things on the authority of the great workers of the past, with the result that the advance of knowledge has been hindered by such acceptance of what were deemed facts, but were really inaccuracies. We may refer, for instance, to the statement made by Boussingault, and accepted by most botanists ever since his time, that the absorption of carbon dioxide from the air takes place by means of solution in the cuticle of the epidermal cells of plants and thence passes by diffusion to the seats of photosynthesis. Only comparatively recently has this been shown to be erroneous. If, however, it is once recognised that authority is fallible this apparent obstacle becomes the opposite. The more evident questions have not yet been solved, leaving only the more difficult ones for the present-day worker.

Recognising the importance of work in this field, and realising that with the advent of a new century new departures must be taken, I have thought I might venture to direct the thoughts of my hearers, many of whom I may call my colleagues, to the present position of certain problems which have long been the subjects of speculation and which offer the prospect, if not of complete solution, at any rate of considerable advance if investigated by modern methods.

I turn first to a few questions connected with the nutritive problems of plants in general.

There are several theories abroad as to the progress of events during photosynthesis, none of which can be regarded as entirely satisfactory. For many reasons it seems desirable that this question shall be thoroughly investigated in the light of the present condition of both chemical and physical science. I may perhaps venture to recall to you the principal hypotheses of carbohydrate formation which have been advanced, so that its present position may be properly appreciated.

The view that has met with the widest acceptance is that of Baeyer. On his hypothesis the carbon dioxide absorbed is decomposed under normal conditions to yield carbon monoxide and oxygen; a corresponding and coincident decomposition of water leads to the production of free hydrogen and oxygen. The oxygen from both sources is exhaled, while the carbon monoxide and hydrogen combine to form formaldehyde. The formaldehyde gives rise by a process of polymerisation to some form of sugar.

A modification of this hypothesis has been advanced, which suggests that the preliminary decomposition of the carbon dioxide and the water may not take place, but that by a rather less violent reaction between them the formaldehyde may be formed and the oxygen liberated.

Erlenmeyer has suggested a somewhat different course of reaction, yielding substantially the same results. He thinks it possible that the first interaction of carbon dioxide and water leads to the formation of formic acid and hydrogen peroxide, and that these subsequently interact with each other, yielding formaldehyde and water and giving off oxygen.

Many years after the views of Baeyer appeared, a hypothesis of a different nature was proposed by Orato. He suggests that the carbon dioxide after absorption becomes ortho-carbonic acid, and that this remains in solution in the cell sap. This acid has the structure of a closed benzene ring in which six

molecules are linked together. This becomes decomposed, liberating six molecules of water and six molecules of oxygen, and forming a hexavalent phenol which subsequently undergoes a molecular rearrangement and becomes glucose.

Yet another suggestion was made by Bach in 1893. He points out that when sulphurous acid is exposed to light it becomes transformed to sulphuric acid, sulphur and water being split off, and he argues that a process analogous with this may take place in a leaf. The carbon dioxide uniting with water would form carbonic acid, and this might then split up in the same way as the sulphurous acid. The carbon and the water thus split off are on this hypothesis not set free separately, but in combination as formaldehyde. The higher carbon acid, to which Bach ascribes the formula H_2CO_4 , splits up into carbon dioxide and hydrogen peroxide, and the latter is decomposed into water and free oxygen.

Lieben has still more recently put forward the view that formic acid and not formaldehyde is formed by the first decompositions. He has found that leaves of grasses and various trees yield formic acid among other products when mixed with their own weight of water containing a trace of sulphuric acid, and distilled with steam. Moreover, when carbon dioxide is acted upon by nascent hydrogen the only product is formic acid.

These speculations afford many points which might be well made the starting places of research. The views of Baeyer have met with most acceptance, though but little success has attended the few efforts that have been made to establish them by experiment.

They involve several definite stages of action, of which the most important seem the production of carbon monoxide and hydrogen, the formation of formaldehyde, and the construction of a sugar. The last two questions arise also in connection with the hypothesis of Bach.

If we examine the work that has been published bearing on the probability of the formation of carbon monoxide in the plant we find little that is satisfactory. The statements that have been made are opposed to the idea that carbon monoxide is of value in nutrition; it is said that when supplied to a plant instead of carbon dioxide it does not lead to the formation of carbohydrates. It is further advanced that this gas is of a very deleterious nature, and if formed would result in the speedy death of the protoplasm of the cell in which it originates. This idea is, of course, specious; but it does not appear to be well founded. The deadly character of carbon monoxide when inhaled by a human being depends upon a peculiar interference which it causes with the oxygen-carrying power of the red blood corpuscles. The pigment hæmoglobin to which these little bodies owe their usefulness forms a loose chemical combination with oxygen, the compound being formed in the blood vessels of the lungs and being decomposed with the liberation of the oxygen in those of the tissues of the body. It is evident, therefore, that the value of the corpuscles as oxygen-carriers depends upon their hæmoglobin. When this pigment is exposed to carbon monoxide it combines with it in the same way as it does with oxygen, forming, however, a more stable compound. The affinity for this gas which the pigment manifests is very considerable. Hence the poisonous nature of carbon monoxide. It is easily seen that the latter is a poison because it throws out of gear and temporarily paralyses a most essential part of the mechanism of respiration, effectually preventing oxygen from reaching the tissues of the body. There is no evidence here that it exerts even a deleterious influence upon the living substance itself. The only poisonous effect it would be able to exert on the plant would necessarily be of the latter character, for there is no oxygen-carrying mechanism that could be interfered with. We cannot lay any stress, therefore, on the objection to Baeyer's view, based upon the action of carbon monoxide upon the human organism.

Another possibility may, however, be mentioned. As we shall see later, there are certain resemblances between hæmoglobin and chlorophyll, the vegetable pigment concerned in photosynthesis. May not carbon monoxide enter into some relationship with the latter, and thereby indirectly hinder its activity? Of that, however, there is no reliable evidence, the facts known to us rather pointing in the opposite direction.

The idea of the poisonous nature of this gas may easily be subjected to experimental examination. It would appear easy to expose a plant to an artificial atmosphere made up to different partial pressures of carbon monoxide, to expose it in such atmospheres to various conditions of warmth and illumination and to note the effect produced. It would seem possible to examine a great variety of plants in that way, to try both aërial and aquatic forms, and indeed to test the matter exhaustively. It must be borne in mind, however, that the solubility of carbon monoxide in water is extremely small, and that there may be a great difficulty in getting it brought within the scope of the influence of the living substance on that account. It must necessarily be in solution in the cell sap before it can affect the activity of the chloroplast. Even the relations of solubility are not, however, outside the range of experiment, and it may be that the slightly acid cell sap has not the same peculiarities as water as a solvent for the gas.

It is important again to take into account in such work the factor of sunlight, on which the power of photosynthesis depends. Should carbon monoxide prove capable of serving as a basis for the formation of carbohydrates, the question would arise, Is the activity of the chlorophyll in sunlight confined to the preliminary formation of carbon monoxide from the dioxide, or is the energy derived from the light brought to bear upon the subsequent constructive processes? We have little or no accurate information as to the way in which the energy is utilised after absorption by the chlorophyll.

This opens up a very important but very difficult line of work, which brings home to us the intimate dependence of vegetable physiology upon physics. The absorption of energy from without, in the form of the radiant energy of the solar rays, is certainly a fact, and to a certain extent we can picture to ourselves the way in which it is secured. The spectrum of chlorophyll shows us a number of absorption bands whose position corresponds with the position in the spectrum of the places where oxygen is liberated in photosynthesis. But the transformation and applications of energy in the body of the vegetable organism need much closer examination. The intimate relationship between the different manifestations or forms of energy and the ways in which they can be transformed into one another have been very minutely scrutinised in recent times. What then should hinder us from learning something much more definite than we at present know about these transformations in the rôle of vegetable life? The electrical phenomena connected with the movements of the leaves of the Venus's fly-trap (*Dionæa muscipula*) have been examined with considerable completeness by Burdon Sanderson, and we have learned that the vegetable and animal organisms show considerable similarities in this respect. Recently again Bose has made important contributions to the subject of the electrical responses to stimulation that can be observed under particular conditions. A promising beginning has thus been made, but only a beginning. The electrical condition of the normal plant under different conditions of rest and activity has still to be investigated. If we return to the subject of photosynthesis and the work done by the chloroplast, may we not hope to discover something about the transformation and utilisation of the radiant energy associated somehow with this structure? Considering the relations between the manifestations of energy which we appreciate respectively as light and electricity, it does not seem wildly improbable to imagine that the energy absorbed as the former may lead to a possible electrolysis of carbonic acid under the influence of the chloroplast, with the formation of carbon monoxide and oxygen. Pfeffer has suggested that perhaps the decomposition of the gas is not due to the light rays at all, and that they may exercise only a stimulating influence upon the chloroplast, the energy concerned being derived from heat rays directly absorbed, or heat vibrations derived from the more rapidly vibrating light rays. In this case is the decomposition brought about directly by the heat vibrations, or have we a transmutation into some other form of energy? The whole subject seems at all events a promising subject for inquiry.

Another problem connected with the action of chlorophyll is associated with the absorption of radiant energy by the different regions of the spectrum. Bands of considerable intensity are noticeable in the blue and violet, though the deepest

absorption takes place in the red. Yet Engelmann's classic bacterium method shows us that very little evolution of oxygen takes place in the position of these bands in the blue and violet. The fact that absorption of radiant energy and photosynthetic activity show no quantitative relationship is of course not new, but the reason remains still to be discovered. Van Tieghem has suggested an explanation which recalls to us the hypothesis advanced by Pfeffer, just alluded to. This explanation is that there are two factors concerned in the action of chlorophyll, the elective absorption of light, shown by the occurrence of the absorption bands in the spectrum, and the calorific energy of the absorbed radiations. The failure of the rays of the blue and violet to effect photosynthesis, in spite of their absorption, would on this view be attributable to their possessing but little calorific energy. The latter is associated much more strongly with the deep band in the red, which is the seat of the maximum evolution of oxygen when the spectrum is thrown upon a collection of active chloroplasts. The heating rays alone are ineffectual, as shown by the fact that there is no liberation of oxygen in the region of the infra-red, due no doubt to the fact that chlorophyll does not absorb these rays.

Limiriazeff, in his classical researches on the liberation of oxygen by the leaves of the bamboo when exposed in tubes of small calibre to a large spectrum, found that the amount of carbon dioxide decomposed by leaves is proportional to the distribution of effective calorific energy in the spectrum.

Van Tieghem's hypothesis that this is a matter of calorific energy may prove to be erroneous, and yet his views may rest on some sound basis. It may be a matter in which electrical rather than calorific energy may be concerned.

Returning now to the chemical steps demanded by Baeyer's hypothesis there are certain considerations which may be urged in favour of the view that carbon monoxide really occurs in photosynthesis. It has been ascertained by Norman Collie that when a mixture of gases containing a large proportion of carbon dioxide is exposed at low pressures in a vacuum tube to the action of an electric discharge from an induction coil there is a very large formation of the monoxide, together with oxygen, in some cases as much as 70 per cent. of the gas, undergoing decomposition.

Appealing to the experience of various observers there seems on the whole to be a balance of evidence in favour of the power of plants to live and prosper in an atmosphere containing a very considerable percentage of carbon monoxide.

The question of the possibility of the latter replacing the dioxide, as the theory appears to require, is complicated very seriously by the differences of solubility between them. Carbon dioxide dissolves very readily in water and in cell sap; carbon monoxide is almost insoluble in either. As the amount of a gas taken up by a solvent depends not only on its solubility, but upon its partial pressure, it is very evident that we cannot compare the two gases by admitting the same quantity of both to plants under simultaneous comparison. It is only necessary to supply the dioxide in the proportion of four parts in 10,000; but the almost insoluble nature of the monoxide makes it inevitable that from two to five per cent. shall be experimented with. The same question of solubility makes it almost out of the question to experiment with an aquatic plant.

It would be of considerable interest from this point of view also to inquire whether if carbon monoxide is liberated at the outset of the photosynthetic processes its combination with other groupings can take place apart from the action of chlorophyll. If so the fungi should be capable of carbohydrate construction if supplied under proper conditions with the monoxide and with hydrogen. The proper conditions, however, might be extremely difficult to establish.

The next stage in the constructive process affords still ample room for investigation. The presence of formaldehyde is not the hypothesis of Baeyer alone, but is demanded according to Bach's views, though the stages of its hypothetical construction are not the same. We have therefore to ask whether formaldehyde can be detected in plants, and if so whether the conditions under which it may exist admit of its being considered an up-grade product in photosynthesis. Objections to the theory of its formation may be advanced based upon its un-

doubtedly poisonous nature. Of all the antiseptics now available to the bacteriologists it is perhaps the most potent, even traces being fatal to the form of vegetable protoplasm which is found in bacteria. We may argue that it must be equally deleterious in the cell containing chlorophyll and to the chloroplast itself, as we have no reason to suppose that any difference in vitality exists between the protoplasm of different plants. At first sight this appears an almost insuperable difficulty in the way of the theory. Formaldehyde has, however, the properties of aldehydes in general, one of which is the power of condensation or polymerisation. It passes with extreme readiness into a much more inert form, para-formaldehyde, a body in which three molecules of the formaldehyde are grouped together. It is therefore possible that it may be prevented from exercising its deleterious properties by a transformation at once into this comparatively harmless modification. This will slowly decompose under proper conditions, giving off the free aldehyde.

Pollacci has stated that it is possible to extract formaldehyde from leaves. In his experiments he took such as had been exposed to light for a very considerable period and then macerated them in water. After a sufficient extraction he distilled the leaves, together with the water in which they had been steeped. The first portions of the distillate yielded reactions indicative of the presence of formaldehyde. His experiments do not enable us to say that free formaldehyde was there, for the more stable *para*-form would be likely to decompose during the distillation, so that the reactions would be explained without demanding the presence of the free aldehyde in the leaves.

But little success has attended hitherto the attempt to show that formaldehyde, in the presence of chlorophyll, or preferably, we may say, of chloroplasts, can give rise to carbohydrates. We have nothing more satisfactory than Bokorny's experiments, in which, after failing to set up photosynthesis in a filament of *Spirogyra* fed with formaldehyde, he succeeded when he supplied the alga with its compound with sodium-hydrogen-sulphite. Experiments on a more comprehensive scale, conducted on a variety of plants of different habits, are needed before we can regard the process as satisfactorily established.

We have further to pursue the problem by an inquiry as to the nature of the sugar first formed. Certain considerations lead to the view that it is probable that a sugar of the aldose type must be accompanied in the plant by a ketose. The hypothesis as stated by Baeyer, and so far accepted till quite recently, took no account of the latter. The aldose *grape sugar* was the one always suggested, and from this all others met with have been held to be constructed. The first appearance of a ketose, *levulose*, or *fruit sugar*, has been associated with the hydrolytic decomposition of *cane sugar*, itself constructed presumably from the grape sugar. I fear sufficient attention has not been paid to probability or to the normal course of chemical action in framing our hypotheses, for it is rather difficult to see how some of the transformations somewhat dogmatically affirmed can possibly take place. I may refer in passing to the statement that in the digestion of fat or oil during germination part of it is converted into starch or sugar.

But to return to the construction of sugar. The condensation of formaldehyde, which can be brought about by the action of basic lead carbonate, leads to the formation of several sugars, each yielding its characteristic osazone. How far the condensation in the plant follows this is still uncertain. It is quite possible that stages intervene between formaldehyde and sugar of any kind. It has been suggested that formaldehyde in the presence of water may under the conditions obtaining in the leaf give rise to glycolaldehyde, a body which forms sugar very readily indeed. The formation of sugar directly from formaldehyde is a much longer process and is attended with greater difficulty.

I may call your attention here to the views of Brown and Morris traversing the theory of the primary carbohydrate being grape sugar. In their classical paper on the chemistry and physiology of foliage leaves they have adduced strong evidence, based upon analyses of the sugar-content of leaves of *Tropæolum majus*, that in this plant at any rate the first sugar to be formed is cane sugar. Whether

or no this is the case in plants generally cannot at present be said, though it appears from many considerations probable.

The part played by chlorophyll in photosynthesis has already been touched upon. Remarkably little is known about chlorophyll itself. It has so far been found impossible to extract it from the chloroplast without causing its decomposition, and hence our ideas of its constitution, such as they are, are based upon the examination of something differing in some not well-ascertained particulars from the pigment itself. A remarkable relationship is known to exist between the latter and iron, for unless this metal is supplied to a plant its chloroplasts do not become green. But the condition of the iron in the plant is uncertain; it seems probable that it does not enter into the molecule of the pigment at all. A remarkable series of resemblances between derivatives of chlorophyll and derivatives of hæmatin, the colouring matter of hæmoglobin, has been brought to light by the researches of Schunck and Marchlewski, which is very suggestive. The same leaning towards iron is found in the two pigments, but in the case of hæmatin our knowledge is further advanced than in that of chlorophyll. The iron is known to be part of its molecule. It can by appropriate treatment be removed, and a body known as *hæmatoporphyrin* is then formed, which presents a most striking similarity to a derivative of chlorophyll which has been named *phylloporphyrin*. The two pigments are almost identical in their percentage composition, the hæmatoporphyrin containing a little more oxygen than the other. Both seem to be derivatives of pyrrol. The most striking similarity between them is their absorption spectra, their ethereal solutions both showing nine bands of identical width and depth, those of hæmatoporphyrin being a little more towards the red end of the spectrum. Their solutions in alcohol and ether show the same colour and the same fluorescence. Though they differ in certain other respects, notably the facility with which they form crystals, it is impossible to deny that a close relationship seems probable. If this is established we may by analogy perhaps learn something about the part played by iron in the action of the chloroplast, which so far has proved as obscure as the relation of the metal to the pigment. It is very suggestive to recall the resemblances between the two pigments, the one playing so prominent a part in animal, the other in vegetable life. Both are associated with a stroma of proteid, or possibly protoplasmic, nature, in which a solution of the pigment is retained, apparently after the fashion of a sponge. Both are concerned in metabolic processes in which gaseous interchanges play a prominent part. Both are in some way dependent on the presence of iron for their individuality, even if iron is not actually present in the molecule of both. The iron being removed, the derivatives which are found are almost identical. Further researches may throw a light on this curious relationship, perhaps showing that chlorophyll may enter into a combination with carbon dioxide as hæmatin does with oxygen. Such a combination might well be the precursor of the decomposition of the carbon dioxide which has been already spoken of.

We meet with another pigment in many plants the physiological significance of which has in recent years begun to attract some attention. This is the red colouring matter, *anthocyan*, apparently related to the tannins, which is developed especially in the young leaves of shade-loving plants when they become exposed to illumination exceeding the intensity which they normally encounter. The formation of this pigment is greatest in tropical plants, where it is found usually in the epidermis of the young leaves, though in some cases it extends to the mesophyll as well. The pigment seems in some way to be supplementary to chlorophyll, for its absorption spectrum shows that it allows all the rays useful in photosynthesis to pass through it. It is unlikely that it takes any share in photosynthesis. Several theories have been advanced to explain its presence; it may be simply to protect the delicate cells from the destructive action of too intense light, or to avert the evil of overheating from the solar rays. It has been suggested that certain rays hinder the translocation of starch, and that the pigment shields the cells from the incidence of such rays. Again the view has been advanced that the red colour is important in accelerating the development of diastase from its

antecedent zymogen, which has been found to take place under the influence of the rays of a certain region of the spectrum. While all these views have been advanced, however, there is little positive information bearing upon either the formation or the function of the pigment.

Very little progress has been made with the problem of the construction of proteid matter in the plant, which still confronts us. The question of its relation to the mechanism of photosynthesis has received some attention without leading to any satisfactory conclusion. Winogradski's success in cultivating the nitrate bacteria upon purely inorganic matter reveals an unexpected constructive power in some forms of vegetable protoplasm. The question of the energy made use of in proteid construction is in an equally unsatisfactory condition. Laurent, Marchal, and Carpiaux have stated that the rays of the violet and ultra-violet region of the spectrum are absorbed and devoted principally to the construction of nitrogen compounds from the nitrates, or the compounds of ammonia, which are absorbed by the plant, while the intervention of the chlorophyll apparatus is unnecessary for this purpose. The experiments which they give in considerable detail upon this absorption carry much weight and appear conclusive. Unfortunately other observers have failed to confirm them, so that at present the matter must be left open.

Among the problems connected with the nutrition of the plant, the part played by alcohol has recently come into prominence. Alcohol was originally associated only with the lower fungi, and especially with the yeast plant. Biological problems of grave importance arose in connection with the *Saccharomyces*, apart from what seemed at first the larger question, viz., the nature of fermentation. A prolonged study of the latter phenomenon led Pasteur to the view that alcoholic fermentation is only the expression of the partial asphyxiation of the yeast, and its efforts to obtain oxygen by the decomposition of the sugar. It is hardly necessary here to remind you of the controversies that centred about the question of fermentation and the theories held and abandoned as to its cause. The biological phenomena have, however, a claim now upon our attention in the light of some very remarkable researches that are calling for our attention and criticism to-day. Pasteur's explanation of the behaviour of the yeast was, as we have seen, such as to connect it with the respiration of the plant. When oxygen was withheld from active yeast 60-80 parts of sugar disappeared for one part of yeast formed. When oxygen was present not more than ten parts of sugar were decomposed for the same amount of yeast production. Undoubtedly the stimulus of asphyxiation materially stimulated the yeast metabolism.

But certain observations did not agree with Pasteur's explanation. An energetic fermentation takes place in the presence of oxygen, the plant multiplies extremely quickly, and its metabolism appears very active. Schützenberger argued against Pasteur's explanation with some force, emphasising these points of disagreement between his hypothesis and the facts, and claimed that the matter rather concerned nutrition than respiration. He based his view on experiments carried out to ascertain how respiration was affected under changed conditions.

The results he obtained were briefly the following:—

(1) In a watery liquid without sugar, but containing oxygen in solution, the quantity of oxygen absorbed in unit time by a gramme of yeast is constant, whatever proportion of oxygen is present.

(2) In a saccharine liquid containing albuminous matter as well as sugar, and with oxygen in solution, the same result is obtained, except that the quantity absorbed in unit time is greater.

(3) In two digestions carried on side by side for some time, one being supplied continuously with oxygen and the other deprived of it, the former produced most alcohol.

If the decomposition of the sugar had been the result of the respiratory activity of the yeast cells at the expense of the combined oxygen of the sugar, it would seem that fermentation should either not have taken place at all in the presence of free oxygen, or that it should have been much less than in the other case, whereas the reverse is what is found. Hence Schützenberger advocated the view that the sugar is alimentary and not respiratory.

Certain facts more recently discovered support strongly the view that the nutrition of the yeast is the chief object of the process normally, though we cannot deny that when partial asphyxiation sets in fermentation is resorted to by the plant in its difficulty, that it may obtain the energy normally supplied by the respiratory processes. The mode of decomposition of the sugar, however, the formation of alcohol and carbon dioxide, raises a question as to the exact form in which the nutritive material is supplied to the protoplasm.

Of these more recent discoveries the work of Devaux on the trunks of trees may be mentioned first, as it seems to point to a similar problem to the one connected with yeast. Devaux examined the composition of the air in the interior of woody stems growing under normal conditions, and found that the proportion of oxygen it contains often sinks as low as 10 per cent., while in a few cases, in the most internal part of the tree, he found this gas to be entirely absent. The disappearance of oxygen becomes easier with every increase of temperature. This partial asphyxiation is attended by the formation of alcohol in the struggling tissue, the spirit being detected by cutting up the branches of the trees and distilling them with a large excess of water. Devaux's experiments were made upon a considerable variety of trees, among which may be noted *Castanea vulgaris*, *Pyrus domestica*, *Alnus glutinosa*, *Ulmus campestris*, *Sambucus nigra*, and *Ficus Carica*.

Similar results have been obtained by Mazé in some researches on seeds. When a number of these are submerged in water, micro-organisms being properly guarded against, they do not readily germinate, but their weight nevertheless somewhat rapidly diminishes. In some of Mazé's experiments with peas he ascertained that this diminution was attended by a considerable formation of alcohol. Three parcels of forty peas were examined, weighing respectively 10, 17, and 27 grammes, and the experiments lasted six, twelve, and twenty-seven days. He found the proportion of alcohol to the original weight of the peas was 2.34, 4.63, and 6.56 per cent. As the peas were submerged, and so kept out of contact with air, it seems possible to suppose we have here again an effect of asphyxiation. Other experiments, however, make this view unsatisfactory. He germinated twenty peas at 22° C. for seven days under normal conditions, till their axes were about 1½ inch long. He then covered them with water, in some cases leaving the terminal bud exposed to air. The development of the submerged plants stopped at once, and at the end of five days the liquid contained 130 milligrammes of alcohol. The seedlings whose terminal buds were exposed to the air continued to grow without showing any disturbance. Mazé concludes that the alcohol produced was utilised by them in their growth, and suggests that it is a normal and necessary product of the digestion of carbohydrate material in seeds in course of development.

He goes on to show that alcohol can be demonstrated to be present in plantlets that have germinated for forty-eight hours at 23° C. under normal conditions.

Another worker of great eminence who has found similar conditions to exist in normal vegetation is Berthelot. He put blades of wheat and leaves of the hazel in flasks, displaced the air by hydrogen, and distilled. In the case of the wheat he heated the flask to 94° C., in that of hazel he conducted the distillation by passing steam through the flask. In both he found the distillate contained alcohol. The quantity was not large, but still measurable; from 10 kilos. of leaves he obtained 10 grammes of alcohol.

Mazé claims to have found alcohol under normal conditions in the stems and leaves of the vine.

Mazé finds further that the weight of a seedling of maize approximates at any moment during the early stages of germination to half that lost by the reserve store in the endosperm.

From his experiments, and those of the other authors alluded to, he concludes that alcohol is formed in the living cells of seeds at the expense of grape sugar by virtue of a normal diastasic process, which makes them approach yeast cells more closely than has been suggested by any of the experiments hitherto published. We may inquire further how far the evidence points to the probability that the

molecule of sugar is split up in that way into alcohol and carbon dioxide, and that the alcohol is the nutritive part of the sugar molecule. Certainly Mazé's experiments on the submerged seeds with the plumule exposed above the water are not inconsistent with that view. Duclaux has spoken more definitely still on this point, and has said that the alcohol formed becomes a true reserve material to be used for nutriment.

We have, however, further evidence that to some plants, at all events, alcohol is a food. Laborde has published some researches conducted upon a fungus, *Eurotiosis Gayoni*, which point unmistakably to this conclusion. He cultivated it in a solution containing only the mineral constituents of Rawlin's fluid and a certain percentage of alcohol, usually from four to five per cent. The plant grew well, forming little circular patches of mycelium, which enlarged radially as the growth progressed. The mycelium became very dense in the centre of the patches, and the fungus evidently thrived well. As it grew the alcohol slowly disappeared, the rate being about equal to that of sugar in a similar culture in which this substance replaced the alcohol. The mycelium in some experiments was cultivated quite from the spores. *Eurotiosis* is a fungus which has the power of setting up alcoholic fermentation in saccharine solutions. When cultivated in these alcohol is accordingly produced, and subsequently used, but the growth of the mould is not so easy under these conditions as when the alcohol is supplied to it at the outset.

Duclaux has shown that in the case of another fungus, the well-known *Aspergillus niger*, though alcohol kills it while it is in course of germination from the spore, it can utilise for nutrition 6·8 per cent. when it becomes adult, continuing to grow, and putting out aërial hyphæ. *Eurotiosis* is more pronounced in its liking for alcohol, for it thrives in a mixture containing 10 per cent.; even if submerged entirely it continues to grow and flourish in an eight per cent. solution.

The peculiarity relates only to ethyl alcohol; methyl alcohol will serve as a nutritive medium for only a little time, sufficient only for the commencing development of the spores into a mycelium and disappearing very slowly from the culture fluid. The higher alcohols, propyl, butyl, and amyl, not only give no nourishment, but are poisonous to spores. A very small trace of any of them can be used by the adult mould.

Laborde claims to have established as the result of his investigations that *Eurotiosis* normally makes alcohol from the sugar to nourish itself with it, just as yeast makes invert sugar from cane sugar because it is the nutritive material it likes best. The enzyme zymase is present in the fungus and plays the part of an alimentary enzyme. Its consumption lasts twice as long as that of a corresponding weight of glucose; it can serve twice as long for the nutrition of the same weight of plant.

These remarkable results lead us to the consideration of the mode in which the carbohydrates, and particularly the sugars, are assimilated by the plant. We have held the view that the sugar molecule is capable of entering with little if any alteration into that of protoplasm. We have found no direct evidence bearing upon its fate. It is possible to detect sugar in the axis of a plant till quite near its growing point. Then the reaction ceases to be obtainable, and we know that assimilation is taking place. But we have still to investigate the steps, no very easy problem to undertake. May it possibly be that it is the alcohol moiety of the sugar which the protoplasm takes up, part of the carbon dioxide evolved by the growing organ being an expression, not of respiration, but of a fermentation preliminary to assimilation?

But I feel I have dealt at sufficient length with this question. I pass, therefore, to consider briefly another nutrition problem of a rather different kind. The germination of seeds is a question that might be thought to have been fairly settled by the investigations of the latter half of the last century. We have come to the conception of the seed as fundamentally a young embryo lying quiescent within its testa, and provided with a store of nourishment deposited either within its own substance, or lying round it in the tissues vaguely named endosperm or

perisperm. The nourishment has been held to be practically ready for its use, needing only a certain amount of enzyme action to be applied to it to convert the food store from the reserve to the nutritive condition. We have recognised here starch, proteids, and glucosides, and have ascertained that the embryo can furnish the appropriate enzymes for their digestion. Each reserve store has apparently been quite independent of the rest, and the embryo has had control of the whole.

Certain considerations, however, lead us to the view that for albuminous seeds at any rate this mode of looking at the matter is no longer satisfactory. We may first ask how far the embryo is the controlling factor in the digestion. Putting the matter in another form, is the influence of the parent plant lost when a stable store of food has been provided for the offspring, and does it leave its utilisation entirely to the latter? Is the gametophyte prothallus merely to become a dead or inactive structure as soon as it has developed its young sporophyte, or may its influence extend for the longer period of germination? There are many reasons for thinking this is the case. Indeed the view has been put forward by some observers at intervals for some years. Gris claimed to have shown it in 1864; but it was opposed by Sachs, who said that the enzymes which cause decompositions in the reserve materials are always formed in the young plant or embryo and are excreted by the latter into the endosperm. Some careful experiments on the point were conducted by Van Tieghem and were published by him in 1877. His work was carried out on the seeds of the castor-oil plant. He deprived the seeds of their embryos and exposed them for some weeks on damp moss to a temperature of 25–30° C. After several days of this exposure he found the isolated endosperms were growing considerably, and at the end of a month they had doubled their dimensions. In the interior of the cells he found the aleurone grains to be gradually dissolving, and the oily matter to be diminishing, though slowly. The dissolution extended throughout the mass of the endosperm, and was not especially prominent in the side that had been nearest to the cotyledons. He noted, too, that though starch did not normally appear in the germinating endosperm, under the condition of non-removal of the products of the decomposition, it did appear in the cells in the form of small grains, though not till after several days had elapsed. Van Tieghem also observed that the progress of the decompositions could be arrested and the endosperms made to reassume a quiescent condition, and that then the aleurone grains again became formed, though in less quantity than before.

In some experiments on *Ricinus* which I carried out in 1889 I found much the same sequence of events as Van Tieghem had described. The endosperm unquestionably became the seat of a renewed metabolism, in the course of which many interactions between the various reserve materials became noticeable. It was remarkable that the activity of this metabolism was much more pronounced when the embryo or parts of it were left in contact with the endosperms.

An observation of a similar character has been made by Haberlandt and by Brown and Morris in the case of the seeds of grasses. The conversion of the reserve cellulose of barley grains has been shown by these observers to be the result of the action of an enzyme *cytase*, which is secreted largely by the so-called aleurone layer, which is found surrounding the endosperm, immediately underneath the testa.

Recently my own work has been bearing on this question, particularly as regards the behaviour of the seeds of *Ricinus* during germination. The reserves of this seed are mainly composed of oil and aleurone grains, hardly a trace of carbohydrates being present. At the onset of germination there is a remarkable appearance of both cane sugar and glucose, which increase as the oil diminishes. The old view advanced to explain this fact has been the transformation of the oil directly into the sugars or one of them, a theory which it was difficult to reconcile with the chemical possibilities of oil. I have found that side by side with the appearance of the sugar we have also the formation of a considerable quantity of lecithin, a fatty body containing nitrogen and phosphorus. The seed contains a comparatively large amount of phosphorus in the form of the well-known globoids

of the aleurone grain, a double phosphate of calcium and magnesium. The occurrence of this body points to a considerable interaction of various substances existing in the seeds, the phosphorus apparently coming from the globoids and the nitrogen from the proteids. Instead therefore of the fat being transformed into sugar it seems certain that a very considerable metabolism is set up, in which the various constituents of the endosperm interact very freely together. I am informed by Mr. Biffin, who has investigated the histological changes accompanying the germination, that the protoplasm of the endosperm cells appears to increase in amount very greatly during the early stages. The observations suggest a very vigorous resumption of metabolic activity by the cells of the endosperm, in the course of which the various reserves are brought into relation with the living substance of the cells and a number of new products are formed to minister to the nutrition of the growing embryo. The formation of the sugars may more probably be referred to the renewed activity of the protoplasm of the parent gametophyte than to a direct transformation of the fat under the influence of the embryo. Further researches upon a large variety of seeds appear necessary to give us a true idea of the chemical processes of germination. What now appears probable in the case of fatty seeds may prove to be true also in the case of those which have other varieties of reserve material.

I have already alluded to the problems concerning the electrical phenomena presented by the plant at rest and during activity. Very little work has so far been done in this direction, and our knowledge of the subject is materially less than that concerning similar phenomena in muscle and nerve. Still a beginning has been made, and we have observations on record due to Waller and to Bose which are of the greatest interest, not only because they show a great correspondence in behaviour between animal and vegetable structures, but on account of their possible importance in determining the character of many of the metabolic processes and the forces at work in the tissues.

Some very striking results were only a few months ago published by Bose on the electric response in ordinary plants to mechanical stimulation. He arranged a piece of vegetable substance, such as the petiole of the horse-chestnut, or the root of a carrot or a radish, so that it was connected with a galvanometer by two non-polarisable electrodes. The uninjured tissue gave little or no evidence of the existence of electrical currents; but if a small area of its surface was killed by a burn or the application of a few drops of strong potash, a current was observed to flow in the stalk from the injured to the uninjured area, just as is the case in animal tissue. The potential difference in a typical experiment amounted to .12 volt. The tissue was then stimulated, either by tapping or by a torsion through a certain angle, and at once a negative variation or current of action was indicated, the potential difference being decreased by .026 volt. Very soon after the cessation of the stimulus the tissue recovered and the current of rest flowed as before. Bose's investigations extended considerably beyond this point, and established a very close similarity in behaviour between the vegetable substance and the nerves of animals. Summation effects were observed, and fatigue effects demonstrated, while it was definitely shown that the responses were physiological. They ceased entirely as soon as the piece of tissue was killed by heating.

This remarkable demonstration of similar electrical properties to those possessed by nerve strengthens very greatly the view of the conduction of stimuli in the plant by means of the protoplasmic threads which have been demonstrated by Gardiner and others to exist throughout the plant, uniting cell to cell into one coherent whole.

Much remains to be done in this field; indeed, not more than a beginning has been made. The electrical accompaniments to response to stimuli have been investigated by Burdon Sanderson in the case of *Dionæa*, but many other instances are still awaiting examination. The peculiar phenomena of electrotonus and their relation to stimulus have so far only been observed in animals.

These observations strengthen considerably the view of the identical nature of animal and vegetable protoplasm which has in recent years come into prominence, and which is receiving more and more support in all directions.

These electrical currents, following mechanical action, which no doubt is accompanied by chemical change, make us ask whether electrical phenomena do not in all probability accompany the slow chemical actions which we call metabolism. The view that electrical energy is concerned in the processes of photosynthesis, suggested in an earlier part of this Address, is certainly not weakened by a consideration of these phenomena.

The probability of the transmission of stimuli through vegetable tissue along the protoplasmic threads, extending from cell to cell, has been supported during the last year or two by some remarkable observations claimed to have been made by Nemec on certain roots and other organs. He says he has succeeded in demonstrating a continuous fibrillar structure in the protoplasm of the cells, fibrils passing along it in a longitudinal direction and apparently connecting the protoplasm of a longitudinal series of cells into a conducting chain. These conducting strands extend between the sensitive region—*e.g.*, the tip of the root—and the region which is growing, and which is caused by the stimulus to curve. Nemec says that these conducting strands can be made evident by the use of appropriate staining reagents. They vary in number and position, but appear to be confined to sensitive and motile organs.

It is clear that the matter cannot rest where it is. The statements made by Nemec call for investigation by both histological and physiological methods. It is possible that appropriate reagents may lead to the recognition of structure in what has been hitherto regarded as undifferentiated protoplasm.

Before concluding this Address I may call attention to the vast field opening up in connection with the pathology of plants. The work done by our predecessors has been more largely work on the morphological peculiarities of various fungi than upon the physiological changes which constitute pathology, properly so called. It is only recently that attention has been given to the broad questions of disease in plants. Even now, however, certain advances have been made, and the direction of research is taking shape. In the science of pathology little in recent years has been so fascinating as the question of immunity against the attacks of certain diseases, either hereditary or acquired. It has been bound up with the very large question of toxins and their attenuation, their opposites, the antitoxins, and matters of a similar nature.

Great results have been obtained in human pathology, with which it is not for me to deal. I mention them here because we are face to face with the possibility of treating some of the diseases of plants in a similar way, and perhaps on the threshold of very far-reaching discoveries.

I may call attention to the researches of Ray and of Beauverie upon the general question of plant infection, and especially upon a disease set up by a fungus known as *Botrytis cinerea*, which attacks grapes, begonias, and other plants. The fungus exists in three forms, one of which is a harmless saprophyte, another a destructive parasite, and a third intermediate between the two. The first is a very common fungus, developing on decaying plants and bearing ordinary gonidia or spores. The second is completely filamentous and bears no reproductive organs. It is produced when the air is heavily charged with moisture and the temperature high, conditions of common occurrence in forcing houses. The third is an attenuated form intermediate between the other two. It bears gonidia like those of the first, and in addition others which germinate without falling off the parent plant and elongate into long threads. Many plants can bear the invasion of this plant without suffering greatly, though it cannot be called harmless. It occurs chiefly when a high temperature is associated with a considerable amount of moisture in the air.

It is not difficult to cultivate this attenuated form of the *Botrytis* in sterilised soil. Beauverie describes one experiment made with it which is very striking. Damp earth was sterilised in a Petri dish of large surface, sown with spores of the *Botrytis*, and kept at a temperature of about 16° C. After three days the surface of the dish was covered with a loose mycelium, which bore numerous gonidiophores. The fungus was allowed to grow for some time under these conditions, and the infected earth was then transferred to fresh pots in which were

placed cuttings of begonias. The plants grew well and were not sensibly affected by the presence of the fungus in the substratum or in its surface. Placed subsequently in conditions which were eminently suitable to the development of the parasitic form, they resisted its action perfectly, though control plants which had not been cultivated in the ground infected by the attenuated form were killed very quickly. From their experiments the authors claim to have shown that the form of *Botrytis cinerea* intermediate between the gonidial and the sterile form can make plants immune to the attacks of the latter.

Researches of a somewhat kindred nature dealing with the infection of particular plants by specific fungi have been communicated recently to this Section by Professor Marshall Ward in his paper read last year on the Bromes and their brown rust. They brought to light many very important facts connected with the question of adaptive parasitism and immunity. Few questions in vegetable physiology can compare in economic importance with these when we think of their possible development in relation to agriculture.

I have now somewhat hurriedly surveyed certain parts of the field of vegetable physiology. It has been impossible in an Address like this to do more than indicate what seem to me some of the more important problems awaiting investigation. May we hope that all such work will be vigorously conducted, but that the conclusions reached will be scrutinised with the greatest care and subjected to repeated examination? Great hindrances to the advance of the science resulted from dogmatic assertions made by eminent men in the past, their personal influence having led to their conclusions, not altogether accurate, being nevertheless almost universally accepted. Many years subsequently these conclusions have needed re-examination, the result being the destruction of a whole fabric that had been reared upon this unworthy foundation. I may close, as I began, by an appeal to the younger school of botanists to take some of this work in hand, and by assiduous and critical experiment and observation to contribute to the solution of the problems pressing upon us in this field.

British Association for the Advancement of Science.

BELFAST, 1902.

ADDRESS

TO THE

EDUCATIONAL SCIENCE SECTION

BY

PROFESSOR HENRY E. ARMSTRONG, LL.D., PH.D., V.P.R.S.,
PRESIDENT OF THE SECTION.

I AM sure all will be in agreement with me when, at the very commencement of my Address, I refer to the grievous loss we have suffered through the death of Mr. Griffith. Many of us could count him as our good friend, and are aware that he displayed a fulness of sympathy altogether rare among officials towards all members of the Association. The value of the service he rendered to the Association, on account of the breadth and accuracy of the stores of knowledge at his disposal, although widely felt and recognised, was so veiled by his modesty of manner and the quiet regularity with which he did his work that only the few who came intimately into contact with him can rate his doings at their proper worth. The smoothness with which the proceedings of the Association were carried on from year to year, notwithstanding the great variety of interests represented in it, was in no small measure due to his diplomacy. I have had special opportunities of appreciating his extraordinary versatility, as it has been my good fortune during recent years to come much into contact with him in connection with the Royal Society's Catalogue of Scientific Papers—a work to which he unsparingly devoted himself, rendering thereby important service to science. To no Section of this Association can his death be a greater loss than it is to ours. The foundation of the Section was in large measure due to his sympathetic encouragement and support, and he looked forward to the time when it would be one of the most important and popular in the Association. He thought of it, I know, as the one which was likely to bring about a fuller understanding of the value of science to the community, and eventually to knit a close relationship between the scientific fraternity and the general public. The withdrawal of his counsel has been to me an irreparable loss, and in judging of my shortcomings I trust you will bear in mind that at the critical moment I have been unable to appeal to his balanced judgment for advice.

The last meeting of the British Association at Belfast was presided over by Professor Tyndall, one of whose most memorable discourses was that delivered at Liverpool in 1870 on 'The Scientific Use of the Imagination.' In the course of his Address the President could point out that 'science had already to some extent leavened the world,' and abundant proof has since been given that he was right in claiming that 'it will leaven it more and more.' Nevertheless if we consider the leavening effect which science has had on the public mind, it is impossible to deny that progress is being made in this direction at a woefully slow rate, in no way proportionate to the growth of knowledge, or to the recognised usefulness of the many discoveries which are the outcome of scientific investigation. Science is still treated by society as a rich *parvenu* all the world

over, and is at most invited to its feasts, but not incorporated, as it should be, with the domestic life of the people.

Complaint has long been rife that the British are indifferent as a people even to things which are of manifest importance, and which as a nation of business men they might be expected to value. It would certainly seem that we are all too forgetful of Tyndall's warning that 'every system which would escape the fate of an organism too rigid to adjust itself to its environment, must be plastic to the extent that the growth of knowledge demands.' As our President said a full quarter of a century ago, 'when this truth has been thoroughly taken in, rigidity will be relaxed, things not deemed essential will be dropped, and elements now rejected will be assimilated. The lifting of the life is the essential point, and as long as dogmatism, fanaticism, and intolerance are kept out, various modes of leverage may be employed to raise life to a higher level.'

But how are we to become plastic to the extent that the growth of knowledge demands, in order that rigidity may be relaxed, that conservatism may give way to a wise spirit of advance? Probably there is no more important question the nation can ask at the present time; for that we are wanting in plasticity is proved to demonstration. Does not the shade of our former President stand before us and solemnly give answer: 'By the cultivation and exercise of imaginative power—by the scientific use of the imagination'; for in these days are we not indeed a people 'of little faith'? There would seem, in fact, to be clear evidence, if not of destruction, at least of impairment, of imaginative power under modern conditions—that the tendency of education is to kill rather than to develop the very power on which the progress of the world depends. A dearth of imaginative power is strikingly apparent in art, in literature, in music, in science, in public taste generally, the prevailing tendency being to imitate rather than to originate and individualise. Commentators and critics of sorts abound, but these rarely display any catholicity of judgment. Leaders are few and far to seek. The prevailing policy is that of the party in power, and more often than not of a caucus behind it—not the policy which on broad general grounds is the most desirable; in fact, little attempt is made to discover in any scientific manner what would be the really wise policy to pursue. Nothing could illustrate this better than the state of chaos into which affairs educational are plunged at the present time. Those who dare to differ or offer advice are looked at askance, and always with jealous eyes; and too often everything is done to block the way of the reformer, not from any base motive, but as a rule from sheer inability to appreciate what is proposed—from sheer lack of imaginative power. Necessarily, as the conditions of civilisation become more complex, the tendency to accept and follow must become greater, and self-satisfaction more and more complete and general; but unless effective means be taken to counteract such a tendency, decay is inevitable.

The phrase 'creatures of habit' is familiar to us all: few will deny that we are seldom otherwise than creatures of habit, and that plasticity of mind is a rare attitude. But the growth of knowledge is taking place at such a compound interest rate that a high degree of plasticity is essential if we are to avail ourselves thereof. We were formerly accounted a nation of shopkeepers—of clever shopkeepers—but now the title is passing from us to the Germans and Americans, because they are more alive than we are to the fact that in these days it is necessary both to organise and to be alive to every opportunity. If we would put money in our purse in future, it will be necessary to put imagination into our affairs, so that we may be far more ready to act than we have been of late years.

And not only is knowledge increasing, but our responsibilities are daily becoming heavier and heavier. In the minds of thinking men at the present time the burden of empire our nation bears is of appalling magnitude; the men who have imaginative power are aghast at the flippant unconsciousness of responsibility manifest in the public at large, and even in the majority of our statesmen and politicians. It is widely felt that a deeper sense of responsibility must be induced among us, if we are to maintain our heritage intact—if we are to remain worthy to play the great part for which by an inscrutable ordinance we find ourselves

cast at the very commencement of a new century. Nothing is so sure as that if we cannot show ourselves to be worthy we shall not long be allowed to play the part: jealousy confronts us on all sides; and we have learnt that the struggle for existence is Nature's first law, against which philanthropy is powerless so long as it be not universal—a contingency which is not even remotely possible. It is little short of remarkable that we should be able to go so far as we do in securing the services of able men to conduct our affairs generally; but we cannot be too mindful of the duty incumbent upon us of developing the store of ability latent in the nation, and above all of maintaining intact our heritage of individuality.

The call to organise the forces of our empire is imperative, but we do not heed it in any proper manner. For many years past we have rarely refused to treat with utmost consideration the representations of those who have dwelt on the importance of our Navy. One of the most highly respected men in the country at the present day is our gifted American cousin, Captain Mahan, on account of the way in which he has exercised his powers of imaginative insight and taught us to understand our achievements at sea, to appreciate the true meaning and value of sea power. We need a Mahan to discuss the larger issues of national defence through education, to teach the nation the true meaning and value of education. The Ship of State is of vastly greater consequence than the mere Navy, and yet those who direct attention to the insufficient character of its armament are scarce listened to; not the slightest effort is made to secure for it a scientifically adjusted and organically complete machinery for the effective administration and working of all its departments; the drill of its crew is woefully incomplete; what is worse, there is a terrible absence of organisation and discipline, a terrible absence of willingness, little if any desire, among those who are charged with its care to co-operate, and the consequences of neglect are not immediately obvious. In war we appreciate the effects suddenly: a long list of killed and wounded brings its meaning home to us at once; and we know that we must pay the penalty of defeat forthwith. The indemnity exacted can be expressed as a lump sum. The battle of life is waged in a less obtrusive way, the killed and maimed are not scheduled in any regular manner, and so it escapes our notice that in reality the carnage is awful, that few if any escape without severe wounds, that defeat is constant and yet often dealt so silently and imperceptibly that it excites little comment. But we know that vastly more than is done might be done to alleviate if not to prevent suffering, and even to give charm to life where at present there is but pain, if only our efforts could be organised. If we reflect on the bareness of the life lived by the majority, on the debasing conditions under which very many are placed, on the terrible evils consequent on indulgence in drink, surely we must agree with Tyndall that the essential point is to raise life to a higher level, to elevate the general tone of thought, and that it is our duty to consider more seriously than we have done hitherto what use can be made of the forces at our disposal for the purpose.

If we will but picture to ourselves how most of our difficulties, and especially our slow advance, are consequences of lack of imaginative power, or perhaps rather of failure to exert the power which, though latent in most of us, is not sufficiently called into being by practice; if we will but consider how much of our success has been due to the exercise of imaginative power, we may be led to propound a fruitful theory of education, a theoretical basis on which a sound educational structure may be reared. It has been well said by Carlyle 'that all that man does and brings to pass is the vesture of a thought.' In fact, the illustrations which may be given of the value of theoretical conceptions, of imaginative power, are innumerable. Taking recent events, if we consider the success achieved by the late Mr. Rhodes, the narrow-sighted will say he was a practical man; a man who did things and led others to do. Those with broader views recognise that at heart Mr. Rhodes was a theorist, an idealist, a man of imagination, and hence his success. And men such as Lord Roberts and Lord Kitchener, whose immense services to the nation have been so universally admitted of late, are not merely practical soldiers of experience, but men gifted with powers of insight and imagination; men able to apply theory to practice. Some of those who were

unsuccessful in the late campaign are currently reported to have gone out to South Africa openly deriding science, and it will be well if the lesson taught by their failure be not disregarded by their colleagues. The importance of the part played by theory in science cannot be exaggerated. We have only to think of the influence exercised by the Newtonian theory of Gravitation, by the Daltonian theory of Atoms, by Faraday's conception of Lines of Force, by the Wave theory in its varied applications, by the Darwinian theory of Evolution; we have only to think of the way in which the reflections of one weak man indited at his study-table in a secluded Kentish village have changed the tone of thought of the civilised world. Such theories are the very foundations of science: whilst facts are the building stones, theories furnish the design, and it is the interpretation of facts in the light of theory and the considered application of theory to practice that constitute true science. The marvellous development of scientific activity during the past century has been consequent on the establishment of fruitful theories. If teachers generally would pay more attention to theory their teaching would doubtless be more fruitful of results; facts they know in plenty, but they lack training in the considered use of facts. False prophets among us have long taught the narrow doctrine that practice is superior to theory, and we pretend to believe in it. That the belief is founded on misconception may safely be contended, however; the two go together and are inseparable. It is true that we have enjoyed the reputation of being a practical people, and have been accustomed to take no little pride in the circumstances, and to scoff somewhat at theory, but behind our practice in the past there was a large measure of imaginative power, of theoretical insight; in fact, we were successful because we were innately possessed of considerable power of overseeing difficulties, of grasping an issue, of brushing aside unessential details and going straight to the point: in other words, of being practical. We are ceasing to be practical because modern practice is based on a larger measure of theory, and our schools are paying no proper attention to the development of imaginative power or to giving training in the use of theory as the interpreter of facts: didactic and dogmatic teaching are producing the result which infallibly follows in their wake: sterility of intellect.

Mr. Francis Darwin, in his *Reminiscences* of his father, tells us that 'he often said that no one could be a good observer unless he was an active theoriser.' And he goes on to say: 'This brings me back to what I said about his instinct for arresting exceptions: it was though he were charged with theorising power ready to flow into any channel on the slightest disturbance, so that no fact, however small, could avoid releasing a stream of theory, *and thus the fact became magnified into importance*. In this way it naturally happened that many untenable theories occurred to him; but fortunately his richness of imagination was equalled by his power of judging and condensing the thoughts that occurred to him. He was just to his theories and did not condemn them unheard: and so it happened that he was willing to test what would seem to most people not at all worth testing.'

In his *Autobiography* Darwin remarks:—'I have steadily endeavoured to keep my mind free so as to give up any hypothesis, however much beloved (*and I cannot resist forming one on every subject*), as soon as facts are shown to be opposed to it.' The italics in these passages are mine.

Our system of education has no proper theoretical basis. Educators have ceased to be practical because they have failed to keep pace with the march of discovery: the theoretical basis underlying their profession having been enlarged so rapidly and to such an extent that it is beyond their power to grasp its problems. The priesthood of the craft are, in fact, possessed by the spirit of narrow parochialism, and upholders of an all too rigid creed, being lineal descendants of a privileged class—'the knowledge caste,' to use Thring's expression—whose functions were far more limited than are those which must now be discharged by teachers if teaching is to be given which will serve as an efficient preparation for life under modern conditions. They enlarge *ad nauseam* on the superiority of literary and especially of classical training, forgetting that their preference for classics is but the survival of a practice, and that their arguments in defence of a literary system are but preconceived opinions. Being incapable of

appreciating the arguments used on the other side, it is unlikely that they will ever be able to admit their force.

So long as the forces of Nature were not tamed to the service of man, they could be neglected; sanitary sins were alone found out and punished with unsparing severity. But now it is otherwise. To succeed in competition with others we must be able to avail ourselves of every opportunity: and wide understanding is demanded of us. Moreover the growth of knowledge has induced severe mental hunger, and the feeling that the dainty dishes provided by Nature should be in no selfish manner restricted to the few is a growing one; altruism is a growing force. We feel that we are called upon to counteract the evils arising from the growth of our cities; from the concentration of workers in large bodies; from the minute subdivision of labour; from the depressing conditions under which the masses daily toil. To provide relief and healthy occupation for leisure hours, and to secure that vacuity of mind and pettiness of motive shall no longer be the sore affliction they now are, we must take all the requirements into consideration, and define with utmost minuteness the task in hand; broader and higher ideals than those now prevailing must be established, and practical requirements must be met. To secure the right attitude of mind for this task will not be easy. Few realise, few know, how signal is our failure to appreciate our power, how deplorably we neglect our opportunities. The bareness of the fare we provide is nothing less than shameful in view of the rich possibilities which lie ready to hand. In saying that

A primrose by a river's brim,
A yellow primrose was to him
And it was nothing more,

the poet has well pictured our average attitude towards our surroundings. To the majority indeed a primrose is scarcely a primrose; it is unseen. It is little short of impossible to account for our callous disregard of the wondrous beauty of the multitudinous objects displayed in Nature's realm, our willingness to remain ignorant of the meaning of the mysterious changes which are ever happening before our eyes. That familiarity should breed such contempt is passing strange; but how great the guilt in these days of those who allow the contempt to grow up, knowing as they must that the ignorance is easy to dispel, knowing also that those versed in the mysteries have ever sought to lay bare all that is within their ken. The failure on the part of those who have the charge of education to make a scientific use of the imagination is nothing short of complete; there is nothing to show that the imagination is ever called into play.

Surely it were time to make some real effort to imbue all with a proper understanding of their surroundings, to create in all minds a higher and reverent interest in life.

It is a sad reflection and a grievous blot on our civilisation that our spiritual advisers are mostly so little regardful, so destitute of understanding, of the works of that Omnipotent Power which all must recognise and humbly submit to, whether or no allegiance be acknowledged in doctrinal terms: they before all others should be prepared to consider their inmost meaning, and to direct attention to their wondrous mechanism. We indeed need to send forth a new mission charged with the holy duty of enabling man to appreciate and acknowledge the beauty of the universe, as well as of preparing him to be a thoroughly effective worker, thus fitting him for the true, unselfish, and reverent enjoyment of life. To use the apt words of the Master, quoted by the Poet at the Breakfast-table: 'if for the Fall of man, science comes to substitute the Rise of man, it means the utter disintegration of all the spiritual pessimisms which have been like a spasm in the heart and a cramp in the intellect of men for so many centuries.'

If we can but make sweet use of our present adversity, though we may not be exempt from public haunt but live even in crowded cities, we shall unquestionably soon find

. . . tongues in trees, books in the babbling brooks,
Sermons in stones and good in every thing.

The wonderful prescience of our great poet is nowhere more clearly displayed than in these lines, and it is more than surprising that although generations have been charmed by the music of the words, so little has been done to realise their meaning or to give them a meaning in the minds of the majority.

It is but a question of attitude, for, as Carlyle somewhere says, 'so soon as men get to discern the importance of a thing they do infallibly set about arranging it, facilitating it, forwarding it, and rest not till in some approximate degree they have accomplished that.'

Unfortunately, there are all too many things of which we fail, through our faulty education, to discern the importance, but which a little understanding, the exercise of some slight imaginative power, would enable us to appreciate. I will take the word *Energy* as an example. No word in the English language carries more meaning to those versed in the principles of physical science, and yet how narrow its connotation in the minds of the uninstructed majority. As a guide of practical conduct, no word is of greater significance, and if its true implication fully seized us the word would ever rankle in our ears and serve to remind us of the maxim 'Waste not, want not.' In Great Britain we are using up our coal stores at the rate of over two hundred millions of tons per annum. Used at such a rate, the supply cannot last many generations; whence will our children derive their supplies of energy? Energy cannot be created. When we have squandered the wealth funded on our earth by the sun in æons past, we must fall back on the modicum we can snatch from the daily allowance the glowing orb dispenses, for his largess will for the most part be wasted, and will be very difficult to garner in our country: sun mills, wind mills, and falling water being but irregular and ill-disciplined servants, trees growing but slowly. In all civilised countries the same criminal waste of fuel—of energy—is going on; but although we recognise that individual men have no right to live beyond their means, and have little pity for bankrupts, no corresponding feeling exists on the subject of collective squandering. The spendthrift is regarded with equanimity, because he but distributes his gold among the many—so that the many gain while he alone is the loser, but the energy of fuel is spent irrecoverably and all waste is not merely apparent but real. To waste fuel is to court criminal bankruptcy; but to how many does it occur that we are all parties to such a crime? Does any schoolmaster or schoolmistress call attention to the fact? How many heads of schools could even write a respectable essay on such a topic? When I have suggested 'A piece of coal' as the subject for a scholarship examination essay, I have actually been told by literary critics that you have no right to ask for knowledge of facts in a schoolboy's essay, the object being but to find out to what extent he can 'gas' in flowing periods! A scuttle full of coal excites no emotions in the literary mind; it should be one to call up harrowing visions, as well as a vista of memories extending far back into the ages of time, for in no other stone can we find a more wonderful sermon.

To descend to the ordinary level, how many householders ever take into consideration the wicked waste of fuel which goes on in their establishments? how many are really thrifty in the use of fuel? I never see a 'Kitchener,' or hear it roar, but I shudder. The prevention of smoke is of no consequence in comparison with the prevention of the waste of fuel. Even when every care is taken the waste is very great—simply because our means of utilising the energy of fuel are so imperfect. The best steam engine can recover for us but very few per cent. of the energy stored up in the coal which is burnt in its boiler fire. If we could succeed in burning fuel electrically—in directly converting the latent energy into electricity—it is conceivable that the engine might be of nearly theoretical efficiency. But what imaginative power must be exercised to secure such a result! Cannot we in some measure hasten the time of such discovery? Professor Perry not long ago had the temerity to direct attention anew to the subject in 'Nature,' and made what many practical people will consider the impossible suggestion of a wildly imaginative, irresponsible Irishman: that a round million or so should be devoted to systematic experiments, with the object of discovering means of increasing the efficiency of our engines. If we consider what is the cost of a modern

battleship; if we consider what has been spent on the war in South Africa; if we consider the extent to which the value of the fuel at our disposal would be increased if we could only double the efficiency of our engines and of our stoves, Professor Perry's proposal cannot be regarded as otherwise than modest and sensible. But what is of real importance is the implied suggestion that the subject should be seriously inquired into at national expense. It must, and at no distant date, be admitted that our fuel stores are national assets over which there should be some national control.

I may take Food as another subject of which we fail to discern the importance, and which is outside the schoolmaster's ken, although teachers have stomachs as well as other men, and boys in particular are believed to take some interest in the existence of that organ. It is but a variant on that of energy, as the food we take is mainly of value as the source of the energy we expend—as fuel, comparatively little being required for the construction and repair of the bodily machinery.

. God has made
This world a strife of atoms and of spheres;
With every breath I sigh myself away
And take my tribute from the wandering wind
To fan the flame of life's consuming fire.

Oliver Wendell Holmes.

How many will appreciate this pregnant passage; in how many schools is instruction given which would make it possible to recognise its beauty and completeness as a statement of the philosophy of the respiratory process? Our ignorance of ourselves and of the functions of food is indeed phenomenal. Life involves the unceasing occurrence of a series of changes for the most part chemical. If the proper study of man be man—as the highest dignitary of our Church some time ago asserted it was—the ordinary person would be prone to assume that those in charge of education would so direct studies as to give man some interest in his own wonderful mechanism; instead they almost uniformly direct that true 'culture' consists in knowing what he has thought and written of himself in classic tongues, in ages gone by before the slightest vestige of understanding of the phenomena of life had been obtained. And we moderns calmly suffer this, and at the same time wonder at the way in which primitive peoples allow their medicine men and wizards to dominate them. Taking into account what is known, ours perhaps is relatively a deeper savagery than is that of most untutored races; our educational priesthood are for the most part never trained to a knowledge of the mysteries and deny admission through ignorance rather than

From food to the preparation of food is an easy step—in point of fact the knowledge how to prepare food properly is of far more importance than any knowledge of what food is and does, as on it depends much of the happiness and health of mankind. Cooking is a branch of applied chemistry. We live in a scientific age—an age of knowingness. We might therefore expect that our girls at least would be so trained at school that with little effort they could become knowing cooks. I am not aware that the authorities who lay down the regulations for University Locals or similar examinations have allowed any such vulgar considerations to guide them in drafting their examination schemes: niceties of grammatical construction, recondite problems in Geography and History, the views of an ancient philosopher who gave himself up to angle worship, are alone thought of on such occasions; and yet there are times, it is said, when these august persons deign to take some notice of culinary efforts, and they cannot be unaware that cookery is a subject of some importance, which might well at least be led up to at school. To justify my reference to the subject, let me read a passage from 'An Address on Education,' delivered, not by a narrow-minded Goth who is so lost to reason as to doubt the sufficiency of an exclusively literary training as a preparation for life, but by a classic, the Headmaster of a great public school, Thring of Uppingham, in speaking of the Higher Education of Women at St. Albans in 1886.

'We English are proud of our homes. We sing songs about them, we write on them; in fact, we are very justly *proud of our homes*. Has it ever entered your minds that home to the great majority in a very large degree, and to all in some degree, is but a loftier name for cookery? In a cottage good cookery means economy, luxury, health, comfort, love. . . . Cookery to the vast majority of mankind means home, and when the weary worker comes back from work wanting to refit, cookery alone can turn him out fit for work again. From this point of view home is cookery.'

Cookery is certainly a subject of which those in charge of education have not yet in any way discerned the importance. Our cooks are inferior and wasteful simply because they fail to exercise sufficient imaginative power. If we wish to make good cooks of our girls, we must teach them to think for themselves and to be imaginative—to make a scientific use of their imagination; they will then come to see that the subject is a vastly interesting one, full of opportunity for research. The kitchen, of all places, is the one, in fact, in which the heuristic method should most flourish.

Could we find tongues in trees we should doubtless find them eloquent on the subject of food supply, and far more delicate in their tastes than any mortals. But how many of us, looking at a green leaf, can in any way call to mind the wonderful mechanism which enables the plant to secure the main bulk of its solid substance from the fleeting stores in the circumambient atmosphere; or the manner in which it is dependent on light; or its mineral needs; or its great need of water and its wonderful transpiratory activity? And yet the chief industry of the world is agriculture—the feeding and tending of plants. At least those who lead a rural life should have their imagination excited on such subjects at school; it is even possible that much of the asserted dulness of a country life might pass away if an interest in plant activity were properly cultivated. And schoolmasters might even find comfort in the reflection that, as Messrs. Brown and Escombe have recently shown, the translocation of the material first formed in the leaves, metabolism, and growth are become so intimately correlated that the perfect working of the entire plant is only possible in an atmosphere containing the normal amount of three parts of carbon dioxide per ten thousand; they might recognise in the plant an organism after their own heart, with ripened conservative instincts, and unwilling to accept any other than the limited diet long favoured by the craft.

In these days not only the obvious but also the microscopic forms of life claim attention, and it is imperative that all should be at least aware of their existence and mindful of the deadly power that some of them exercise. All should be able to read with intelligence the wonderful story of the beneficent labours of the great Pasteur—a true saviour of mankind—and appreciate their value. The lessons of sanitary science will never be properly brought home to us and heeded in daily life until a more direct intimacy with micro-organisms is encouraged at school.

And whether or no there be 'good in everything,' children must at least be encouraged to seek it: to use their eyes always, and to reflect on what they see. A proper use will be made of leisure and of holidays when they are so trained, and even 'Days in the Country' will be days of enjoyment and peace for all, never of mere vacuous wanderings, let alone of wanton destruction, and will leave no memories of broken glass and waste paper behind them. And in the end, the national drink bill may be considerably diminished if Shakespeare's words come to have some slight meaning for all.

Let us consider what we can do to further this most desirable end. Section L is in advance of the times, being concerned with a non-existent science—the Science of Education. The science will come into existence only when a rational theory of education is developed and applied; but it is clearly on the very eve of coming into existence, otherwise the Section could not have been established; and we may contribute much to its development.

Surely, the primary article of our creed will be that—as Thring has said—'the

whole human being is the teacher's care,' for all must admit that the faculties generally should be cultivated and educated. At present we make the fundamental mistake of disregarding this truth, but there is evidence that sounder views are beginning to prevail. It is very noteworthy, for example, that in the recent report of the Committee on Military Education it is laid down that *five* subjects are to be regarded as *necessary* elements of a sound general education, viz., English, Mathematics, a Modern language, Latin, and Experimental Science. Moreover it is recognised that each of these subjects has a peculiar educational value of its own. Such a conclusion takes the breath away; indeed, it is almost beyond belief that Headmasters of Public Schools could commit their brethren by attaching their names to a report containing such a paragraph as the following:—

'The fifth subject, which may be considered as an essential part of a sound general education, is Experimental Science, that is to say, the Science of Physics and Chemistry treated experimentally. As a means of mental training, and also viewed as useful knowledge, this may be considered a necessary part of the intellectual equipment of every educated man, and especially so of the officer, whose profession in all its branches is daily becoming more and more dependent on Science.'

Just consider what this recommendation means: that it is now publicly admitted by high authority that *all* boys should have the opportunity given to them at school of gaining knowledge *by experience*—by actually doing things themselves, not merely by reading about them or being told about them, because this, and nothing short of this, is what is aimed at by all who advocate the introduction of Experimental Science as a necessary part of school training. The reign of the cleric as absolute monarch of the school kingdom will be at an end if such doctrine be accepted and acted upon, and there will be some chance of our regaining the reputation of being a practical people. Members of the British Association will be carried back in a dream, some thirty odd years, to 1867, when a report from a Committee, consisting of the General Officers of the Association, the Trustees, the Rev. F. W. Farrar, the Rev. T. N. Hutchinson, Professor Huxley, Mr. Joseph Payne, Professor Tyndall, and Mr. J. M. Wilson, specially appointed to consider the best method of extending Scientific Education in schools, was presented by the Council to the General Committee, and it was resolved: 'That the President of the Association be requested to communicate the Report to the President of the Privy Council,' &c. One among the reasons then given why general education in schools ought to include some training in science was, 'as providing the best discipline in observation and collection of facts, in the combination of inductive with deductive reasoning, and in accuracy both of thought and language.' History does not record what the Privy Council did with the memorial. Had the Council been mindful of its duty to the country and paid serious attention to so weighty a representation our present position might have been a very different one; the German and American bogies would have assumed less portentous dimensions in our eyes, and we might have found ourselves far better prepared than we were to cope with the conditions in South Africa. Accuracy of thought and language, according to the evidence given before the Committee on Military Education, are qualities in which military candidates are particularly lacking, notwithstanding the asserted value of Latin—the chief subject of study in the Public Schools—as mental discipline.

Unless we are prepared to disregard not only all the lessons of the recent war, but also the lessons we have been receiving during years past in the wider war of commercial competition; unless we are prepared to disregard the still wider consideration that education must be an effective preparation for life and not merely for business, the findings of the Committee on Military Education must be embodied in our practice. Undoubtedly the real issue decided by the Committee was the question whether the *antecedent*, and not the technical, training of military candidates was properly conducted. In other words, *our Public School system was on its trial*. Although not referred to in so many words, this system is most effectively condemned in spirit in every line of the Report, and far more between

the lines. But the Committee have merely recognised what has been known for years and years; not a single novel point is brought out—not a single novel issue is raised in their report. By making definite recommendations, however, they have lifted the subject on to a higher plane, and it is these recommendations which require the most careful consideration *and revision*; for if carried out, as they stand, there will be little improvement in our condition. The Committee have certainly done more than they were asked to do, but not more than they were bound to do. By the terms of reference they were to consider and report what changes, if any, are desirable in the system of training candidates *for the Army* at the Public Schools. Instead they have recognised that education at secondary schools has in a great measure conformed to the course generally prescribed by public professional examinations originally designed to secure the selection of candidates who had availed themselves of the advantages of a good general education; and that the State has been careful in the matter of examinations that they should be so framed as not to disqualify or hinder the unsuccessful candidate from entrance into other professions; or, in other words, that neither more nor less is to be exacted from candidates for entrance into the Army than from candidates for other professions. Consequently the requirements to be laid down for Army candidates are such as can be met from a sound general education, and in no way special. The Committee have, in fact, pronounced judgment on the subject of all others which is of greatest consequence to the nation at the moment. But they were not actually appointed for such a purpose, although they should have been, as it was to be foreseen that the major issue must be tried if the minor were to be settled. The modern spirit in education was not sufficiently represented on the Committee. Of the witnesses examined too few had any practical acquaintance with the work of education, although a great many who could judge of its effects gave evidence; and the practical side of education was scarcely considered. Only one witness was examined on behalf of 'Science,' and Mathematics was unrepresented. Such being the case, it is surprising that the Committee should have gone so far in their recommendations, and a proof how overwhelming the case must be in favour of change.

Among the signs of the time showing that liberal views are coming into vogue, I may refer to the provision made in the new buildings designed by Mr. Aston Webb and Mr. Ingress Bell for Christ's Hospital School, which was removed from London in May last. The new home of this ancient foundation is situated in the county of Sussex, about four miles south-west of Horsham, and comprises an area of 1,800 acres of land—meadow, arable, and woodland. Nearly 600,000*l.* have been expended on the new school up to date. Provision is made for 800 boys, and together with the necessary staff, these will form a colony of some thousand persons. The school provides its own water supply, disposes of its sewage by the bacterial system on its own premises, and is lit entirely by electricity generated on the spot. Only food and clothing are derived from the outside. If senior boys, in the future, are allowed to gain some insight into the interior management and economy of such an institution, what wonderful opportunities they will enjoy! And I hope the day is not far distant when boys will learn to understand everything connected with the school in which they pass so many years of their lives. A school should be the last to deny to boys every opportunity of gaining such invaluable experience. Fortunately Christ's Hospital School is conducted on the hostel system; the masters are therefore not charged with household cares, and have no temptation to withdraw their thoughts from the work of education. The school has no taint of commercialism about it. It will be a happy day for our country when this is true of all our schools.

The school buildings are placed nearly in the centre of the site, and cover an area of about eleven acres. They are disposed along a slightly convex line facing southwards, the extremities curving gently towards the east and west respectively. The main range has a frontage of 2,200 feet. At the eastern end, detached from the main range and somewhat retired, are the Infirmary and Sanatorium, which have a frontage of 500 feet. There are extensive playing fields and also a Gymnasium and Swimming Bath.

The scholastic buildings are grouped in the centre around a 'Quad,' 300 feet by 240 feet.

The Dining Hall, 154 feet by 56 feet, behind which are the Kitchens and subsidiary offices, is placed on the north side of the Quad. The Chapel has sole possession of the western side. The School Hall, 130 feet by 50 feet, is at the centre of the southern side, class rooms being provided in two buildings parallel to it, but separated by intervals of 40 feet.

The Science School faces the Chapel, filling the eastern side. The Art School and Library are arranged at right angles to it, somewhat in the background. The Science School consists of four main 'laboratories,' with subsidiary smaller rooms attached to each. No lecture rooms are provided, as Science is to be studied at the work bench; but each of the laboratories has a space arranged so that demonstrations may be conducted within it. The laboratories are fitted up as workshops, as well as in the ordinary way, so that boys may use tools as well as test tubes, and the effort has been made to keep the fittings as simple as possible. Workshops for specific manual instruction will be provided in addition to the Science Schools. Experimental Science will be taught throughout the school. It will be obvious that body, mind, and soul have all been cared for. Whilst due provision has been made for the intake of that energy which is so indispensable to the indulgence in mental effort as well as to the maintenance of the vital machinery, science has received recognition at the hands of the designers of the Buildings, of the Governing Body, and of the Head Master in a manner heretofore unusual: it has actually been placed on an equality even with religion and with literary study, and it may be hoped that the reverent regard of the beauties and wonders of Nature gained in the Science workshops and in the surrounding country will but deepen the feelings of devotion proper to the Chapel and greatly help in lifting the life of the school to a high level. May the example not be without effect.

It has been my privilege to act as the nominee of the Royal Society of London on the Governing Body of the School during several years past, and I may be permitted to bear witness to the manner in which one and all have been mindful of the needs of the times in arranging the new buildings. I believe few Governing Bodies of Schools will do otherwise than promote advance, if properly advised. Resistance to progress comes from within the schools. The public must force the schools to reform.

Let me now return to the recommendations of the Committee on military education. It is to be noted that they clearly involve the recognition of two sides to education—a *literary* and a *practical*. I use the term practical advisedly, because it would be wrong to draw a distinction between a literary and a scientific side, as the whole of education should be scientific, and science—true knowledge—and scientific method—true method—should pervade and dominate the whole of our teaching, whatever the subject-matter; and as the object of introducing experimental science into the school is to give the scholars an opportunity of gaining their knowledge at first hand—by practical heuristic methods, as distinguished from literary didactic methods—the introduction of such discipline may be properly said to involve the recognition of a practical side.

The term practical must not be understood as the antithesis of theoretical. Practice is inseparable from theory in all true teaching, the advance from one practical step to the next being always over a bridge of theory. But if it be granted that education necessarily has two sides, it follows that the Committee on Military Education are illogical in their recommendation that Latin and Experimental Science may be treated as alternative subjects; they are but complementary, not alternative, subjects. The only possible alternative to Latin would be a subject in the literary branch—another language in fact.

But the recommendations of the Committee are also far from satisfactory on the subject of languages. 'The study of languages,' they say, 'forms a third main feature of a sound general education. Of these the most important, from an educational point of view, is Latin. Modern languages, though much inferior to Latin as a means of mental discipline (at least as generally taught), must none

the less be regarded as an important part of a sound general education.' In face of this conclusion it would have been logical to make a modern language rather than Latin the alternative to experimental science, but obviously the Committee dared not omit the modern language. It is true the recognition of experimental science and Latin as possible alternatives may be regarded as a high compliment to the latter, but it was never intended to be such; in truth it marks the recognition of the inevitable: that Latin will ere long be deposed from its high estate, and intellectual freedom granted to our schools, greatly to the advantage of Latin, I believe. There is no doubt that the relative value of Latin as an educational subject is grossly exaggerated; those who dwell on its merits are rarely conversant with other subjects to a sufficient extent to be able to appreciate the effects these would produce if equally well taught. As a matter of fact, in the case of Latin the most capable teachers have been chosen to teach the most capable boys, and the results obtained have been unfairly quoted in proof of the superior value of the subject. We have yet to discover the highest value of other subjects, their depth of power as disciplinary agents having been most imperfectly sounded. And if we consider results, do not they afford proof that the belief in Latin (as taught) is misplaced? It has been the staple subject of education and has been supposed to afford the most valuable training possible in composition.¹ Nevertheless the complaint is general, and not only here but also in Germany—where Latin is far more taught and believed in—that composition is the one subject of all others which the schools do not teach. The fact is, Latin is a subject which appeals to the minority of scholars, and the time of the majority is wasted in studying it. I would give to all an opportunity of proving their aptitude in Latin and Greek, or at least some opportunity of appreciating the construction of these languages; but I am inclined to favour the proposal—made by high authority, I believe—that such studies should follow that of modern languages rather than precede it. The true study of classical languages should be reserved for the University. In any case, it is beyond question that a very large proportion of those who would make magnificent officers are incapable of learning Latin to advantage; such will in future enjoy the inestimable advantage of studying Experimental Science; but if those who take up Latin are in consequence to lose all opportunity of acquiring some power of reading the secrets of Nature, and of thereby developing thought-power and mental alertness—and such must be the effect of the adoption of the recommendations of the Committee—they will prove to be of little value to the army in comparison with their colleagues whose eyes have been trained as well as their 'intellect.' In the course of the evidence given to the Committee, Dr. Warre expressed the view that Science would kill Latin eventually. Nothing could be more unfortunate, but the course adopted by the Committee is that most calculated to bring about such a result, as Latin is thereby put in competition with a subject which must ere long be regarded as a necessary subject of school instruction under all conditions. Latin should be made one of the optional subjects along with Greek.

In their scheme of marks for the examination, the Committee put Latin, French or German, and Experimental Science on an equality by assigning 2,000 marks to each; but English and Mathematics are rated at a higher value, each receiving 3,000 marks. It would have been better to have assigned equal values to the several group-subjects regarded as essential to a sound general education. It should scarcely be necessary to put a premium on the proper study of a man's own language; the subject has naturally a great advantage over others. As to

¹ Dr. Warre was continually harping on this point in his questions to witnesses examined by the Committee. Thus (Q. 3,124): 'I want to put Geography and History into English, and your composition would be tested in that way. We think, for instance, that Composition is admirably taught by translation from Latin or Greek. (To the witness:) Would you agree with that, that translation from another language is teaching English Composition?'

Again (Q. 3,129): 'When officers have talked to us of the uselessness of Greek and Latin, they have neglected the fact that Greek and Latin are the great instructors in English.' *Witness* (the Rev. A. Robertson): 'I quite concur in that.'

Mathematics, there is no doubt that this also is a subject of which the relative value as mental training has been greatly over-valued, and that the methods adopted in teaching it have been very faulty; consequently much time has been wasted and its true value has not been appreciated, as it has been made to appear unnecessarily difficult and forbidding. The evidence before the Committee against Mathematics being carried too far was very strong. Thus Captain Lee, in examining Major-General Sir C. Grove (speaking of the training at Woolwich), said (Q. 604): 'There was an immense amount of pure mathematics and so forth, which one never has occasion to utilise afterwards, unless one becomes an Instructor of Cadets at Woolwich, where you teach them the same useless things you have learned yourself.' This elicited from General Grove the reply: 'Well, there is a strange tendency in Mathematics—I do not know why—that wherever you introduce them they encroach horribly. I am always struggling to cut down advanced mathematics.' And more to the same effect. Again, Lieutenant-Colonel S. Moores, when asked whether he considered the syllabus for the entrance examinations at Woolwich and Sandhurst to be reasonable (Q. 2,353) at once replied, 'No, sir; Mathematics are, in my opinion, very much over-valued as a subject for Army examinations, excepting for the Royal Engineers.'

After all, if reasonable standards were adopted both in Mathematics and Latin, these subjects would not create the difficulty they do in examinations at present by absorbing so much of the time in school that no proper attention can be given to subjects in reality at least of equal importance. It should be insisted that fundamentals be thoroughly taught, and by practical methods, so that the knowledge acquired may be real and usable; it is astonishing how far students may be carried in Mathematics, and how real and interesting the subject becomes, when they grasp the fact that it has a practical bearing.

While dealing with Mathematics, I cannot refrain from quoting a statement made by Captain Lee (Q. 4,209), with regard to the relative values of this subject and science to military men, as the opinion he expressed is of very general application. 'I think it is quite true,' said Captain Lee, 'that a great number of Artillery officers do go through their service without using Science, but I think they feel that any science they know proves of much more practical use to them in their profession than the Mathematics they have learned. As far as I know, in the most scientific branch of the Artillery, the Garrison Artillery, there are practically no occasions where a knowledge of Mathematics is required beyond the Mathematics necessary to solve a simple formula, whereas the lack of knowledge of Electricity, Steam, and Hydraulics is often a serious handicap to the officer.' I will venture to enlarge on this, and say that, assuming Latin, Mathematics, and Experimental Science were taught equally well, by equally sound methods, and that they proved to be of equal value as forms of mental training (though, of course, developing somewhat different faculties), the training gained through Experimental Science would be far the most valuable because the recipients would be brought thereby most intimately into contact with the world, and most fitted to help themselves by having their thought-power developed. Of course this is but an opinion, but one which, I venture to think, many share with me; and yet I make no superior claim for the subject, and ask only that it should rank equally with literary and mathematical training among the necessary subjects of education.

It still remains to consider the specific recommendations of the Committee with regard to Experimental Science, as these are most unsatisfactory. Nothing could be more satisfactory than the manner in which the subject is dealt with by the Committee in their general report, paragraph 20, already quoted (p. 9). But on turning to the scheme of the proposed examination (Appendix A), it appears that not one Experimental Science but two Experimental Sciences are contemplated, viz., Physics and Chemistry, either of which may be taken in preference to Latin and together with English, Mathematics, and French or German. A most important issue is involved in this recommendation, and it cannot be too strongly opposed.

It is very strange, and a proof of how little we are accustomed to act consistently

or to organise, that having found a good thing we rarely make use of it. In the early days of scientific teaching the elementary parts of chemistry and physics were taught as one subject; but gradually, as the individual sciences developed, this healthy practice fell into abeyance. Then time brought its revenge; it was seen that a very one-sided creature was being trained up; that the subjects were in reality interdependent. Moreover, a revolt had been setting in against the formal stereotyped manner in which chemistry was being taught in the schools; this came to a head about 1887, and a better policy was inaugurated by the Reports and scheme presented to Section B of this Association in 1889 and 1890, which condemned 'test-tubing' in favour of problem work, and led to the introduction of the quantitative exercises which are now generally admitted to be of the first importance. Although the scheme dealt primarily with chemistry, being the work of the Chemical Section, it yet had a physical basis; physical measurement, in fact, was its life blood, and all the earlier exercises prescribed in it were in essence physical exercises; moreover the importance of paying some attention to bio-chemical and bio-physical phenomena was not overlooked. As teachers have gained experience of the educational value of the heuristic methods advocated in the British Association scheme, they have been led to apply them more and more widely, and the teaching of Elementary Science has in consequence been regarded with growing favour of late years; more and more has been done to give it the necessary breadth so as to constitute it an effective system of 'Nature Study.'

The University of London—not the reconstituted body of the present day, but the much-abused examining body of the past—after careful inquiry a few years ago advisedly substituted the subject of General Elementary Science for the specific sciences previously prescribed for the Matriculation Examination, and by so doing took a forward step which has generally been admitted by those who can really appreciate the issue to be one of the most important possible from an educational point of view. But the syllabus was imperfectly drawn up—although it had many good points—and the examination was entrusted to men who, besides having little sympathy with the subject, had scant knowledge of school requirements and possibilities. Consequently, the examination was a failure, as everyone foresaw it would be if conducted without proper consideration. The new University has taken the *most unwise* step of reverting to single subjects. It has done far worse than this, however, in making 'science' an alternative subject. Such a reversal of the policy so long pursued by its forerunner can only be described as a *National disaster*. I make this statement with utmost consideration, and trust that the fact that it is so pronounced from the Chair of this Section may give increased force to my opinion.

It may be claimed that the action taken by the Committee on Military Education is in harmony with that approved of by the Senate of the University of London. The only comfort left open to us is that afforded by the proverb that two wrongs do not make a right. Let us hope that wiser counsels will ere long prevail. The consequences of perseverance in so narrow a policy must be very serious. Consider the effect even from a limited professional point of view. It is widely felt that, owing to the growth of knowledge, it is necessary to specialise if we are to do effective work; but this does not mean that we should be uncultured. We know that the very contrary is the case, and that there was never a time when general knowledge was of greater value than it is at the present day. Yet how little this is recognised. The physicist is already unable to understand the chemist. And although the biologist is attempting to unravel almost transcendental problems in chemistry, he has but the most rudimentary knowledge of the subject. What intellectual pigmies we shall be if we pursue so short-sighted a policy; how ineffective must be our treatment of borderland problems. How little right men of science will have to reproach those who have received only a classical and literary training with lack of general culture if we remain so narrow within our own domain. And from a general point of view the outlook is still more serious. The object of introducing Experimental Science into schools is to give training in knowledge of the world, and to cultivate appreciation of its beauties and mysteries. To do this involves resort in some

measure to all the sciences. Chemistry and physics are put first merely because they are of fundamental importance, chemical and physical changes being at the root of all natural phenomena.

As to the value of 'Science' to military men, it is easy to understand that they should have little conception what it may do for them; having never received proper training hitherto, they cannot have had the opportunity of testing its usefulness or of appreciating its merits. But making all allowances, it is difficult to understand an answer such as that given by Lieutenant-Colonel Murray (Q. 4,806) to the Committee on Military Education, viz., that 'Science is a narrowing study for the young mind, and we want to widen and open the mind as much as possible; let them learn their science afterwards' (that is, after the entrance examination). The contention of the advocates of 'Science' has always been that of all subjects it tends most to widen and open the mind. Why attention should be specially called to this answer by the Committee in their report is a riddle; I hope it was because they desired to show they could rise superior to the occasion. But the idea that science 'can be learnt afterwards' is a very common one, and one of the most pernicious abroad. Learning from books and teachers is a lazy method of learning, and the average scholar is corrupted at an early age by exclusive resort to such methods. Much of the mental inertness of the day is acquired at school by over-indulgence in book study. But apart from this, early youth is the period when the mind is most alert and the desire to acquire and experiment greatest; it is the time when the powers of observing and of reasoning can be most easily developed into fixed habits; in fact, if they are not then developed, it is only in exceptional cases that the omission can be rectified in after life. It is too cruel that Mr. Shenstone, the one witness on the subject heard by the Committee on Military Education, should have given expression to the ill-considered opinion that the beginning of the study of Science necessarily comes somewhat later than that of Latin. The statement shows how prone we are to draw false conclusions, how little we think before we speak. The study of Science begins when the infant opens its eyes; every step it takes when it toddles is an attempt to apply the methods of experimental science; some training in scientific method is given in well-conducted Kindergarten schools; but when school is entered, the curtain is suddenly drawn upon all such rational study: if it be the fate of the child to enter a Preparatory school prior to entering a Public school, he is at once referred back to the times of the Romans and Greeks, his teachers being oblivious to the real lesson to be learnt from the study of the scholastic methods of classical times: that the training given to the youth should be such as to fit him to do his work as a man. How can our officers, how can any of us, be otherwise than ill-prepared to do our duty in the world when we are so treated as youths?

Of course all such narrow views, all such narrow actions, as those I have referred to are but consequences of the lack of imaginative power—of our failure to make any scientific use of our imagination. Surely it were time we recognised this, and that we sought to do our duty towards our children. An Arnold who could introduce morality into school method, not merely into school manners, would be a precious gift to the world in these days. Steeped as we are in mediævalism, we need some cataclysm—some outburst of glowing sand and steam such as the world has recently witnessed in the islands of Martinique and St. Vincent—which would sweep away preconceived opinions and give clearness to the atmosphere. American industry is distinguished by the readiness with which manufacturers scrap their machinery and refit. Why cannot we agree to scrap our scholastic and academic ideals, if not our schools and schoolmasters, and refit on scientific lines? If we are to weld our Empire into a coherent whole and maintain it intact, we must do so. Unless we recognise prophets—if progress be allowed to depend on the multitude—we shall perish. And time presses; we cannot with safety much longer remain a 'nation of amateurs.' An appeal must ere long be made to the masses to enforce the provision of leaders; it must be urged upon the men that they see to it that their masters are educated; for

however democratic we may be in our ideals, history teaches, in a manner which admits of no denial, that leaders are the salt of the earth, and in these days leaders need a deal of training to be effective.

Unfortunately it too often happens that those placed in authority are the very last to attempt to march with the times. Bodies such as our Universities, the Education Department, and the Civil Service Commissioners might have been expected to lead the way, to keep the most watchful eye on all that was happening, and to note and apply all improvements. The very contrary has been the case. As a rule, they have advanced only under severe pressure from outside, and scarcely a change can be credited to their initiative. It does not seem to have occurred to them that an Intelligence Department would be a desirable appendage. All suffer from the fatal blot that discretion and authority are vested only in a few heads of departments; the younger and more active spirits have no opportunity granted them while their minds are plastic, full of courage and instinct with advance; so when the time comes that they can act they have lost the desire through inanition. This is the terrible disease from which all our public offices and many industries suffer. It is right to accord experience its proper value, but it is wrong to put aside youthful energy and inventiveness. Our American cousins owe their advance largely to the recognition of these facts.

At bottom the spirit of commercialism is the cause of much of the contorted action we complain of. Neither Cambridge nor Oxford will take the step which has long been pressed upon them—and never more eloquently than by the Bishop of Hereford in his paper read before this Section last year—to make their entrance examination one which would be in accordance with our knowledge and the recognised needs of the times, and one which would have the effect of leading schools generally to impart the rudiments of a sound general education. They cannot act together, and are afraid to act singly, each fearing that it would prejudice its entry if it took a step in advance, and in any way sought to influence the Schools. The Colleges vie with each other in securing the best scholars in the hope of scoring in the general competition. And the Schools have discovered that successes gained in examinations are the most effective means of advertising, and are therefore being turned more and more into establishments resembling those engaged in the manufacture of *pâté de foie gras*, in which the most crammable are tutored without the least consideration of the manner in which lifelong mental biliousness is engendered by the treatment. Parents, with strange perversity, worship the success achieved by Tom and Dick, Mary and Jane, and think they are doing their duty by their children in allowing them to be made use of—for private ends. The worst feature of the system is the narrow spirit of trades unionism which it has engendered, which leads to the worship for ever afterwards of those who have gained the prizes, instead of regarding them but as victors for the moment and requiring them at each step to give fresh proof of power. Nothing is more unwise than the way in which we overrate the pretensions of the 'first class' man; we too often make a prig of him by so doing. Those who succeed in examinations are too frequently not those most fitted for the work of the world. A long experience has convinced me that the boys a few places down a class are, as a rule, the best material. Those at the top may have acquisitive power, but more often than not they lack individuality and the power of exercising initiative. We must base our judgment in the future on evidence of training and of general conduct, not on isolated examinations. If any sincerity of purpose be left in us, if any sense of the value of true training—of what constitutes true training—can be rescued from the scholastic wreck on which we find ourselves at present embarked, we must institute some form of leaving examination which will give the requisite freedom to the schools and every opportunity for the development of individuality, and at the same time necessitate thoroughness of training and patient regard of every grade of intelligence; leaders will show themselves and will not need to be examined for. Examinations as commercial enterprises must suffer an enforced bankruptcy.

Racing studs must be regarded as luxuries in schools and kept apart from the ordinary stables, these being regarded as the first charge upon the establishment, as

the serious work of the world will fall upon their occupants. In other words, special provision must be made for scholars, and they must not be allowed to monopolise attention and set the pace to the detriment of the majority. When Carlyle made the statement that we had in our islands a population of so many millions, mostly fools, he stated what is only a half truth. He failed to realise that the foolishness is very largely begotten of neglect and want of opportunity, not innate. Our schools mostly fail to find out the intelligence latent in the great majority of their pupils, and give it little chance of developing by offering them a varied diet from which to select. During a long experience as a teacher, I have over and over again seen weaklings develop in course of time into strong men when they have been properly encouraged and an opportunity at last found for the exercise of their 'talents.' The Briton is in this respect a most mysterious creature; you never know when it is safe to call him a fool. All are agreed that the mistakes in the recent war were not due to lack of intelligence, but to lack of training. There can be no doubt of that. All who have taught in our colleges will, I am sure, agree with me that the material sent up from the schools is in substance magnificent, but too often hopelessly unfit to benefit from higher teaching. The things said of those who enter for the military profession are as nothing in comparison with what could be said of those who enter for the professions generally. If our young people fail to show intelligence in later life, it is as a rule because the conditions under which we place them in earlier life are such as not only to leave their intelligence undeveloped, but, what is far worse, such as to mar their ability. The best return we can make to those who did such magnificent service in the late war will be to take to heart the real lessons taught by the mistakes, and to see to it that their children and successors generally are trained in a happier school than that in which they were placed.

Examining bodies at the present time do not appear to realise the full measure of their responsibility. To examine well is at all times a difficult task, far more difficult than to teach well. The examiner wields a large measure of authority, and it is imperative that he should exercise this wisely. Examiners should therefore be chosen with extreme care and with due regard to their fitness for the work; but this too rarely happens; the choice falls too frequently on specialists, with little knowledge of educational requirements and possibilities. The examination of boys and girls is far too often put into the hands of those who have no real knowledge of the species and little sympathy with its ways.

There are three courses open to examining bodies—to lead, to maintain themselves just abreast of the times, to stagnate. As a matter of fact, the last is that almost invariably chosen—a syllabus, when once adopted, remaining in force year after year. Consequently, examinations tend to retard rather than to favour the introduction of improved methods of teaching. It is impossible to justify a policy which has such results. The evil effect of examinations would be less if the syllabus were abolished, and the limits of examinations very broadly indicated; this is done in some cases, and might be in all. The incompetent examiner and teacher are not in the least helped by the conventional curt syllabus, but the liberty of action of the competent examiner and teacher, and their desire to effect improvements, are materially limited by it. The competent examiner should know what is a fair demand to make of a particular class of students, and should be in a position to take count of the advances that are being made; and the competent teacher should be able to do all in his power to make the teaching effective, and be secure in feeling that his efforts could not fail to be appreciated. To take my own subject, the chemistry syllabus recently laid down for the London Matriculation examination is quite unsuited to its purpose and most hopelessly behind the times. The scheme put forward in the report of the Committee on Military Education is but a bag of dry bones. In the case of several subjects, the South Kensington schemes are full of the gravest faults, their hoary antiquity being their least objectionable feature. Surely a national institution, dispensing public funds, should be the last to hold back the nation; it should be provided with machinery which would enable it to march with the times. In making this criticism I should like to recognise the great work done by Sir William

Abney in instituting reforms; but one swallow does not make a summer; a self-acting, governing mechanism is needed which would at all times maintain the balance of practice with progress.

If we consider the process by which decisions on such matters are arrived at, even in the bodies representative of very large interests, it is a curiously imperfect one. Usually very few individuals are concerned. We are all still imbued with primitive instincts. In some way two parties arise, and the question is, which shall conquer? More often than not the true inwardness of the issue presented is left out of account—the considered opinion of the day is scarcely asked for, or if opinions are collected they are not weighted. Therefore calm reason is rarely the arbiter. The conditions of modern civilisation require that some better method shall be devised, which will really enable us to do that which would be of the greatest good to the greatest number. We do not sufficiently remember that while we are tilting, the enemy at our gates is contemplating our failure to maintain and strengthen our fortifications, and quietly advancing his forces to the attack. Speaking of the Navy in the House of Commons not long ago, Mr. Arnold Forster said: 'There was a need for some reinforcement of the intellectual equipment which directed, or ought to direct, the enormous forces of our Empire.' Surely we may take these words as true generally.

At the present time, when the responsibility of controlling all grades of education is about to be cast upon the community, and the actual call to arms is imminent, it is imperative that a sound public policy should be framed, and that nothing should be allowed to stand in the way of the public good. It cannot be denied that School Boards have done most admirable service; but there are many who are convinced that in not a few respects they have been disastrous failures, and that we need a wider organisation, penetrated with sounder and especially with more practical views. The one essential condition of success is that the public itself treat the matter seriously, realising that their own immediate interests are at stake, and that they will be the first to suffer if those who are chosen by them to formulate the new policy and to supervise the work of education are unqualified and, to emphasise my meaning, let me add, unpractical. If the State is to retain any measure of authority, it too must be prepared to exercise that authority wisely. The blame to be put upon School Boards in England for having allowed an unpractical system of education in the schools is as nothing compared with the blame to be put upon the Education Department for having allowed such a system to grow up by the adoption of academic ideals and academic machinery. Until recently, it was a disqualification for an inspector to have teaching experience. A good degree, if not political influence, was the one qualification. Consequently men were chosen whose practical instincts had never been developed, who knew nothing of practical life and of common-place requirements, and nothing of children and their ways; with rare exceptions the inspectors could look at education only through literary blinkers. To intensify the evil the wicked system of payment by results was introduced. An inspector such as I have described, working under such a system, could not do otherwise than destroy teaching.¹

The first necessary step to take will be to reorganise the Education Department, root and branch; to imbue it throughout with sound ideals, and lead it to understand its great importance as the head centre of the Educational system: for dis-establish as we may, and however much we may favour local self-government, a head centre there must be to correlate the efforts made throughout the country and to distribute wisdom; but its functions will be those of an exchange and inquiry office rather than directive and assertive. At least, such is my reading of the tendency of the *Zeitgeist*. Such a Department will have an Intelligence Board, whose members are partly official, partly unofficial, so that it may maintain itself in

¹ The inspector destroys teaching, because he is bound by law and necessity to examine according to a given pattern; and the perfection of teaching is that it does not work by a given pattern (Thring).

constant touch with outside opinion and effort. One function of this Board will be to preside at a monthly bonfire of red tape and official forms; for in future, even if no other subject of Government concern be kept in a lively and living state, education must infallibly be. The whole staff of the office, including the inspectorate, will be required to avail itself of that most valuable institution, the sabbatical year, *i.e.*, to spend every seventh year in some other employment, so that they may not forget that the world has ways sometimes different from those pictured within the office, and which it is advisable to take note of in education. Refreshed and invigorated, they will return to work, prepared to sacrifice all sorts of traditions, and to recognise the existence of short cuts across fields which had before appeared to be of interminable dimensions; and as it will be required that they spend a certain proportion of their close time in the company of children—if they have none of their own—they will learn that a child has ways and views of its own, none the less interesting and worthy of consideration because they are somewhat different from those of grown-up people.

It is fortunate that the Technical Education Movement has been coincident in England with the development of the School Board system. Those engaged in it have worked untrammelled by official requirements, and much original thought has been enlisted in its service. In essence it has always been a revolt against the academic ideals permeating University education and the schools generally; the faults of the schools, in fact, are the more obvious in the light of experience gained in technical education, which will now come to our aid in correcting them.

The really serious tasks before those who direct the work of education in the immediate future will be the choice of a programme and the provision of capable teachers. If they enter on these tasks with a light heart, God help our nation; they will thereby give proof that they have no true conception of the great responsibility attaching to the position they occupy. Let no man offer himself for the work unless he feels certain that he is in some degree qualified.

As to the programme, it may be said that that is for the teachers to settle; and so it should be. But it cannot be denied that by long-continued neglect to read the writing on the wall, they have lost the claim to legislate; they have shown that they do not know how to legislate. The public must lay down the programme in its broad outlines; teachers must fill in the details. The task imposed upon the schools will be to develop the faculties generally—not in the lop-sided manner customary heretofore—and especially to develop thought-power in all its forms and the due application of thought-power.

I believe that gradually a complete revolution must take place in school procedure, and that the school building of the future will be altogether different from the conventional building of to-day, which is but an expansion of the monkish cell and the cloister. Instead of being a place fitted only for the rearing of what I have elsewhere termed desk-ridden emasculates, the school will be for the most part modelled on the workshop, giving to this term the most varied meaning possible, and a great part of the time will be spent at the work bench, tool in hand. Nature's workshop will, of course, be constantly utilised, and the necessary provision will be made for outdoor exercise and physical training. Scientific method will underlie the whole of education.

It will be recognised that education has two sides, a literary and a practical: that the mind can work through fingers; in fact, through all the senses; that it is not embodied only in the so-called intellect, a narrow creation of the schools. The practical training will therefore be regarded as at least equal in importance to the literary. Heads of schools will not only be potential bishops, but almost all careers will be open to them. In fact, I trust the system will be in operation which I have already advocated should be applied to the Education Department, and that the members of the school staff will be forced out into the world at stated intervals, so that they may not degenerate into pedants capable only of applying set rules much after the manner of that delightful creation Beckmesser in Wagner's opera 'Die Meistersinger.'

The class system will be largely abandoned. Children's school time will not be chopped up into regulated periods in a manner which finds no analogy in the

work-a-day world, but they will have certain tasks confided to them to do and will be allowed considerable latitude in carrying them to completion. In fact, they will be treated as rational beings, and their individuality and self-respect developed from the outset. The Boer War will have taught us to adopt open order teaching as well as open order firing. Schools will glory in turning out individuals, not machines. The success of the Americans is largely due to the way in which Republican doctrines are applied to the up-bringing of children in America. We must follow their example, and set our children free and encourage them to be free at an early age. The human animal develops at a sufficiently slow rate in all conscience, and there is little need for man to retard his own development. School, with its checks upon freedom and individuality, should be quitted at seventeen at latest, I believe, and all subsequent systematic training should take place at college. Boys are kept at school after seventeen mainly for the purposes of the school. It is claimed that by remaining they gain most valuable experience by acting as monitors and prefects; but this experience is enjoyed only by the few, and might be obtained at an earlier age. Then it is said that seventeen is too early an age to enter Oxford or Cambridge, but this has only been the case since schools have retained boys to prepare them for examinations, and in order that they might assist in the management. I believe that the attempts which have been made in these latter days to do college work at schools and to establish engineering sides in order to find work for senior boys have had a most detrimental effect. It is said that the training given in technical schools is too far removed from practice; but how much more must this be true of technical work done under school conditions? The excessive devotion to literary methods favoured by schools and the older Universities tends to develop unpractical habits which unfit many to face the rough-and-tumble life of the world, and is productive of a disinclination for practical avocations. By leaving school at a properly early period this danger is somewhat lessened; moreover it is necessary in many walks of life that school should be left early in order that the school of practice may be entered sufficiently soon to secure the indispensable manual dexterity and habits. For a long time past we have been drifting away from the practical, and those who are acquainted with the work of the schools, especially the elementary schools, are aghast at the influence they are exercising in hindering the development of practical ability. We must in some way counteract this tendency. On the other hand we have to meet the views of those who very properly urge that it is cruel to withdraw children from school even at the age we do. The two views must in some way be reconciled. The only way will be to so improve the teaching in schools that school becomes a palace of delight and the continuation school a necessity. The habits formed at school should be such that study would never be intermitted on leaving school. At present school so nauseates the majority that on quitting it they have neither desire nor aptitude to study left in them: the work done in it is so impossible to translate into ordinary practice, so foreign to outside requirements.

The problem can only be solved by the scientific use of the imagination. The solution I would venture to offer is that an honest attempt be made to teach, not only the three R's, but also a fourth, Reasoning—the use of thought-power—and that a properly wide meaning be given to all the R's.

Of all powers that can be acquired at school, that of reading is of first importance. Let teachers read what Carlyle says in the 'Hero as Man of Letters,' correcting his exaggerations by reading into his words some of the lessons taught by experimental science. Reading is not taught in schools in these days; if it were, people would not waste their time on the rubbish which now figures as literature, and for which a rational substitute *must be found*. A well-read man is worshipped at the Universities, and is held up to all comers as a pattern. Why should not children be encouraged to be 'well read'? Let us admit this and sow books in their path. Thring, in giving utterance to his 'Practical Thoughts on Education after Thirty Years' Work,' speaks strongly on this point. 'Great interest will make up for want of time. Create great interest,' he says; and

these are noteworthy words. 'As soon as children can read throw away all lesson-books for a time. Let them read. Let them read aloud—really read, not tumble through the pages. Give them to read poetry, the lives of good men, narratives of noble deeds, historical stories and historical novels, books of travel, and all the fascinating literature of discovery and adventure. The person who has once learnt to read well is tempted to go on. And such books, selected by a carefully graduated scheme, would supply endless knowledge whilst kindling the mind, without any waste of time from drudgery and disgust. Geography, history, and power of speech are all comprised in such books if properly used.'

Thring here advocates what I would advocate—the *incidental* method of teaching. Why should there be any set lesson in subjects such as history and geography? Nothing is worse, more stereotyped, more cramping to the intellect, than the set lesson of so many lines or pages, of a sort of Liebig's Essence of information, with the attendant obligation of committing the facts recorded in them to memory. The child, like the restive, high-mettled young steed, wants to be off and away—not to be held severely in hand. Why should not the method by which we get up a subject in later life be followed in schools? At least it should be properly tried. Let us give freedom to children, and at least during early years lead them to read hard and wisely; they will do so gladly; and give them pictures innumerable in illustration of their reading. And children must not only be taught to read books; they must learn also to regard and use them as sources of information; the habit of flying for information to books must be cultivated. They must be constantly referred to dictionaries and works of reference generally; they must be set to hunt up all sorts of stories. Of course the scholastic Beckmesser will object that such a system is impossible, that there would be an end to all discipline; but to say this is to show a want of understanding of children and of faith in them, and is proof of failure to recognise their power of accepting responsibility when it is properly put upon them. The secret of success lies in beginning sufficiently early; once let them appreciate what they are doing and the majority will work eagerly and spontaneously.

But when the full meaning is given to the first of the R's, it will be held to cover not only the reading of printed or written character, but also the reading of some of Nature's signs, to the end that sermons *may* be discovered in stones and good in everything. That is to say, at the same time that they are acquiring the true art of reading, they must be learning the true art of experimenting—to find out things by putting questions of their own and obtaining direct answers. The teaching of the elements of experimental science must therefore accompany the teaching of reading. And great care must be exercised that the palate for experimenting, for results, is not spoiled by reading. The use of text-books must be most carefully avoided at this stage in order that that which should be elicited by experiment is not previously known and merely demonstrated—a most inferior method from any true educational point of view, and of little value as a means of developing thought-power. I regard Huxley's physiography, for example, as a type of the book to be avoided until method has been fully mastered. The great difficulty in the way of teaching the art of reading arises from the comparative paucity of readable books for young people. Text-books are not readable, and in fact tend to spoil reading; and the majority of books are written for grown-up people having considerable experience of the world. The mistake is too commonly made of expecting children to master 'classics.' On the other hand, we need not fear allowing advanced books to fall into the hands of children; they are the first to despise the namby pamby stuff that is too frequently offered to them. A new literature must be created, if education is to be put on a sound basis; something beyond mere word painting is required. Books are wanted, written in a bright, attractive, and simple style, full of accurate information, which would carry us over the world and give clear pictures of all that is to be seen, as well as of the character and customs of its inhabitants; and books are wanted which, in like manner, would carry us back in time and sketch the history of the peoples of the earth. The various branches of science all need their popular exponents; our

books are for the most part too technical, and whilst much has been done to advocate the introduction of 'science' into general education, little has been done to make this possible. Unfortunately those who attempt to write readable books are too frequently not those who are possessed of sound knowledge, and it is time that it were realised by those who could write well and accurately that there is a duty incumbent upon them; on the other hand, something should be done to stem the torrent of text-books which is now flooding the field of education with the destroying force of a deluge, and making proper reading impossible.

The true use of books has yet to be found and admitted; we do not sufficiently recognise their value as stores of information and savers of brain waste. Why should long trains of facts be committed to memory but to be forgotten? It is impossible to believe that such a process is mental training; it must involve loss of energy and mental degradation. In future we must give the training at less cost, and teach the art of going to books for minute details whenever they are wanted. Nearly every subject is taught in an eminently selfish manner at the present time, the expert declaring that the learner must become acquainted with all the main facts of the subject, instead of recognising that it is far more important to acquire knowledge of first principles together with the power of acquiring the knowledge of facts whenever these become necessary.

The second R may be held to cover not only mere writing, but also composition. Why is the art of composition taught so badly? Because it is impossible even for children to make bricks without straw: they have little to write about under ordinary school conditions. The subject is also one, I believe, which must be taught incidentally, at least during the earlier years, and chiefly in connection with the experimental work; in fact, to make this last the training it should be an absolute record of all that is done must be properly written out, and while the work is being done too. Many teachers, I know, shy at this, and say that it is their business to teach 'Science,' and not literary style; but they are wrong, and must inevitably accept the burden if they are to succeed in teaching 'Science' at all. An experiment, like an act, 'hath three branches'—to conceive, to do, to utilise: a clearly defined motive must underlie it; it must be properly executed; the result must be interpreted and applied. It is only when the motive is clearly written out that it is clearly understood—that the meaning or intention of the experiment is clearly grasped; and this is equally true of the result. Of course, it is necessary to proceed slowly and not to demand too much from beginners; but it is surprising how the power grows. Drawing, of course, must be included under the second R; but this also may with advantage be taught incidentally, and only receive individual attention at a later stage, when those who show aptitude in the incidental work have been selected out for higher instruction.

The third R must be held to cover, not merely the simple rules of arithmetic, and all that is necessary of formal mathematics, but also measurement work. Mathematics claims to be an exact subject, and therefore must be treated exactly, and made the means of inculcating training in exactness, and not on paper merely, but in fact. Moreover, physical science reposes on a basis of exact measurement, so that the introduction of experimental work into schools involves the introduction of measurement work as a matter of course.

The fourth R—Reasoning—will necessarily be taught in connection with every subject of instruction, not specifically. It is introduced as marking the absolute need of developing thought-power; and, in point of fact, should be put before all others in importance.

Under such a system as I suggest the time of study would be spent in two ways—in reading and experimenting. But whatever we do let us be thorough; the danger lies in attempting too much, too many things. Each step must be taken slowly and warily, and a secure position established before going further.

Ireland is fortunate at the present time in that far-reaching changes are being introduced into its educational system. A body of men are engaged in this work who are, I believe, in every way specially qualified to promote reforms and

earnestly desirous of developing a sound policy. The Irish race have rich powers of imagination such as no other section of the nation possesses, and it is only necessary that these powers be trained to considered and balanced action to make the Irish capable of deeds before which the splendid achievements of the past will appear as nothing. Of course the development of a true policy must come about slowly, and we must not be too impatient of results, but give every encouragement and all possible support to those engaged in the work. It is before all things necessary to remember that the school is a preparation for life, not for the inspector's visit; in the future the inspector will act more as adviser and friend, let us hope, than as mentor.

Turning to my own subject, the programmes laid down for primary and intermediate schools appear to me to be well thought out and full of promise, the only fault that I might be inclined to find being that perhaps they are somewhat too ambitious. But very able men are directing the work, and they should be able to see that thoroughness is aimed at before all things. Nothing could be more gratifying than Mr. Heller's statement in the Report for 1900, 'that the Irish teachers as a whole seem to possess a great natural taste and aptitude for science and the method of experimental inquiry.' May they seek to set the example which is sorely needed to teachers in other parts of the Kingdom. I fear there has been a good deal of hand-to-mouth teaching in the past; to avoid this, the teacher should not only have a carefully drawn-up scheme of work, but should keep a diary in which the work accomplished each week is carefully recorded. In this way the weaker teachers will check any tendency they may have to relax their efforts, and inspectors will be in the position to understand at once what progress is being made. Education, unfortunately, is subject to booms as the money market is; just now the 'Nature study' boom is on. We must be very careful not to let this carry us away; whatever is done must be by way of real Nature study, and must have very simple beginnings. In most of the work that is being boomed the presence of the eternal book is only too evident, and such teaching must be worthless. Let the teachers remember that the great object in view is to acquire the art of experimenting and observing with a clearly defined and logical purpose. If they once learn to experiment properly all else will follow. The inspectors must give constructive help to the work; they too must be students and labourers in the cause of progress, not mere commentators. And there will be a great opportunity for experts to assist who can be helpful to schools. Every school should be provided with a workshop, simply equipped with flat-topped tables, in which all the subjects which are taught practically can be taken. Elaborately fitted laboratories are not only unnecessary but undesirable; the work should be done under conditions such as obtain in ordinary life. A due proportion of the school time must be devoted to experimental studies: no difficulty will arise when it is seen that so much else is taught incidentally; and that this is the case must be carefully borne in mind in arranging the curriculum—otherwise there will be much overlapping and waste of time. Lastly, every effort must be made to keep down the size of the classes. I trust that in Ireland the girls will receive as much attention as the boys. Experimental teaching is of even greater value to them than to boys, as boys have more opportunities of doing work which is akin to it in the world. The work done by girls should of course bear directly on their domestic occupations.

If we are to improve our schools the teachers must be trained to teach properly—or rather, let me say, must be put in the right way to teach, because practice and experience alone can give proficiency. This is the most difficult of all the problems to be faced in providing for the future. It is the one of all others to be thought out with the greatest care, and in solving it the help of all who can help must be secured. No amount of didactic teaching will make teachers; the training must be practical. To graft on the ordinary training a course of lectures on the theory and practice of teaching plus a certain amount of practice in a school is not enough. How can we attempt to teach the theory and practice of teaching when we are agreed that we do not know how to teach most subjects?

How can a master of method instruct us how to teach subjects of which he has only heard? It cannot be done; in point of fact, we are talking about the thing—beating about the bush—instead of treating the problem as one which can only be solved by experiment. To teach method, you must know your subject; one man cannot know many subjects. Of course there are quite a number of good general rules to be learnt, but the application of these must rest with the specialist; and the only proper way of giving training in method is to teach the subject in the way it seems desirable that it should be taught. The end result of training should be the development of a spirit of absolute humility—of the feeling that no task is so difficult as that of teaching properly, no career in which finality is more impossible to attain to, no career which offers greater opportunity for perpetual self-improvement. The effect of the narrow and unimaginative system in vogue to-day is to send forth a set of young persons who arrogantly consider that they are 'trained'; if they would only think of the amount of preparation involved in training for athletic competitions, or in training race-horses even, they would entertain more modest views and be aware that they have everything to learn when they commence their work. The Beckmessers reign supreme in our training colleges of to-day: they must be got rid of, and true modest experts introduced in their place. The test of efficiency must be a real one, not that of a mere final examination. The inspectors must see to it that the instruction is given always with a view to the fact that the students are to become teachers, which at present seems to be the last consideration borne in mind. Every effort must be made to secure a higher class of student for the training colleges; a fair secondary training *must be insisted on*. A narrow spirit of trades unionism pervades the primary school system at the present time, and School Boards and managers of Pupil Teachers' Centres make no effort to secure the assistance of secondary teachers.

My receipt for a training college would be: Develop thought-power and individuality; develop imagination. Teach whatever will do this most effectively, and let special subjects be studied in the way that may best be followed in teaching them subsequently.

It is to the lasting shame of our State organisation and of our School Boards that so little has been done to provide competent teachers.

The future rests with the Universities; but to save the nation the Universities must be practical, and broader conceptions must prevail in them. A course of training which will give true culture must be insisted on. The Universities have recently shown a disposition—to use a vulgarism—to throw themselves at the heads of the military authorities, and to make special provision for the training of military students. It is much more their office to train teachers. Why should not the example to hand in the engineering school at Cambridge be followed? Why should not a special Tripos be established for teachers in training? I believe this to be the true solution of the problem.

The desire now manifest in several of our large towns to establish new Universities comes most opportunely, and should receive every possible encouragement from all who have the interests of our country at heart. I believe the objections to be altogether fanciful and the outcome of academic views. It is said that the value of the degree will go down like that of Consols. But in what does the value of a degree consist? Simply and solely in the evidence it affords of training. We regard the Oxford and Cambridge degrees as of value because they are proof that their possessors have lived for some time under certain conditions which are recognised to be productive of good. The degrees of other Universities must soon come to be regarded as proof of sound and healthy training. It must become impossible to obtain degrees such as the University of London has been in the habit of awarding, which have been the result of mere garret-study; proof of training will be required of all candidates for degrees.

But I must now bring this Address to a conclusion. The only apology that I can offer for its length is that having had over thirty years' experience as a teacher,

and being profoundly impressed by the serious character of the outlook, the opportunity being given me, I felt that, as the walrus said to the carpenter,

'The time has come, . . .
To talk of many things :
Of shoes, and ships, and sealing-wax,
Of cabbages, and kings,
And why the sea is boiling hot,
And whether pigs have wings.'

(*'Alice through the Looking-glass.'*)

This list of subjects is no more varied and disconnected—the problems set no deeper—than those to which we must give our attention in dealing with education; and the sooner the fate of the oysters is that of our present educational 'system' the better. Having shown by this quotation that I am not an absolute modern, but have some knowledge of the classics, let me finally say, in the words of another poet—of him who on various occasions gave utterance to much wisdom at the breakfast table, that 'I don't want you to believe anything I say, I only want you to try to see what makes me believe it.'

Something more than an apology for an Education Act such as the powers are now engaged in shaping for us must be framed at no distant date, and a determinate policy arrived at. That policy may perhaps be found in the words put into Hamlets mouth:—

Hamlet. To what base uses we may return, Horatio! Why may not imagination trace the noble dust of Alexander, till he find it stopping a bung-hole?

Horatio. 'Twere to consider too curiously, to consider so.

Hamlet. No, faith, not a jot; but to follow him thither with modesty enough and likelihood to lead it, as thus: Alexander died, Alexander was buried, Alexander returneth into dust; the dust is earth; of earth we make loam; and why of that loam, whereto he was converted, might they not stop a beer barrel?

Imperious Cæsar, dead and turned to clay,
Might stop a hole to keep the wind away;
O, that that earth, which kept the world in awe,
Should patch a wall to expel the winter's flaw!

Shakespeare thus taught the use of the imagination before Tyndall! The fact that we can now carry our imagination far further afield and contemplate the survival of atoms once embodied in imperious Cæsar in the flowers and fruit which deck the fair face of Nature—a higher end than that Hamlet paints—may serve to justify the adoption of a method he advocated. Modern progress is based on research—the application of imagination. Surely then there is every reason to make the spirit of research the dominant force in education!

British Association for the Advancement of S

WINNIPEG, 1909.

ADDRESS

BY

PROFESSOR SIR J. J. THOMSON, M.A., LL.D.,
D.Sc., F.R.S.,

PRESIDENT.

TWENTY-FIVE years ago a great change was made in the practice of the British Association. From the foundation of our Society until 1884 its meetings had always been held in the British Isles; in that year, however, the Association met in Montreal, and a step was taken which changed us from an Insular into an Imperial Association. For this change, which now I think meets with nothing but approval, Canada is mainly responsible. Men of science welcome it for the increased opportunities it gives them of studying under the most pleasant and favourable conditions different parts of our Empire, of making new friends; such meetings as these not only promote the progress of science, but also help to strengthen the bonds which bind together the different portions of the King's Dominions.

This year, for the third time in a quarter of a century, we are meeting in Canada. As if to give us an object lesson in the growth of Empire, you in Winnipeg took the opportunity at our first meeting in Canada in 1884 to invite our members to visit Manitoba and see for themselves the development of the Province at that time. Those who were fortunate enough to be your guests then as well as now are confronted with a change which must seem to them unexampled and almost incredible. Great cities have sprung up, immense areas have

been converted from prairies to prosperous farms, flourishing industries have been started, and the population has quadrupled. As the President of a scientific association I hope I may be pardoned if I point out that even the enterprise and energy of your people and the richness of your country would have been powerless to effect this change without the resources placed at their disposal by the labours of men of science.

The eminence of my predecessors in the chair at the meetings of the British Association in Canada makes my task this evening a difficult one. The meeting at Montreal was presided over by Lord Rayleigh, who, like Lord Kelvin, his colleague in the chair of Section A at that meeting, has left the lion's mark on every department of physics, and who has shown that, vast as is the empire of physics, there are still men who can extend its frontiers in all of the many regions under its sway. It has been my lot to succeed Lord Rayleigh in other offices as well as this, and I know how difficult a man he is to follow.

The President of the second meeting in Canada—that held in 1897 at Toronto—was Sir John Evans, one of those men who, like Boyle, Cavendish, Darwin, and Huggins, have, from their own resources and without the aid derived from official positions or from the universities, made memorable contributions to science: such men form one of the characteristic features of British science. May we not hope that, as the knowledge of science and the interest taken in it increase, more of the large number of men of independent means in our country may be found working for the advancement of science, and thereby rendering services to the community no less valuable than the political, philanthropic, and social work at which many of them labour with so much zeal and success?

I can, however, claim to have some experience of, at any rate, one branch of Canadian science, for it has been my privilege to receive at the Cavendish Laboratory many students from your universities. Some of these have been holders of what are known as the 1851 scholarships. These scholarships are provided from the surplus of the Great Exhibition of 1851, and are placed at the disposal of most of the younger universities in the British Empire, to enable students to devote themselves for two or three years to original research in various branches of science. I have had many opportunities of seeing the work of these scholars, and I should like to put on record my opinion that there is no educational endowment in the country which has done or is doing better work.

I have had, as I said, the privilege of having as pupils students from your universities as well as from those of New Zealand, Australia, and the United States, and have thus had opportunities of comparing the effect on the best men of the educational system in force at your

universities with that which prevails in the older English universities. Well, as the result, I have come to the conclusion that there is a good deal in the latter system which you have been wise not to imitate. The chief evil from which we at Cambridge suffer and which you have avoided is, I am convinced, the excessive competition for scholarships which confronts our students at almost every stage of their education. You may form some estimate of the prevalence of these scholarships if I tell you that the colleges in the University of Cambridge alone give more than 35,000*l.* a year in scholarships to undergraduates, and I suppose the case is much the same at Oxford. The result of this is that preparation for these scholarships dominates the education of the great majority of the cleverer boys who come to these universities, and indeed in some quarters it seems to be held that the chief duty of a schoolmaster, and the best test of his efficiency, is to make his boys get scholarships. The preparation for the scholarship too often means that about two years before the examination the boy begins to specialise, and from the age of sixteen does little else than the subject, be it mathematics, classics, or natural science, for which he wishes to get a scholarship; then, on entering the university, he spends three or four years studying the same subject before he takes his degree, when his real life work ought to begin. How has this training fitted him for this work? I will take the case in which the system might perhaps be expected to show to greatest advantage, when his work is to be original research in the subject he has been studying. He has certainly acquired a very minute acquaintance with his subject—indeed, the knowledge possessed by some of the students trained under this system is quite remarkable, much greater than that of any other students I have ever met. But though he has acquired knowledge, the effect of studying one subject, and one subject only, for so long a time is too often to dull his enthusiasm for it, and he begins research with much of his early interest and keenness evaporated. Now there is hardly any quality more essential to success in research than enthusiasm. Research is difficult, laborious, often disheartening. The carefully designed apparatus refuses to work, it develops defects which may take months of patient work to rectify, the results obtained may appear inconsistent with each other and with every known law of Nature, sleepless nights and laborious days may seem only to make the confusion more confounded, and there is nothing for the student to do but to take for his motto 'It's dogged as does it,' and plod on, comforting himself with the assurance that when success does come, the difficulties he has overcome will increase the pleasure—one of the most exquisite men can enjoy—of getting some conception which will make all that was tangled, confused, and contradictory clear and consistent. Unless he has enthusiasm to carry him on when the prospect seems almost

hopeless and the labour and strain incessant, the student may give up his task and take to easier, though less important, pursuits.

I am convinced that no greater evil can be done to a young man than to dull his enthusiasm. In a very considerable experience of students of physics beginning research, I have met with more—many more—failures from lack of enthusiasm and determination than from any lack of knowledge or of what is usually known as cleverness.

This continual harping from an early age on one subject, which is so efficient in quenching enthusiasm, is much encouraged by the practice of the colleges to give scholarships for proficiency in one subject alone. I went through a list of the scholarships awarded in the University of Cambridge last winter, and, though there were 202 of them, I could only find three cases in which it was specified that the award was made for proficiency in more than one subject.

The premature specialisation fostered by the preparation for these scholarships, injures the student by depriving him of adequate literary culture, while when it extends, as it often does, to specialisation in one or two branches of science, it retards the progress of science by tending to isolate one science from another. The boundaries between the sciences are arbitrary, and tend to disappear as science progresses. The principles of one science often find most striking and suggestive illustrations in the phenomena of another. Thus, for example, the physicist finds in astronomy that effects he has observed in the laboratory are illustrated on the grand scale in the sun and stars. No better illustration of this could be given than Professor Hale's recent discovery of the Zeeman effect in the light from sunspots; in chemistry, too, the physicist finds in the behaviour of whole series of reactions illustrations of the great laws of thermodynamics, while if he turns to the biological sciences he is confronted by problems, mostly unsolved, of unsurpassed interest. Consider for a moment the problem presented by almost any plant—the characteristic and often exquisite detail of flower, leaf, and habit—and remember that the mechanism which controls this almost infinite complexity was once contained in a seed perhaps hardly large enough to be visible. We have here one of the most entrancing problems in chemistry and physics it is possible to conceive.

Again the specialisation prevalent in schools often prevents students of science from acquiring sufficient knowledge of mathematics; it is true that most of those who study physics do some mathematics, but I hold that, in general, they do not do enough, and that they are not as efficient physicists as they would be if they had a wider knowledge of that subject. There seems at present a tendency in some quarters to discourage the use of mathematics in physics; indeed, one might infer, from the statements of some writers in quasi-scientific journals,

that ignorance of mathematics is almost a virtue. If this is so, then surely of all the virtues this is the easiest and most prevalent.

I do not for a moment urge that the physicist should confine himself to looking at his problems from the mathematical point of view; on the contrary, I think a famous French mathematician and physicist was guilty of only slight exaggeration when he said that no discovery was really important or properly understood by its author unless and until he could explain it to the first man he met in the street.

But two points of view are better than one, and the physicist who is also a mathematician possesses a most powerful instrument for scientific research with which many of the greatest discoveries have been made; for example, electric waves were discovered by mathematics long before they were detected in the laboratory. He has also at his command a language clear, concise, and universal, and there is no better way of detecting ambiguities and discrepancies in his ideas than by trying to express them in this language. Again, it often happens that we are not able to appreciate the full significance of some physical discovery until we have subjected it to mathematical treatment, when we find that the effect we have discovered involves other effects which have not been detected, and we are able by this means to duplicate the discovery. Thus James Thomson, starting from the fact that ice floats on water, showed that it follows by mathematics that ice can be melted and water prevented from freezing by pressure. This effect, which was at that time unknown, was afterwards verified by his brother, Lord Kelvin. Multitudes of similar duplication of physical discoveries by mathematics could be quoted.

I have been pleading in the interests of physics for a greater study of mathematics by physicists. I would also plead for a greater study of physics by mathematicians in the interest of pure mathematics.

The history of pure mathematics shows that many of the most important branches of the subject have arisen from the attempts made to get a mathematical solution of a problem suggested by physics. Thus the differential calculus arose from attempts to deal with the problem of moving bodies. Fourier's theorem resulted from attempts to deal with the vibrations of strings and the conduction of heat; indeed, it would seem that the most fruitful crop of scientific ideas is produced by cross-fertilisation between the mind and some definite fact, and that the mind by itself is comparatively unproductive.

I think, if we could trace the origin of some of our most comprehensive and important scientific ideas, it would be found that they arose in the attempt to find an explanation of some apparently trivial and very special phenomenon; when once started the ideas grew to such generality and importance that their modest origin could hardly be suspected. Water vapour we know will refuse to condense into rain

unless there are particles of dust to form nuclei; so an idea before taking shape seems to require a nucleus of solid fact round which it can condense.

I have ventured to urge the closer union between mathematics and physics, because I think of late years there has been some tendency for these sciences to drift apart, and that the workers in applied mathematics are relatively fewer than they were some years ago. This is no doubt due to some extent to the remarkable developments made in the last few years in experimental physics on the one hand and in the most abstract and metaphysical parts of pure mathematics on the other. The fascination of these has drawn workers to the frontiers of these regions who would otherwise have worked nearer the junction of the two. In part, too, it may be due to the fact that the problems with which the applied mathematician has to deal are exceedingly difficult, and many may have felt that the problems presented by the older physics have been worked over so often by men of the highest genius that there was but little chance of any problem which they could have any hope of solving being left.

But the newer developments of physics have opened virgin ground which has not yet been worked over and which offers problems to the mathematician of great interest and novelty—problems which will suggest and require new methods of attack, the development of which will advance pure mathematics as well as physics.

I have alluded to the fact that pure mathematicians have been indebted to the study of concrete problems for the origination of some of their most valuable conceptions; but though no doubt pure mathematicians are in many ways very exceptional folk, yet in this respect they are very human. Most of us need to tackle some definite difficulty before our minds develop whatever powers they may possess. This is true for even the youngest of us, for our school boys and school girls, and I think the moral to be drawn from it is that we should aim at making the education in our schools as little bookish and as practical and concrete as possible.

I once had an illustration of the power of the concrete in stimulating the mind which made a very lasting impression upon me. One of my first pupils came to me with the assurance from his previous teacher that he knew little and cared less about mathematics, and that he had no chance of obtaining a degree in that subject. For some time I thought this estimate was correct, but he happened to be enthusiastic about billiards, and when we were reading that part of mechanics which deals with the collision of elastic bodies I pointed out that many of the effects he was constantly observing were illustrations of the subject we were studying. From that time he was a changed man. He had never before regarded mathematics as anything but a means of annoying

innocent undergraduates; now, when he saw what important results it could obtain, he became enthusiastic about it, developed very considerable mathematical ability, and, though he had already wasted two out of his three years at college, took a good place in the Mathematical Tripos.

It is possible to read books, to pass examinations without the higher qualities of the mind being called into play. Indeed, I doubt if there is any process in which the mind is more quiescent than in reading without interest. I might appeal to the widespread habit of reading in bed as a prevention of insomnia as a proof of this. But it is not possible for a boy to make a boat or for a girl to cook a dinner without using their brains. With practical things the difficulties have to be surmounted, the boat must be made watertight, the dinner must be cooked, while in reading there is always the hope that the difficulties which have been slurred over will not be set in the examination.

I think it was Helmholtz who said that often in the course of a research more thought and energy were spent in reducing a refractory piece of brass to order than in devising the method or planning the scheme of campaign. This constant need for thought and action gives to original research in any branch of experimental science great educational value even for those who will not become professional men of science. I have had considerable experience with students beginning research in experimental physics, and I have always been struck by the quite remarkable improvement in judgment, independence of thought and maturity produced by a year's research. Research develops qualities which are apt to atrophy when the student is preparing for examinations, and, quite apart from the addition of new knowledge to our store, is of the greatest importance as a means of education.

It is the practice in many universities to make special provision for the reception of students from other universities who wish to do original research or to study the more advanced parts of their subject, and considerable numbers of such students migrate from one university to another. I think it would be a good thing if this practice were to extend to students at an earlier stage in their career; especially should I like to see a considerable interchange of students between the universities in the Mother Country and those in the Colonies.

I am quite sure that many of our English students, especially those destined for public life, could have no more valuable experience than to spend a year in one or other of your universities, and I hope some of your students might profit by a visit to ours.

I can think of nothing more likely to lead to a better understanding of the feelings, the sympathies, and, what is not less important, the prejudices, of one country by another, than by the youths of those countries spending a part of their student life together.

Undergraduates as a rule do not wear a mask either of politeness or any other material, and have probably a better knowledge of each other's opinions and points of view—in fact, know each other better than do people of riper age. To bring this communion of students about there must be co-operation between the universities throughout the Empire; there must be recognition of each other's examinations, residence, and degrees. Before this can be accomplished there must, as my friend Mr. E. B. Sargent pointed out in a lecture given at the McGill University, be co-operation and recognition between the universities in each part of the Empire. I do not mean for a moment that all universities in a country should be under one government. I am a strong believer in the individuality of universities, but I do not think this is in any way inconsistent with the policy of an open door from one university to every other in the Empire.

It has usually been the practice of the President of this Association to give some account of the progress made in the last few years in the branch of science which he has the honour to represent.

I propose this evening to follow that precedent and to attempt to give a very short account of some of the more recent developments of physics, and the new conceptions of physical processes to which they have led.

The period which has elapsed since the Association last met in Canada has been one of almost unparalleled activity in many branches of physics, and many new and unsuspected properties of matter and electricity have been discovered. The history of this period affords a remarkable illustration of the effect which may be produced by a single discovery; for it is, I think, to the discovery of the Röntgen rays that we owe the rapidity of the progress which has recently been made in physics. A striking discovery like that of the Röntgen rays acts much like the discovery of gold in a sparsely populated country; it attracts workers who come in the first place for the gold, but who may find that the country has other products, other charms, perhaps even more valuable than the gold itself. The country in which the gold was discovered in the case of the Röntgen rays was the department of physics dealing with the discharge of electricity through gases, a subject which, almost from the beginning of electrical science, had attracted a few enthusiastic workers, who felt convinced that the key to unlock the secret of electricity was to be found in a vacuum tube. Röntgen, in 1895, showed that when electricity passed through such a tube, the tube emitted rays which could pass through bodies opaque to ordinary light; which could, for example, pass through the flesh of the body and throw a shadow of the bones on a suitable screen. The fascination of this discovery attracted many workers to the subject of the discharge of electricity through gases, and led to great improvements in the instruments used in this type of research. It is not, however, to

the power of probing dark places, important though this is, that the influence of Röntgen rays on the progress of science has mainly been due; it is rather because these rays make gases, and, indeed, solids and liquids, through which they pass conductors of electricity. It is true that before the discovery of these rays other methods of making gases conductors were known, but none of these was so convenient for the purposes of accurate measurement.

The study of gases exposed to Röntgen rays has revealed in such gases the presence of particles charged with electricity; some of these particles are charged with positive, others with negative electricity.

The properties of these particles have been investigated; we know the charge they carry, the speed with which they move under an electric force, the rate at which the oppositely charged ones recombine, and these investigations have thrown a new light, not only on electricity, but also on the structure of matter.

We know from these investigations that electricity, like matter, is molecular in structure, that just as a quantity of hydrogen is a collection of an immense number of small particles called molecules, so a charge of electricity is made up of a great number of small charges, each of a perfectly definite and known amount.

Helmholtz said in 1880 that in his opinion the evidence in favour of the molecular constitution of electricity was even stronger than that in favour of the molecular constitution of matter. How much stronger is that evidence now, when we have measured the charge on the unit and found it to be the same from whatever source the electricity is obtained. Nay, further, the molecular theory of matter is indebted to the molecular theory of electricity for the most accurate determination of its fundamental quantity, the number of molecules in any given quantity of an elementary substance.

The great advantage of the electrical methods for the study of the properties of matter is due to the fact that whenever a particle is electrified it is very easily identified, whereas an uncharged molecule is most elusive; and it is only when these are present in immense numbers that we are able to detect them. A very simple calculation will illustrate the difference in our power of detecting electrified and unelectrified molecules. The smallest quantity of unelectrified matter ever detected is probably that of neon, one of the inert gases of the atmosphere. Professor Strutt has shown that the amount of neon in $\frac{1}{20}$ of a cubic centimetre of the air at ordinary pressures can be detected by the spectroscope; Sir William Ramsay estimates that the neon in the air only amounts to one part of neon in 100,000 parts of air, so that the neon in $\frac{1}{20}$ of a cubic centimetre of air would only occupy at atmospheric pressure a volume of half a millionth of a cubic centimetre. When stated in this form the quantity seems exceedingly small, but in this small volume there are about

ten million million molecules. Now the population of the earth is estimated at about fifteen hundred millions, so that the smallest number of molecules of neon we can identify is about 7,000 times the population of the earth. In other words, if we had no better test for the existence of a man than we have for that of an unelectrified molecule we should come to the conclusion that the earth is uninhabited. Contrast this with our power of detecting electrified molecules. We can by the electrical method, even better by the cloud method of C. T. R. Wilson, detect the presence of three or four charged particles in a cubic centimetre. Rutherford has shown that we can detect the presence of a single α particle. Now the particle is a charged atom of helium; if this atom had been uncharged we should have required more than a million million of them, instead of one, before we should have been able to detect them.

We may, I think, conclude, since electrified particles can be studied with so much greater ease than unelectrified ones, that we shall obtain a knowledge of the ultimate structure of electricity before we arrive at a corresponding degree of certainty with regard to the structure of matter.

We have already made considerable progress in the task of discovering what the structure of electricity is. We have known for some time that of one kind of electricity—the negative—and a very interesting one it is. We know that negative electricity is made up of units all of which are of the same kind; that these units are exceedingly small compared with even the smallest atom, for the mass of the unit is only $\frac{1}{1836}$ part of the mass of an atom of hydrogen; that its radius is only 10^{-13} centimetre, and that these units, 'corpuscles' as they have been called, can be obtained from all substances. The size of these corpuscles is on an altogether different scale from that of atoms; the volume of a corpuscle bears to that of the atom about the same relation as that of a speck of dust to the volume of this room. Under suitable conditions they move at enormous speeds which approach in some instances the velocity of light.

The discovery of these corpuscles is an interesting example of the way Nature responds to the demands made upon her by mathematicians. Some years before the discovery of corpuscles it had been shown by a mathematical investigation that the mass of a body must be increased by a charge of electricity. This increase, however, is greater for small bodies than for large ones, and even bodies as small as atoms are hopelessly too large to show any appreciable effect; thus the result seemed entirely academic. After a time corpuscles were discovered, and these are so much smaller than the atom that the increase in mass due to the charge becomes not merely appreciable, but so great that, as the experiments of Kaufmann and Bucherer have shown, the whole of the mass of the corpuscle arises from its charge.

We know a great deal about negative electricity; what do we know about positive electricity? Is positive electricity molecular in structure? Is it made up into units, each unit carrying a charge equal in magnitude though opposite in sign to that carried by a corpuscle? Does, or does not, this unit differ, in size and physical properties, very widely from the corpuscle? We know that by suitable processes we can get corpuscles out of any kind of matter, and that the corpuscles will be the same from whatever source they may be derived. Is a similar thing true for positive electricity? Can we get, for example, a positive unit from oxygen of the same kind as that we get from hydrogen?

For my own part, I think the evidence is in favour of the view that we can, although the nature of the unit of positive electricity makes the proof much more difficult than for the negative unit.

In the first place we find that the positive particles—‘canalstrahlen’ is their technical name—discovered by our distinguished guest, Dr. Goldstein, which are found when an electric discharge passes through a highly rarefied gas, are, when the pressure is very low, the same, whatever may have been the gas in the vessel to begin with. If we pump out the gas until the pressure is too low to allow the discharge to pass, and then introduce a small quantity of gas and restart the discharge, the positive particles are the same whatever kind of gas may have been introduced.

I have, for example, put into the exhausted vessel oxygen, argon, helium, the vapour of carbon tetrachloride, none of which contain hydrogen, and found the positive particles to be the same as when hydrogen was introduced.

Some experiments made lately by Wellisch, in the Cavendish Laboratory, strongly support the view that there is a definite unit of positive electricity independent of the gas from which it is derived; these experiments were on the velocity with which positive particles move through mixed gases. If we have a mixture of methyl-iodide and hydrogen exposed to Röntgen rays, the effect of the rays on the methyl-iodide is so much greater than on the hydrogen that, even when the mixture contains only a small percentage of methyl-iodide, practically all the electricity comes from this gas, and not from the hydrogen.

Now if the positive particles were merely the residue left when a corpuscle had been abstracted from the methyl-iodide, these particles would have the dimensions of a molecule of methyl-iodide; this is very large and heavy, and would therefore move more slowly through the hydrogen molecules than the positive particles derived from hydrogen itself, which would, on this view, be of the size and weight of the light hydrogen molecules. Wellisch found that the velocities of both the positive and negative particles through the mixture were the same as the velocities through pure hydrogen, although in the one case the ions had originated from methyl-iodide and in the other from hydrogen; a

similar result was obtained when carbon tetrachloride, or mercury methyl, was used instead of methyl-iodide. These and similar results lead to the conclusion that the atom of the different chemical elements contain definite units of positive as well as of negative electricity, and that the positive electricity, like the negative, is molecular in structure.

The investigations made on the unit of positive electricity show that it is of quite a different kind from the unit of negative, the mass of the negative unit is exceedingly small compared with any atom, the only positive units that up to the present have been detected are quite comparable in mass with the mass of an atom of hydrogen; in fact they seem equal to it. This makes it more difficult to be certain that the unit of positive electricity has been isolated, for we have to be on our guard against its being a much smaller body attached to the hydrogen atoms which happen to be present in the vessel. If the positive units have a much greater mass than the negative ones, they ought not to be so easily deflected by magnetic forces when moving at equal speeds; and in general the insensibility of the positive particles to the influence of a magnet is very marked; though there are cases when the positive particles are much more readily deflected, and these have been interpreted as proving the existence of positive units comparable in mass with the negative ones. I have found, however, that in these cases the positive particles are moving very slowly, and that the ease with which they are deflected is due to the smallness of the velocity and not to that of the mass. It should, however, be noted that M. Jean Becquerel has observed in the absorption spectra of some minerals, and Professor Wood in the rotation of the plane of polarisation by sodium vapour, effects which could be explained by the presence in the substances of positive units comparable in mass with corpuscles. This, however, is not the only explanation which can be given of these effects, and at present the smallest positive electrified particles of which we have direct experimental evidence have masses comparable with that of an atom of hydrogen.

A knowledge of the mass and size of the two units of electricity, the positive and the negative, would give us the material for constructing what may be called a molecular theory of electricity, and would be a starting-point for a theory of the structure of matter; for the most natural view to take, as a provisional hypothesis, is that matter is just a collection of positive and negative units of electricity, and that the forces which hold atoms and molecules together, the properties which differentiate one kind of matter from another, all have their origin in the electrical forces exerted by positive and negative units of electricity, grouped together in different ways in the atoms of the different elements.

As it would seem that the units of positive and negative electricity

are of very different sizes, we must regard matter as a mixture containing systems of very different types, one type corresponding to the small corpuscle, the other to the large positive unit.

Since the energy associated with a given charge is greater the smaller the body on which the charge is concentrated, the energy stored up in the negative corpuscles will be far greater than that stored up by the positive. The amount of energy which is stored up in ordinary matter in the form of the electrostatic potential energy of its corpuscles is, I think, not generally realised. All substances give out corpuscles, so that we may assume that each atom of a substance contains at least one corpuscle. From the size and the charge on the corpuscle, both of which are known, we find that each corpuscle has 8×10^{-7} ergs of energy; this is on the supposition that the usual expressions for the energy of a charged body hold when, as in the case of a corpuscle, the charge is reduced to one unit. Now in one gramme of hydrogen there are about 6×10^{23} atoms, so if there is only one corpuscle in each atom the energy due to the corpuscles in a gramme of hydrogen would be 48×10^{16} ergs, or 11×10^9 calories. This is more than seven times the heat developed by one gramme of radium, or than that developed by the burning of five tons of coal. Thus we see that even ordinary matter contains enormous stores of energy; this energy is fortunately kept fast bound by the corpuscles; if at any time an appreciable fraction were to get free the earth would explode and become a gaseous nebula.

The matter of which I have been speaking so far is the material which builds up the earth, the sun, and the stars, the matter studied by the chemist, and which we can represent by a formula; this matter occupies, however, but an insignificant fraction of the universe, it forms but minute islands in the great ocean of the ether, the substance within which the whole universe is filled.

The ether is not a fantastic creation of the speculative philosopher; it is as essential to us as the air we breathe. For we must remember that we on this earth are not living on our own resources; we are dependent from minute to minute upon what we are getting from the sun, and the gifts of the sun are conveyed to us by the ether. It is to the sun that we owe not merely night and day, springtime and harvest, but it is the energy of the sun, stored up in coal, in waterfalls, in food, that practically does all the work of the world.

How great is the supply the sun lavishes upon us becomes clear when we consider that the heat received by the earth under a high sun and a clear sky is equivalent, according to the measurements of Langley, to about 7,000 horse-power per acre. Though our engineers have not yet discovered how to utilise this enormous supply of power, they will, I have not the slightest doubt, ultimately succeed in doing so;

and when coal is exhausted and our water-power inadequate, it may be that this is the source from which we shall derive the energy necessary for the world's work. When that comes about, our centres of industrial activity may perhaps be transferred to the burning deserts of the Sahara, and the value of land determined by its suitability for the reception of traps to catch sunbeams.

This energy, in the interval between its departure from the sun and its arrival at the earth, must be in the space between them. Thus this space must contain something which, like ordinary matter, can store up energy, which can carry at an enormous pace the energy associated with light and heat, and which can, in addition, exert the enormous stresses necessary to keep the earth circling round the sun and the moon round the earth.

The study of this all-pervading substance is perhaps the most fascinating and important duty of the physicist.

On the electromagnetic theory of light, now universally accepted, the energy streaming to the earth travels through the ether in electric waves; thus practically the whole of the energy at our disposal has at one time or another been electrical energy. The ether must, then, be the seat of electrical and magnetic forces. We know, thanks to the genius of Clerk Maxwell, the founder and inspirer of modern electrical theory, the equations which express the relation between these forces, and although for some purposes these are all we require, yet they do not tell us very much about the nature of the ether.

The interest inspired by equations, too, in some minds is apt to be somewhat Platonic; and something more grossly mechanical—a model, for example, is felt by many to be more suggestive and manageable, and for them a more powerful instrument of research, than a purely analytical theory.

Is the ether dense or rare? Has it a structure? Is it at rest or in motion? are some of the questions which force themselves upon us.

Let us consider some of the facts known about the ether. When light falls on a body and is absorbed by it, the body is pushed forward in the direction in which the light is travelling, and if the body is free to move it is set in motion by the light. Now it is a fundamental principle of dynamics that when a body is set moving in a certain direction, or, to use the language of dynamics, acquires momentum in that direction, some other mass must lose the same amount of momentum; in other words, the amount of momentum in the universe is constant. Thus when the body is pushed forward by the light some other system must have lost the momentum the body acquires, and the only other system available is the wave of light falling on the body; hence we conclude that there must have been momentum in the wave in the direction in which it is travelling. Momentum, however, implies mass in motion. We con-

clude, then, that in the ether through which the wave is moving there is mass moving with the velocity of light. The experiments made on the pressure due to light enable us to calculate this mass, and we find that in a cubic kilometre of ether carrying light as intense as sunlight is at the surface of the earth, the mass moving is only about one-fifty-millionth of a milligramme. We must be careful not to confuse this with the mass of a cubic kilometre of ether; it is only the mass moved when the light passes through it; the vast majority of the ether is left undisturbed by the light. Now, on the electro-magnetic theory of light, a wave of light may be regarded as made up of groups of lines of electric force moving with the velocity of light; and if we take this point of view we can prove that the mass of ether per cubic centimetre carried along is proportional to the energy possessed by these lines of electric force per cubic centimetre, divided by the square of the velocity of light. But though lines of electric force carry some of the ether along with them as they move, the amount so carried, even in the strongest electric fields we can produce, is but a minute fraction of the ether in their neighbourhood.

This is proved by an experiment made by Sir Oliver Lodge in which light was made to travel through an electric field in rapid motion. If the electric field had carried the whole of the ether with it, the velocity of the light would have been increased by the velocity of the electric field. As a matter of fact no increase whatever could be detected, though it would have been registered if it had amounted to one-thousandth part of that of the field.

The ether carried along by a wave of light must be an exceedingly small part of the volume through which the wave is spread. Parts of this volume are in motion, but by far the greater part is at rest; thus in the wave front there cannot be uniformity, at some parts the ether is moving, at others it is at rest—in other words, the wave front must be more analogous to bright specks on a dark ground than to a uniformly illuminated surface.

The place where the density of the ether carried along by an electric field rises to its highest value is close to a corpuscle, for round the corpuscles are by far the strongest electric fields of which we have any knowledge. We know the mass of the corpuscle, we know from Kaufmann's experiments that this arises entirely from the electric charge, and is therefore due to the ether carried along with the corpuscle by the lines of force attached to it.

A simple calculation shows that one-half of this mass is contained in a volume seven times that of a corpuscle. Since we know the volume of the corpuscle as well as the mass, we can calculate the density of the ether attached to the corpuscle; doing so, we find it amounts to the prodigious value of about 5×10^{10} , or about 2,000 million times that

of lead. Sir Oliver Lodge, by somewhat different considerations, has arrived at a value of the same order of magnitude.

Thus around the corpuscle ether must have an extravagant density; whether the density is as great as this in other places depends upon whether the ether is compressible or not. If it is compressible, then it may be condensed round the corpuscles, and there have an abnormally great density; if it is not compressible, then the density in free space cannot be less than the number I have just mentioned.

With respect to this point we must remember that the forces acting on the ether close to the corpuscle are prodigious. If the ether were, for example, an ideal gas whose density increased in proportion to the pressure, however great the pressure might be, then if, when exposed to the pressures which exist in some directions close to the corpuscle, it had the density stated above, its density under atmospheric pressure would only be about 8×10^{-6} , or a cubic kilometre would have a mass less than a gramme; so that instead of being almost incomparably denser than lead, it would be almost incomparably rarer than the lightest gas.

I do not know at present of any effect which would enable us to determine whether ether is compressible or not. And although at first sight the idea that we are immersed in a medium almost infinitely denser than lead might seem inconceivable, it is not so if we remember that in all probability matter is composed mainly of holes. We may, in fact, regard matter as possessing a bird-cage kind of structure in which the volume of the ether disturbed by the wires when the structure is moved is infinitesimal in comparison with the volume enclosed by them. If we do this, no difficulty arises from the great density of the ether; all we have to do is to increase the distance between the wires in proportion as we increase the density of the ether.

Let us now consider how much ether is carried along by ordinary matter, and what effects this might be expected to produce.

The simplest electrical system we know, an electrified sphere, has attached to it a mass of ether proportional to its potential energy, and such that if the mass were to move with the velocity of light its kinetic energy would equal the electrostatic potential energy of the particle. This result can be extended to any electrified system, and it can be shown that such a system binds a mass of the ether proportional to its potential energy. Thus a part of the mass of any system is proportional to the potential energy of the system.

The question now arises, Does this part of the mass add anything to the weight of the body? If the ether were not subject to gravitational attraction it certainly would not; and even if the ether were ponderable, we might expect that as the mass is swimming in a sea of ether it would not increase the weight of the body to which it is attached. But if it does not, then a body with a large amount of potential energy may have an

appreciable amount of its mass in a form which does not increase its weight, and thus the weight of a given mass of it may be less than that of an equal mass of some substance with a smaller amount of potential energy. Thus the weights of equal masses of these substances would be different. Now, experiments with pendulums, as Newton pointed out, enable us to determine with great accuracy the weights of equal masses of different substances. Newton himself made experiments of this kind, and found that the weights of equal masses were the same for all the materials he tried. Bessel, in 1830, made some experiments on this subject which are still the most accurate we possess, and he showed that the weights of equal masses of lead, silver, iron, brass did not differ by as much as one part in 60,000.

The substances tried by Newton and Bessel did not, however, include any of those substances which possess the marvellous power of radio-activity; the discovery of these came much later, and is one of the most striking achievements of modern physics.

These radio-active substances are constantly giving out large quantities of heat, presumably at the expense of their potential energy; thus when these substances reach their final non-radio-active state their potential energy must be less than when they were radio-active. Professor Rutherford's measurements show that the energy emitted by one gramme of radium in the course of its degradation to non-radio-active forms is equal to the kinetic energy of a mass of 1-13th of a milligramme moving with the velocity of light.

This energy, according to the rule I have stated, corresponds to a mass of 1-13th of a milligramme of the ether, and thus a gramme of radium in its radio-active state must have at least 1-13th of a milligramme more of ether attached to it than when it has been degraded into the non-radio-active forms. Thus if this ether does not increase the weight of the radium, the ratio of mass to weight for radium would be greater by about one part in 13,000 than for its non-radio-active products.

I attempted several years ago to find the ratio of mass to weight for radium by swinging a little pendulum, the bob of which was made of radium. I had only a small quantity of radium, and was not, therefore, able to attain any great accuracy. I found that the difference, if any, in the ratio of the mass to weight between radium and other substances was not more than one part in 2,000. Lately we have been using at the Cavendish Laboratory a pendulum whose bob was filled with uranium oxide. We have got good reasons for supposing that uranium is a parent of radium, so that the great potential energy and large ethereal mass possessed by the radium will be also in the uranium; the experiments are not yet completed. It is, perhaps, expecting almost too much to hope that the radio-active substances may add to the great services they have

already done to science by furnishing the first case in which there is some differentiation in the action of gravity.

The mass of ether bound by any system is such that if it were to move with the velocity of light its kinetic energy would be equal to the potential energy of the system. This result suggests a new view of the nature of potential energy. Potential energy is usually regarded as essentially different from kinetic energy. Potential energy depends on the configuration of the system, and can be calculated from it when we have the requisite data; kinetic energy, on the other hand, depends upon the velocity of the system. According to the principle of the conservation of energy the one form can be converted into the other at a fixed rate of exchange, so that when one unit of one kind disappears a unit of the other simultaneously appears.

Now in many cases this rule is all that we require to calculate the behaviour of the system, and the conception of potential energy is of the utmost value in making the knowledge derived from experiment and observation available for mathematical calculation. It must, however, I think, be admitted that from the purely philosophical point of view it is open to serious objection. It violates, for example, the principle of continuity. When a thing changes from a state A to a different state B, the principle of continuity requires that it must pass through a number of states intermediate between A and B, so that the transition is made gradually, and not abruptly. Now, when kinetic energy changes into potential, although there is no discontinuity in the quantity of the energy, there is in its quality, for we do not recognise any kind of energy intermediate between that due to the motion and that due to the position of the system, and some portions of energy are supposed to change *per saltum* from the kinetic to the potential form. In the case of the transition of kinetic energy into heat energy in a gas, the discontinuity has disappeared with a fuller knowledge of what the heat energy in a gas is due to. When we were ignorant of the nature of this energy, the transition from kinetic into thermal energy seemed discontinuous; but now we know that this energy is the kinetic energy of the molecules of which the gas is composed, so that there is no change in the type of energy when the kinetic energy of visible motion is transformed into the thermal energy of a gas—it is just the transference of kinetic energy from one body to another.

If we regard potential energy as the kinetic energy of portions of the ether attached to the system, then all energy is kinetic energy, due to the motion of matter or of portions of ether attached to the matter. I showed, many years ago, in my 'Applications of Dynamics to Physics and Chemistry,' that we could imitate the effects of the potential energy of a system by means of the kinetic energy of invisible systems connected in an appropriate manner with the main system, and that

the potential energy of the visible universe may in reality be the kinetic energy of an invisible one connected up with it. We naturally suppose that this invisible universe is the luminiferous ether, that portions of the ether in rapid motion are connected with the visible systems, and that their kinetic energy is the potential energy of the systems.

We may thus regard the ether as a bank in which we may deposit energy and withdraw it at our convenience. The mass of the ether attached to the system will change as the potential energy changes, and thus the mass of a system whose potential energy is changing cannot be constant; the fluctuations in mass under ordinary conditions are, however, so small that they cannot be detected by any means at present at our disposal. Inasmuch as the various forms of potential energy are continually being changed into heat energy, which is the kinetic energy of the molecules of matter, there is a constant tendency for the mass of a system such as the earth or the sun to diminish, and thus as time goes on for the mass of ether gripped by the material universe to become smaller and smaller; the rate at which it would diminish would, however, get slower as time went on, and there is no reason to think that it would ever get below a very large value.

Radiation of light and heat from an incandescent body like the sun involves a constant loss of mass by the body. Each unit of energy radiated carries off its quota of mass, but as the mass ejected from the sun per year is only one part in 20 billionths ($1 \text{ in } 2 \times 10^{13}$) of the mass of the sun, and as this diminution in mass is not necessarily accompanied by any decrease in its gravitational attraction, we cannot expect to be able to get any evidence of this effect.

As our knowledge of the properties of light has progressed, we have been driven to recognise that the ether, when transmitting light, possesses properties which, before the introduction of the electromagnetic theory, would have been thought to be peculiar to an emission theory of light and to be fatal to the theory that light consists of undulations.

Take, for example, the pressure exerted by light. This would follow as a matter of course if we supposed light to be small particles moving with great velocities, for these, if they struck against a body, would manifestly tend to push it forward, while on the undulatory theory there seemed no reason why any effect of this kind should take place.

Indeed, in 1792, this very point was regarded as a test between the theories, and Bennet made experiments to see whether or not he could find any traces of this pressure. We now know that the pressure is there, and if Bennet's instrument had been more sensitive he must have observed it. It is perhaps fortunate that Bennet had not at his command more delicate apparatus. Had he discovered the pressure of light, it would have shaken confidence in the undulatory theory and

checked that magnificent work at the beginning of the last century which so greatly increased our knowledge of optics.

As another example, take the question of the distribution of energy in a wave of light. On the emission theory the energy in the light is the kinetic energy of the light particles. Thus the energy of light is made up of distinct units, the unit being the energy of one of the particles.

The idea that the energy has a structure of this kind has lately received a good deal of support. Planck, in a very remarkable series of investigations on the Thermodynamics of Radiation, pointed out that the expressions for the energy and entropy of radiant energy were of such a form as to suggest that the energy of radiation, like that of a gas on the molecular theory, was made up of distinct units, the magnitude of the unit depending on the colour of the light; and on this assumption he was able to calculate the value of the unit, and from this deduce incidentally the value of Avogadro's constant—the number of molecules in a cubic centimetre of gas at standard temperature and pressure.

This result is most interesting and important because if it were a legitimate deduction from the Second Law of Thermodynamics, it would appear that only a particular type of mechanism for the vibrators which give out light and the absorbers which absorb it could be in accordance with that law.

If this were so, then, regarding the universe as a collection of machines all obeying the laws of dynamics, the Second Law of Thermodynamics would only be true for a particular kind of machine.

There seems, however, grave objection to this view, which I may illustrate by the case of the First Law of Thermodynamics, the principle of the Conservation of Energy. This must be true whatever be the nature of the machines which make up the universe, provided they obey the laws of dynamics, any application of the principle of the Conservation of Energy could not discriminate between one type of machine and another.

Now, the Second Law of Thermodynamics, though not a dynamical principle in as strict a sense as the law of the Conservation of Energy, is one that we should expect to hold for a collection of a large number of machines of any type, provided that we could not directly affect the individual machines, but could only observe the average effects produced by an enormous number of them. On this view, the Second Law, as well as the First, should be incapable of saying that the machines were of any particular type: so that investigations founded on thermodynamics, though the expressions they lead to may suggest—cannot, I think, be regarded as proving—the unit structure of light energy.

It would seem as if in the application of thermodynamics to radiation some additional assumption has been implicitly introduced, for these applications lead to definite relations between the energy of the light of any particular wave length and the temperature of the luminous body.

Now a possible way of accounting for the light emitted by hot bodies is to suppose that it arises from the collisions of corpuscles with the molecules of the hot body, but it is only for one particular law of force between the corpuscles and the molecules that the distribution of energy would be the same as that deduced by the Second Law of Thermodynamics, so that in this case, as in the other, the results obtained by the application of thermodynamics to radiation would require us to suppose that the Second Law of Thermodynamics is only true for radiation when the radiation is produced by mechanism of a special type.

Quite apart, however, from considerations of thermodynamics, we should expect that the light from a luminous source should in many cases consist of parcels, possessing, at any rate to begin with, a definite amount of energy. Consider, for example, the case of a gas like sodium vapour, emitting light of a definite wave length; we may imagine that this light, consisting of electrical waves, is emitted by systems resembling Leyden jars. The energy originally possessed by such a system will be the electrostatic energy of the charged jar. When the vibrations are started, this energy will be radiated away into space, the radiation forming a complex system, containing, if the jar has no electrical resistance, the energy stored up in the jar.

The amount of this energy will depend on the size of the jar and the quantity of electricity with which it is charged. With regard to the charge, we must remember that we are dealing with systems formed out of single molecules, so that the charge will only consist of one or two natural units of electricity, or, at all events, some small multiple of that unit, while for geometrically similar Leyden jars the energy for a given charge will be proportional to the frequency of the vibration; thus, the energy in the bundle of radiation will be proportional to the frequency of the vibration.

We may picture to ourselves the radiation as consisting of the lines of electric force which, before the vibrations were started, were held bound by the charges on the jar, and which, when the vibrations begin, are thrown into rhythmic undulations, liberated from the jar and travel through space with the velocity of light.

Now let us suppose that this system strikes against an uncharged condenser and gives it a charge of electricity, the charge on the plates of the condenser must be at least one unit of electricity, because fractions of this charge do not exist, and each unit charge will anchor a unit tube of force, which must come from the parcel of radiation falling upon it. Thus a tube in the incident light will be anchored by the condenser, and the parcel formed by this tube will be anchored and withdrawn as a whole from the pencil of light incident on the condenser. If the energy required to charge up the condenser with a unit of electricity is greater than the energy in the incident parcel, the

tube will not be anchored and the light will pass over the condenser and escape from it. These principles that radiation is made up of units, and that it requires a unit possessing a definite amount of energy to excite radiation in a body on which it falls, perhaps receive their best illustration in the remarkable laws governing Secondary Röntgen radiation, recently discovered by Professor Barkla. Professor Barkla has found that each of the different chemical elements, when exposed to Röntgen rays, emit a definite type of secondary radiation whatever may have been the type of primary, thus lead emits one type, copper another, and so on; but these radiations are not excited at all if the primary radiation is of a softer type than the specific radiation emitted by the substance; thus the secondary radiation from lead being harder than that from copper; if copper is exposed to the secondary radiation from lead the copper will radiate, but lead will not radiate when exposed to copper. Thus, if we suppose that the energy in a unit of hard Röntgen rays is greater than that in one of soft, Barkla's results are strikingly analogous to those which would follow on the unit theory of light.

Though we have, I think, strong reasons for thinking that the energy in the light waves of definite wave length is done up into bundles, and that these bundles, when emitted, all possess the same amount of energy, I do not think there is any reason for supposing that in any casual specimen of light of this wave length, which may subsequent to its emission have been many times refracted or reflected, the bundles possess any definite amount of energy. For consider what must happen when a bundle is incident on a surface such as glass, when part of it is reflected and part transmitted. The bundle is divided into two portions, in each of which the energy is less than the incident bundle, and since these portions diverge and may ultimately be many thousands of miles apart, it would seem meaningless to still regard them as forming one unit. Thus the energy in the bundles of light, after they have suffered partial reflection, will not be the same as in the bundles when they were emitted. The study of the dimensions of these bundles, for example, the angle they subtend at the luminous source, is an interesting subject for investigation; experiments on interference between rays of light emerging in different directions from the luminous source would probably throw light on this point.

I now pass to a very brief consideration of one of the most important and interesting advances ever made in physics, and in which Canada, as the place of the labours of Professors Rutherford and Soddy, has taken a conspicuous part. I mean the discovery and investigation of radio-activity. Radio-activity was brought to light by the Röntgen rays. One of the many remarkable properties of these rays is to excite phosphorescence in certain substances, including the salts of uranium, when they fall upon them. Since Röntgen rays produce phosphor-

escence, it occurred to Becquerel to try whether phosphorescence would produce Röntgen rays. He took some uranium salts which had been made to phosphoresce by exposure, not to Röntgen rays but to sunlight, tested them, and found that they gave out rays possessing properties similar to Röntgen rays. Further investigation showed, however, that to get these rays it was not necessary to make the uranium phosphoresce, that the salts were just as active if they had been kept in the dark. It thus appeared that the property was due to the metal and not to the phosphorescence, and that uranium and its compounds possessed the power of giving out rays which, like Röntgen rays, affect a photographic plate, make certain minerals phosphoresce, and make gases through which they pass conductors of electricity.

Niepe de Saint-Victor had observed some years before this discovery that paper soaked in a solution of uranium nitrate affected a photographic plate, but the observation excited but little interest. The ground had not then been prepared, by the discovery of the Röntgen rays, for its reception, and it withered and was soon forgotten.

Shortly after Becquerel's discovery of uranium, Schmidt found that thorium possessed similar properties. Then Monsieur and Madame Curie, after a most difficult and laborious investigation, discovered two new substances, radium and polonium, possessing this property to an enormously greater extent than either thorium or uranium, and this was followed by the discovery of actinium by Debierne. Now the researches of Rutherford and others have led to the discovery of so many new radio-active substances that any attempts at christening seems to have been abandoned, and they are denoted, like policemen, by the letters of the alphabet.

Mr. Campbell has recently found that potassium, though far inferior in this respect to any of the substances I have named, emits an appreciable amount of radiation, the amount depending only on the quantity of potassium, and being the same whatever the source from which the potassium is obtained or whatever the elements with which it may be in combination.

The radiation emitted by these substances is of three types known as α , β and γ rays. The α rays have been shown by Rutherford to be positively electrified atoms of helium, moving with speeds which reach up to about one-tenth of the velocity of light. The β rays are negatively electrified corpuscles, moving in some cases with very nearly the velocity of light itself, while the γ rays are unelectrified, and are analogous to the Röntgen rays.

The radio-activity of uranium was shown by Crookes to arise from something mixed with the uranium, and which differed sufficiently in properties from the uranium itself to enable it to be separated by chemical analysis. He took some uranium, and by chemical treat-

ment separated it into two portions, one of which was radio-active and the other not.

Next Becquerel found that if these two portions were kept for several months, the part which was not radio-active to begin with regained radio-activity, while the part which was radio-active to begin with had lost its radio-activity. These effects and many others receive a complete explanation by the theory of radio-active change which we owe to Rutherford and Soddy.

According to this theory, the radio-active elements are not permanent, but are gradually breaking up into elements of lower atomic weight; uranium, for example, is slowly breaking up, one of the products being radium, while radium breaks up into a radio-active gas called radium emanation, the emanation into another radio-active substance, and so on, and that the radiations are a kind of swan's song emitted by the atoms when they pass from one form to another; that, for example, it is when a radium atom breaks up and an atom of the emanation appears that the rays which constitute the radio-activity are produced.

Thus, on this view the atoms of the radio-active elements are not immortal, they perish after a life whose average value ranges from thousands of millions of years in the case of uranium to a second or so in the case of the gaseous emanation from actinium.

When the atoms pass from one state to another they give out large stores of energy, thus their descendants do not inherit the whole of their wealth of stored-up energy, the estate becomes less and less wealthy with each generation; we find, in fact, that the politician, when he imposes death duties, is but imitating a process which has been going on for ages in the case of these radio-active substances.

Many points of interest arise when we consider the rate at which the atoms of radio-active substance disappear. Rutherford has shown that whatever be the age of these atoms, the percentage of atoms which disappear in one second is always the same; another way of putting it is that the expectation of life of an atom is independent of its age—that an atom of radium one thousand years old is just as likely to live for another thousand years as one just sprung into existence.

Now this would be the case if the death of the atom were due to something from outside which struck old and young indiscriminately; in a battle, for example, the chance of being shot is the same for old and young; so that we are inclined at first to look to something coming from outside as the cause why an atom of radium, for example, suddenly changes into an atom of the emanation. But here we are met with the difficulty that no changes in the external conditions that we have as yet been able to produce have had any effect on the life of the atom; as far as we know at present the life of a radium atom is the same at the temperature of a furnace as at that of liquid air—it is not altered by sur-

rounding the radium by thick screens of lead or other dense materials to ward off radiation from outside, and what to my mind is especially significant, it is the same when the radium is in the most concentrated form, when its atoms are exposed to the vigorous bombardment from the rays given off by the neighbouring atoms, as when it is in the most dilute solution, when the rays are absorbed by the water which separates one atom from another. This last result seems to me to make it somewhat improbable that we shall be able to split up the atoms of the non-radio-active elements by exposing them to the radiation from radium; if this radiation is unable to affect the unstable radio-active atoms, it is somewhat unlikely that it will be able to affect the much more stable non-radio-active elements.

The evidence we have at present is against a disturbance coming from outside breaking up of the radio-active atoms, and we must therefore look to some process of decay in the atom itself; but if this is the case, how are we to reconcile it with the fact that the expectation of life of an atom does not diminish as the atom gets older? We can do this if we suppose that the atoms when they are first produced have not all the same strength of constitution, that some are more robust than others, perhaps because they contain more intrinsic energy to begin with, and will therefore have a longer life. Now if when the atoms are first produced there are some which will live for one year, some for ten, some for a thousand, and so on; and if lives of all durations, from nothing to infinity, are present in such proportion that the number of atoms which will live longer than a certain number of years decrease in a constant proportion for each additional year of life, we can easily prove that the expectation of life of an atom will be the same whatever its age may be. On this view the different atoms of a radio-active substance are not, in all respects, identical.

The energy developed by radio-active substances is exceedingly large, one gramme of radium developing nearly as much energy as would be produced by burning a ton of coal. This energy is mainly in the α particles, the positively charged helium atoms which are emitted when the change in the atom takes place; if this energy were produced by electrical forces it would indicate that the helium atom had moved through a potential difference of about two million volts on its way out of the atom of radium. The source of this energy is a problem of the deepest interest; if it arises from the repulsion of similarly electrified systems exerting forces varying inversely as the square of the distance, then to get the requisite amount of energy the systems, if their charges were comparable with the charge on the α particle, could not when they start be further apart than the radius of a corpuscle, 10^{-13} cm. If we suppose that the particles do not acquire this energy at the explosion, but that before they are shot out of the radium atom they move in circles

inside this atom with the speed with which they emerge, the forces required to prevent particles moving with this velocity from flying off at a tangent are so great that finite charges of electricity could only produce them at distances comparable with the radius of a corpuscle.

One method by which the requisite amount of energy could be obtained is suggested by the view to which I have already alluded—that in the atom we have electrified systems of very different types, one small, the other large; the radius of one type is comparable with 10^{-13} cm., that of the other is about 100,000 times greater. The electrostatic potential energy in the smaller bodies is enormously greater than that in the larger ones; if one of these small bodies were to explode and expand to the size of the larger ones, we should have a liberation of energy large enough to endow an α particle with the energy it possesses. Is it possible that the positive units of electricity were, to begin with, quite as small as the negative, but while in the course of ages most of these have passed from the smaller stage to the larger, there are some small ones still lingering in radio-active substances, and it is the explosion of these which liberates the energy set free during radio-active transformation?

The properties of radium have consequences of enormous importance to the geologist as well as to the physicist or chemist. In fact, the discovery of these properties has entirely altered the aspect of one of the most interesting geological problems, that of the age of the earth. Before the discovery of radium it was supposed that the supplies of heat furnished by chemical changes going on in the earth were quite insignificant, and that there was nothing to replace the heat which flows from the hot interior of the earth to the colder crust. Now when the earth first solidified it only possessed a certain amount of capital in the form of heat, and if it is continually spending this capital and not gaining any fresh heat it is evident that the process cannot have been going on for more than a certain number of years, otherwise the earth would be colder than it is. Lord Kelvin in this way estimated the age of the earth to be less than 100 million years. Though the quantity of radium in the earth is an exceedingly small fraction of the mass of the earth, only amounting, according to the determinations of Professors Strutt and Joly, to about five grammes in a cube whose side is 100 miles, yet the amount of heat given out by this small quantity of radium is so great that it is more than enough to replace the heat which flows from the inside to the outside of the earth. This, as Rutherford has pointed out, entirely vitiates the previous method of determining the age of the earth. The fact is that the radium gives out so much heat that we do not quite know what to do with it, for if there was as much radium throughout the interior of the earth as there is in its crust, the temperature of the earth would increase much more rapidly than it does as we descend below the

earth's surface. Professor Strutt has shown that if radium behaves in the interior of the earth as it does at the surface, rocks similar to those in the earth's crust cannot extend to a depth of more than forty-five miles below the surface.

It is remarkable that Professor Milne from the study of earthquake phenomena had previously come to the conclusion that rocks similar to those at the earth's surface only descend a short distance below the surface; he estimates this distance at about thirty miles, and concludes that at a depth greater than this the earth is fairly homogeneous.

Though the discovery of radio-activity has taken away one method of calculating the age of the earth it has supplied another.

The gas helium is given out by radio-active bodies, and since, except in beryls, it is not found in minerals which do not contain radio-active elements, it is probable that all the helium in these minerals has come from these elements. In the case of a mineral containing uranium, the parent of radium in radio-active equilibrium, with radium and its products, helium will be produced at a definite rate. Helium, however, unlike the radio-active elements, is permanent and accumulates in the mineral; hence if we measure the amount of helium in a sample of rock and the amount produced by the sample in one year we can find the length of time the helium has been accumulating, and hence the age of the rock. This method, which is due to Professor Strutt, may lead to determinations not merely of the average age of the crust of the earth, but of the ages of particular rocks and the date at which the various strata were deposited; he has, for example, shown in this way that a specimen of the mineral thorionite must be more than 240 million years old.

The physiological and medical properties of the rays emitted by radium is a field of research in which enough has already been done to justify the hope that it may lead to considerable alleviation of human suffering. It seems quite definitely established that for some diseases, notably rodent ulcer, treatment with these rays has produced remarkable cures; it is imperative, lest we should be passing over a means of saving life and health, that the subject should be investigated in a much more systematic and extensive manner than there has yet been either time or material for. Radium is, however, so costly that few hospitals could afford to undertake pioneering work of this kind; fortunately, however, through the generosity of Sir Ernest Cassel and Lord Iveagh a Radium Institute, under the patronage of his Majesty the King, has been founded in London for the study of the medical properties of radium, and for the treatment of patients suffering from diseases for which radium is beneficial.

The new discoveries made in physics in the last few years, and the ideas and potentialities suggested by them, have had an effect upon the

workers in that subject akin to that produced in literature by the Renaissance. Enthusiasm has been quickened, and there is a hopeful, youthful, perhaps exuberant, spirit abroad which leads men to make with confidence experiments which would have been thought fantastic twenty years ago. It has quite dispelled the pessimistic feeling, not uncommon at that time, that all the interesting things had been discovered, and all that was left was to alter a decimal or two in some physical constant. There never was any justification for this feeling, there never were any signs of an approach to finality in science. The sum of knowledge is at present, at any rate, a diverging not a converging series. As we conquer peak after peak we see in front of us regions full of interest and beauty, but we do not see our goal, we do not see the horizon; in the distance tower still higher peaks, which will yield to those who ascend them still wider prospects, and deepen the feeling, whose truth is emphasised by every advance in science, that 'Great are the Works of the Lord.'

British Association for the Advancement of Science.

WINNIPEG, 1909.

ADDRESS

TO THE

MATHEMATICAL AND PHYSICAL SECTION

BY

PROFESSOR E. RUTHERFORD, M.A., D.Sc., F.R.S.,

PRESIDENT OF THE SECTION.

It is a great privilege and pleasure to address the members of this Section on the occasion of the visit of the British Association to a country with which I have had such a long and pleasant connection. I feel myself in the presence of old friends, for the greater part of what may be called my scientific life has been spent in Canada, and I owe much to this country for the unusual facilities and opportunity for research so liberally provided by one of her great Universities. Canada may well regard with pride her Universities, which have made such liberal provision for teaching and research in pure and applied science. As a physicist, I may be allowed to refer in particular to the subject with which I am most intimately connected. After seeing the splendid home for physical science recently erected by the University of Toronto, and the older but no less serviceable and admirably equipped laboratories of McGill University, one cannot but feel that Canada has recognised in a striking manner the great value attaching to teaching and research in physical science. In this, as in other branches of knowledge, Canada has made notable contributions in the past, and we may confidently anticipate that this is but an earnest of what will be accomplished in the future.

It is my intention to-day to say a few words upon the present position of the atomic theory in physical science, and to discuss briefly the various methods that have been devised to determine the values of certain fundamental atomic magnitudes. The present time seems very opportune for this purpose, for the rapid advance of physics during the last decade has not only given us a much clearer conception of the relation between electricity and matter and of the constitution of the atom, but has provided us with experimental methods of attack undreamt of a few years ago. At a time when, in the vision of the physicist, the atmosphere is dim with flying fragments of atoms, it may not be out of place to see how it has fared with the atoms themselves, and to look carefully at the atomic foundations on which the great superstructure of modern science has been raised.

Every physicist and chemist cannot but be aware of the great part the atomic hypothesis plays in science to-day. The idea that matter consists of a great number of small discrete particles forms practically the basis of the explanation of all properties of matter. As an indication of the importance of this theory in the advance of science it is of interest to read over the Reports of this Association and to note how many addresses, either wholly or in part, have been devoted to a consideration of this subject. Amongst numerous examples I may instance the famous and oft-quoted lecture of Maxwell on Molecules, at Bradford in 1873; the discussion of the Kinetic Theory of Gases by Lord Kelvin, then Sir William Thomson, in Montreal in 1884; and the Presidential Address of Sir Arthur Rucker in 1901, which will be recalled by many here to-day.

It is far from my intention to discuss, except with extreme brevity, the gradual rise and development of the atomic theory. From the point of view of modern science, the atomic theory dates from the work of Dalton about 1805, who put it forward as an explanation of the combination of elements in definite proportions. The simplicity of this explanation of the facts of chemistry led to the rapid adoption of the atomic theory as a very convenient and valuable working hypothesis. By the labour of the chemists matter was shown to be composed of a number of elementary substances which could not be further decomposed by laboratory agencies, and the relative weights of the atoms of the elements were determined. On the physical side, the mathematical development of the kinetic or dynamical theory of gases by the labours of Clausius and Clerk Maxwell enormously extended the utility of this conception. It was shown that the properties of gases could be satisfactorily explained on the assumption that a gas consisted of a great assemblage of minute particles or molecules in continuous agitation, colliding with each other and with the walls of the containing vessel. Between encounters the molecules travelled in straight lines, and the free path of the molecules between collisions was supposed to be large compared with the linear dimensions of the molecules themselves. One cannot but regard with admiration the remarkable success of this statistical theory in explaining the general properties of gases and even predicting unexpected relations. The strength and at the same time the limitations of the theory lie in the fact that it does not involve any definite conception of the nature of the molecules themselves or of the forces acting between them. The molecule, for example, may be considered as a perfectly elastic sphere or a Boscovitch centre of force, as Lord Kelvin preferred to regard it, and yet on suitable assumptions the gas would show the same general statistical properties. We are consequently unable, without the aid of special subsidiary hypotheses, to draw conclusions of value in regard to the nature of the molecules themselves.

Towards the close of the last century the ideas of the atomic theory had impregnated a very large part of the domain of physics and chemistry. The conception of atoms became more and more concrete. The atom in imagination was endowed with size and shape, and unconsciously in many cases with colour. The simplicity and utility of atomic conceptions in explaining the most diverse phenomena of physics and chemistry naturally tended to enhance the importance of the theory in the eyes of the scientific worker. There was a tendency to regard the atomic theory as one of the established facts of nature, and not as a useful working hypothesis for which it was exceedingly difficult to obtain direct and convincing evidence. There were not wanting scientific men and philosophers to point out the uncertain foundations of the theory on which so much depended. Granting how useful molecular ideas were for the explanation of experimental facts, what evidence was there that the atoms were realities and not the figments

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of the imagination? It must be confessed that this lack of direct evidence did not in any way detract from the strength of the belief of the great majority of scientific men in the discreteness of matter. It was not unnatural, however, that there should be a reaction in some quarters against the domination of the atomic theory in physics and in chemistry. A school of thought arose that wished to do away with the atomic theory as the basis of explanation of chemistry, and substitute as its equivalent the law of combination in definite proportions. This movement was assisted by the possibility of explaining many chemical facts on the basis of thermodynamics without the aid of any hypothesis as to the particular structure of matter. Everyone recognises the great importance of such general methods of explanation, but the trouble is that few can think, or at any rate think correctly, in terms of thermo-dynamics. The negation of the atomic theory has not, and does not, help us to make new discoveries. The great advantage of the atomic theory is that it provides, so to speak, a tangible and concrete idea of matter which serves at once for the explanation of a multitude of facts and is of enormous aid as a working hypothesis. For the great majority of scientists it is not sufficient to group together a number of facts on general abstract principles. What is wanted is a concrete idea, however crude it may be, of the mechanism of the phenomena. This may be a weakness of the scientific mind, but it is one that deserves our sympathetic consideration. It represents an attitude of mind that appeals, I think, very strongly to the Anglo-Saxon temperament. It has no doubt as its basis the underlying idea that the facts of nature are ultimately explicable on general dynamical principles, and that there must consequently be some type of mechanism capable of accounting for the observed facts.

It has been generally considered that a decisive proof of the atomic structure of matter was in the nature of things impossible, and that the atomic theory must of necessity remain an hypothesis unverifiable by direct methods. Recent investigations have, however, disclosed such new and powerful methods of attack that we may well ask the question whether we do not now possess more decisive evidence of its truth.

Since molecules are invisible, it might appear, for example, an impossible hope that an experiment could be devised to show that the molecules of a fluid are in that state of continuous agitation which the kinetic theory leads us to suppose. In this connection I should like to draw your attention for a short time to a most striking phenomenon known as the 'Brownian movement,' which has been closely studied in recent years. Quite apart from its probable explanation the phenomenon is of unusual interest. In 1827 the English botanist Brown observed by means of a microscope that minute particles like spores of plants introduced into a fluid were always in a state of continuous irregular agitation, dancing to and fro in all directions at considerable speeds. For a long time this effect, known as the Brownian movement, was ascribed to inequalities in the temperature of the solution. This was disproved by a number of subsequent investigations, and especially by those of Gouy, who showed that the movement was spontaneous and continuous and was exhibited by very small particles of whatever kind when immersed in a fluid medium. The velocity of agitation increased with decrease of diameter of the particles and increased with temperature, and was dependent on the viscosity of the surrounding fluid. With the advent of the ultra-microscope it has been possible to follow the movements with more certainty and to experiment with much smaller particles. Exner and Zsigmondy have determined the mean velocity of particles of known diameter in various solutions, while Svedberg has devised an ingenious method of determining the mean free

path and the average velocity of particles of different diameter. The experiments of Ehrenhaft in 1907 showed that the Brownian movement was not confined to liquids, but was exhibited far more markedly by small particles suspended in gases. By passing an arc discharge between silver poles he produced a fine dust of silver in the air. When examined by means of the ultra-microscope the suspended particles exhibited the characteristic Brownian movement, with the difference that the mean free path for particles of the same size was much greater in gases than in liquids.

The particles exhibit in general the character of the motion which the kinetic theory ascribes to the molecules themselves, although even the smallest particles examined have a mass which is undoubtedly very large compared with that of the molecule. The character of the Brownian movement irresistibly impresses the observer with the idea that the particles are hurled hither and thither by the action of forces resident in the solution, and that these can only arise from the continuous and ceaseless movement of the invisible molecules of which the fluid is composed. Smoluchowski and Einstein have suggested explanations which are based on the kinetic theory, and there is a fair agreement between calculation and experiment. Strong additional confirmation of this view has been supplied by the very recent experiments of Perrin (1909). He obtained an emulsion of gamboge in water which consisted of a great number of spherical particles nearly of the same size, which showed the characteristic Brownian movement. The particles settled under gravity and when equilibrium was set up the distribution of these particles in layers at different heights was determined by counting the particles with a microscope. The number was found to diminish from the bottom of the vessel upwards according to an exponential law—i.e., according to the same law as the pressure of the atmosphere diminishes from the surface of the earth. In this case, however, on account of the great mass of the particles, their distribution was confined to a region only a fraction of a millimetre deep. In a particular experiment the number of particles per unit volume decreased to half in a distance of 0.038 millimetre, while the corresponding distance in our atmosphere is about 6000 metres. From measurements of the diameter and weight of each particle, Perrin found that, within the limit of experimental error, the law of distribution with height indicated that each small particle had the same average kinetic energy of movement as the molecules of the solutions in which they were suspended; in fact, the particles in suspension behaved in all respects like molecules of very high molecular weight. This is a very important result, for it indicates that the law of equipartition of energy among molecules of different masses, which is an important deduction from the kinetic theory, holds, at any rate very approximately, for a distribution of particles in a medium whose masses and dimensions are exceedingly large compared with that of the molecules of the medium. Whatever may prove to be the exact explanation of this phenomenon, there can be little doubt that it results from the movement of the molecules of the solution and is thus a striking if somewhat indirect proof of the general correctness of the kinetic theory of matter.

From recent work in radioactivity we may take a second illustration which is novel and far more direct. It is well known that the α rays of radium are deflected by both magnetic and electric fields. It may be concluded from this evidence that the radiation is corpuscular in character, consisting of a stream of positively charged particles projected from the radium at a very high velocity. From the measurements of the deflection of the rays in passing through magnetic and electric fields the ratio e/m of the charge carried by the particle to its mass has been determined, and

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the magnitude of this quantity indicates that the particle is of atomic dimensions.

Rutherford and Geiger have recently developed a direct method of showing that this radiation is, as the other evidence indicated, discontinuous, and that it is possible to detect by a special electric method the passage of a single α particle into a suitable detecting vessel. The entrance of an α particle through a small opening was marked by a sudden movement of the needle of the electrometer which was used as a measuring instrument. In this way, by counting the number of separate impulses communicated to the electrometer needle, it was possible to determine by direct counting the number of α particles expelled per second from one gram of radium. But we can go further and confirm the result by counting the number of α particles by an entirely distinct method. Sir William Crookes has shown that when the α rays are allowed to fall upon a screen of phosphorescent zinc sulphide, a number of brilliant scintillations are observed. It appears as if the impact of each α particle produced a visible flash of light where it struck the screen. Using suitable screens the number of scintillations per second on a given area can be counted by means of a microscope. It has been shown that the number of scintillations determined in this way is equal to the number of impinging α particles when counted by the electric method. This shows that the impact of each α particle on the zinc sulphide produces a visible scintillation. There are thus two distinct methods—one electrical, the other optical—for detecting the emission of a single α particle from radium. The next question to consider is the nature of the α particle itself. The general evidence indicates that the α particle is a charged atom of helium, and this conclusion was decisively verified by Rutherford and Royds by showing that helium appeared in an exhausted space into which the α particles were fired. The helium, which is produced by radium, is due to the accumulated α particles which are so continuously expelled from it. If the rate of production of helium from radium is measured, we thus have a means of determining directly how many α particles are required to form a given volume of helium gas. This rate of production has recently been measured accurately by Sir James Dewar. He has informed me that his final measurements show that one gram of radium in radioactive equilibrium produces 0.46 cubic millimetres of helium per day, or 5.32×10^{-6} cubic millimetres per second. Now from the direct counting experiments it is known that 13.6×10^{10} α particles are shot out per second from one gram of radium in equilibrium. Consequently it requires 2.56×10^{19} α particles to form one cubic centimetre of helium gas at standard pressure and temperature.

From other lines of evidence it is known that all the α particles from whatever source are identical in mass and constitution. It is not then unreasonable to suppose that the α particle, which exists as a separate entity in its flight, can exist also as a separate entity when the α particles are collected together to form a measurable volume of helium gas, or, in other words, that the α particle on losing its charge becomes the fundamental unit or atom of helium. In the case of a monatomic gas like helium, where the atom and molecule are believed to be identical, no difficulty of deduction arises from the possible combination of two or more atoms to form a complex molecule.

We consequently conclude from these experiments that one cubic centimetre of helium at standard pressure and temperature contains 2.56×10^{19} atoms. Knowing the density of helium, it at once follows that each atom of helium has a mass of 6.8×10^{-24} grams, and that the average distance apart of the molecules in the gaseous state at standard pressure and temperature is 3.4×10^{-7} centimetres.

The above result can be confirmed in a different way. It is known that the value of e/m for the α particle is 5,070 electromagnetic units. The positive charge carried by each α particle has been deduced by measuring the total charge carried by a counted number of α particles. Its value is 9.3×10^{-10} electrostatic units, or 3.1×10^{-20} electromagnetic units. Substituting this number in the value of e/m , it is seen that m , the mass of the α particle, is equal to 6.1×10^{-24} grams—a value in fair agreement with the number previously given.

I trust that my judgment is not prejudiced by the fact that I have taken some share in these investigations; but the experiments, taken as a whole, appear to me to give an almost direct and convincing proof of the atomic hypothesis of matter. By direct counting, the number of identical entities required to form a known volume of gas has been measured. May we not conclude that the gas is discrete in structure, and that this number represents the actual number of atoms in the gas?

We have seen that under special conditions it is possible to detect easily by an electrical method the emission of a single α particle—*i.e.*, of a single charged atom of matter. This has been rendered possible by the great velocity and energy of the expelled α particle, which confers on it the power of dissociating or ionising the gas through which it passes. It is obviously only possible to detect the presence of a single atom of matter when it is endowed with some special property or properties which distinguishes it from the molecules of the gas with which it is surrounded. There is a very important and striking method, for example, of visibly differentiating between the ordinary molecules of a gas and the ions produced in the gas by various agencies. C. T. R. Wilson showed in 1897 that under certain conditions each charged ion became a centre of condensation of water vapour, so that the presence of each ion was rendered visible to the eye. Sir Joseph Thomson, H. A. Wilson, and others have employed this method to count the number of ions present and to determine the magnitude of the electric charge carried by each.

A few examples will now be given which illustrate the older methods of estimating the mass and dimensions of molecules. As soon as the idea of the discrete structure of matter had taken firm hold, it was natural that attempts should be made to estimate the degree of coarse-grainedness of matter, and to form an idea of the dimension of molecules, assuming that they have extension in space. Lord Rayleigh has drawn attention to the fact that the earliest estimate of this kind was made by Thomas Young in 1805, from considerations of the theory of capillarity. Space does not allow me to consider the great variety of methods that have later been employed to form an idea of the thickness of a film of matter in which a molecular structure is discernible. This phase of the subject was always a favourite one with Lord Kelvin, who developed a number of important methods of estimating the probable dimensions of molecular structure.

The development of the kinetic theory of gases on a mathematical basis at once suggested methods of estimating the number of molecules in a cubic centimetre of any gas at normal pressure and temperature. This number, which will throughout be denoted by the symbol N , is a fundamental constant of gases; for, according to the hypothesis of Avogadro, and also on the kinetic theory, all gases at normal pressure and temperature have an identical number of molecules in unit volume. Knowing the value of N , approximate estimates can be made of the diameter of the molecule; but in our ignorance of the constitution of the molecule, the meaning of the term diameter is somewhat indefinite. It is usually considered to refer to the diameter of the sphere of action of the forces surrounding the molecule. This diameter is not necessarily the same for the molecules of all

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gases, so that it is preferable to consider the magnitude of the fundamental constant N . The earliest estimates based on the kinetic theory were made by Loschmidt, Johnstone Stoney, and Maxwell. From the data then at his disposal, the latter found N to be 1.9×10^{19} . Meyer, in his 'Kinetic Theory of Gases,' discusses the various methods of estimating the dimensions of molecules on the theory, and concludes that the most probable estimate of N is 6.1×10^{19} . Estimates of N based on the kinetic theory are only approximate, and in many cases serve merely to fix an inferior or superior limit to the number of the molecules. Such estimates are, however, of considerable interest and historical importance, since for a long time they served as the most reliable methods of forming an idea of molecular magnitudes.

A very interesting and impressive method of determining the value of N was given by Lord Rayleigh in 1899 as a deduction from his theory of the blue colour in the cloudless sky. This theory supposes that the molecules of the air scatter the waves of light incident upon them. This scattering for particles, small compared with the wave length of light, is proportional to the fourth power of the wave length, so that the proportion of scattered to incident light is much greater for the violet than for the red end of the spectrum, and consequently the sky which is viewed by the scattered light is of a deep blue colour. This scattering of the light in passing through the atmosphere causes alterations of brightness of stars when viewed at different altitudes, and determinations of this loss of brightness have been made experimentally. Knowing this value, the number N of molecules in unit volume can be deduced by aid of the theory. From the data thus available, Lord Rayleigh concluded that the value of N was not less than 7×10^{18} . Lord Kelvin in 1902 recalculated the value of N on the theory by using more recent and more accurate data, and found it to be 2.47×10^{19} . Since in the simple theory no account is taken of the additional scattering due to fine suspended particles which are undoubtedly present in the atmosphere, this method only serves to fix an inferior limit to the value of N . It is difficult to estimate with accuracy the correction to be applied for this effect, but it will be seen that the uncorrected number deduced by Lord Kelvin is not much smaller than the most probable value 2.77×10^{19} given later. Assuming the correctness of the theory and data employed, this would indicate that the scattering due to suspended particles in the atmosphere is only a small portion of the total scattering due to molecules of air. This is an interesting example of how an accurate knowledge of the value of N may possibly assist in forming an estimate of unknown magnitudes.

It is now necessary to consider some of the more recent and direct methods of estimating N which are based on recent additions to our scientific knowledge. The newer methods allow us to fix the value of N with much more certainty and precision than was possible a few years ago.

We have referred earlier in the paper to the investigations of Perrin on the law of distribution in a fluid of a great number of minute granules, and his proof that the granules behave like molecules of high molecular weight. The value of N can be deduced at once from the experimental results, and is found to be 3.14×10^{19} . The method developed by Perrin is a very novel and ingenious one, and is of great importance in throwing light on the law of equipartition of energy. This new method of attack of fundamental problems will no doubt be much further developed in the future.

It has already been shown that the value $N = 2.56 \times 10^{19}$ has been obtained by the direct method of counting the particles and determining the corresponding volume of helium produced. Another very simple method of determining N from radioactive data is based on the rate of transforma-

tion of radium. Boltwood has shown by direct experiment that radium is half transformed in 2,000 years. From this it follows that initially in a gram of radium $\cdot 346$ milligram breaks up per year. Now it is known from the counting method that $3\cdot4\times 10^{10}$ α particles are expelled per second from one gram of radium, and the evidence indicates that one α particle accompanies the disintegration of each atom. Consequently the number of α particles expelled per year is a measure of the number of atoms of radium present in $\cdot 346$ milligram. From this it follows that there are $3\cdot1\times 10^{21}$ atoms in one gram of radium, and taking the atomic weight of radium as 225, it is simply deduced that the value of N is $3\cdot1\times 10^{19}$.

The study of the properties of ionised gases in recent years has led to the development of a number of important methods of determining the charge carried by the ion, produced in gases by α rays or the rays from radioactive substances. On modern views, electricity, like matter, is supposed to be discrete in structure, and the charge carried by the hydrogen atom set free by the electrolysis of water is taken as the fundamental unit of quantity of electricity. On this view, which is supported by strong evidence, the charge carried by the hydrogen atom is the smallest unit of electricity that can be obtained, and every quantity of electricity consists of an integral multiple of this unit. The experiments of Townsend have shown that the charge carried by a gaseous ion is, in the majority of cases, the same and equal in magnitude to the charge carried by a hydrogen atom in the electrolysis of water. From measurement of the quantity of electricity required to set free one gram of hydrogen in electrolysis, it can be deduced that $Ne=1\cdot29\times 10^{10}$ electrostatic units where N , as before, is the number of molecules of hydrogen in one cubic centimetre of gas, and e the charge carried by each ion. If e be determined experimentally, the value of N can at once be deduced from this relation.

The first direct measurement of the charge carried by the ion was made by Townsend in 1897. When a solution of sulphuric acid is electrolysed, the liberated oxygen is found in a moist atmosphere to give rise to a dense cloud composed of minute globules of water. Each of these minute drops carries a negative charge of electricity. The size of the globules, and consequently the weight, was deduced with the aid of Stokes' formula by observing the rate of fall of the cloud under gravity. The weight of the cloud was measured, and, knowing the weight of each globule, the total number of drops present was determined. Since the total charge carried by the cloud was measured, the charge e carried by each drop was deduced. The value of e , the charge carried by each drop, was found by this method to be about $3\cdot0\times 10^{-10}$ electrostatic units. The corresponding value of N is about $4\cdot3\times 10^{19}$.

We have already referred to the method discovered by C. T. R. Wilson of rendering each ion visible by the condensation of water upon it by a sudden expansion of the gas. The property was utilised by Sir Joseph Thomson to measure the charge e carried by each ion. When the expansion of the gas exceeds a certain value, the water condenses on both the negative and positive ions, and a dense cloud of small water drops is seen. J. J. Thomson found $e=3\cdot4\times 10^{-10}$, H. A. Wilson $e=3\cdot1\times 10^{-10}$, and Millikan and Begeman $4\cdot06\times 10^{-10}$. The corresponding values of N are $3\cdot8$, $4\cdot2$, and $3\cdot2\times 10^{19}$ respectively. This method is of great interest and importance, as it provides a method of directly counting the number of ions produced in the gas. An exact determination of e by this method is, however, unfortunately beset with great experimental difficulties.

Moreau has recently measured the charge carried by the negative ions produced in flames. The values deduced for e and N were respectively $4\cdot3\times 10^{-10}$ and $3\cdot0\times 10^{19}$.

We have referred earlier in the paper to the work of Ehrenhaft on the Brownian movement in air shown by ultra-microscopic dust of silver. In a recent paper (1909) he has shown that each of these particles carries a positive or negative charge. The size of each particle was measured by the ultra-microscope, and also by the rate of fall under gravity. The charge carried by each particle was deduced from the measured mass of the particle, and its rate of movement in an electric field. The mean value of e was found to be 4.6×10^{-10} , and thus N becomes 2.74×10^{10} .

A third important method of determination of N from radioactive data was given by Rutherford and Geiger in 1908. The charge carried by each α particle expelled from radium was measured by directly determining the total charge carried by a counted number of α particles. The value of the charge on each α particle was found to be 9.3×10^{-10} . From consideration of the general evidence, it was concluded that each α particle carries two unit positive charges, so that the value of e becomes 4.65×10^{-10} , and of N 2.77×10^{10} . This method is deserving of considerable confidence as the measurements involved are direct and capable of accuracy.

The methods of determination of e , so far explained, have depended on direct experiment. This discussion would not be complete without a reference to an important determination of e from theoretical considerations by Planck. From the theory of the distribution of energy in the spectrum of a hot body, Planck found that $e = 4.69 \times 10^{-10}$, and $N = 2.80 \times 10^{10}$. For reasons that we cannot enter into here, this theoretical deduction must be given great weight.

When we consider the great diversity of the theories and methods which have been utilised to determine the values of the atomic constants e and N , and the probable experimental errors, the agreement among the numbers is remarkably close. This is especially the case in considering the more recent measurements by very different methods, which are far more reliable than the older estimates. It is difficult to fix on one determination as more deserving of confidence than another; but I may be pardoned if I place some reliance on the radioactive method previously discussed, which depends on the charge carried by the α particle. The value obtained in this way is not only in close agreement with the theoretical estimate of Planck, but is in fair agreement with the recent determinations by several other distinct methods. We may consequently conclude that the number of molecules in a cubic centimetre of any gas at standard pressure and temperature is about 2.77×10^{10} , and that the value of the fundamental unit of quantity of electricity is about 4.65×10^{-10} electrostatic units. From these data it is a simple matter to deduce the mass of any atom whose atomic weight is known, and to determine the values of a number of related atomic and molecular magnitudes.

There is now no reason to view the values of these fundamental constants with scepticism, but they may be employed with confidence in calculations to advance still further our knowledge of the constitution of atoms and molecules. There will no doubt be a great number of investigations in the future to fix the values of these important constants with the greatest possible precision; but there is every reason to believe that the values are already known with reasonable certainty, and with a degree of accuracy far greater than it was possible to attain a few years ago. The remarkable agreement in the values of e and N , based on so many different theories, of itself affords exceedingly strong evidence of the correctness of the atomic theory of matter, and of electricity, for it is difficult to believe that such concordance would show itself if the atoms and their charges had no real existence.

There has been a tendency in some quarters to suppose that the develop-

ment of physics in recent years has cast doubt on the validity of the atomic theory of matter. This view is quite erroneous, for it will be clear from the evidence already discussed that the recent discoveries have not only greatly strengthened the evidence in support of the theory, but have given an almost direct and convincing proof of its correctness. The chemical atom as a definite unit in the subdivision of matter is now fixed in an impregnable position in science. Leaving out of account considerations of etymology, the atom in chemistry has long been considered to refer only to the smallest unit of matter that enters into ordinary chemical combination. There is no assumption made that the atom itself is indestructible and eternal, or that methods may not ultimately be found for its subdivision into still more elementary units. The advent of the electron has shown that the atom is not the unit of smallest mass of which we have cognisance, while the study of radioactive bodies has shown that the atoms of a few elements of high atomic weight are not permanently stable, but break up spontaneously with the appearance of new types of matter. These advances in knowledge do not in any way invalidate the position of the chemical atom, but rather indicate its great importance as a subdivision of matter whose properties should be exhaustively studied.

The proof of the existence of corpuscles or electrons with an apparent mass very small compared with that of the hydrogen atom, marks an important stage in the extension of our ideas of atomic constitution. This discovery, which has exercised a profound influence on the development of modern physics, we owe mainly to the genius of the President of this Association. The existence of the electron as a distinct entity is established by similar methods and with almost the same certainty as the existence of individual α particles. While it has not yet been found possible to detect a single electron by its electrical or optical effect, and thus to count the number directly as in the case of the α particles, there seems to be no reason why this should not be accomplished by the electric method. The effect to be anticipated for a single β particle is much smaller than that due to an α particle, but not too small for measurement. In this connection it is of interest to note that Regener has observed evidence of scintillations produced by the β particles of radium falling on a screen of platinocyanide of barium, but the scintillations are too feeble to count with certainty.

Experiment has shown that the apparent mass of the electron varies with its speed, and, by comparison of theory with experiment, it has been concluded that the mass of the electron is entirely electrical in origin and that there is no necessity to assume a material nucleus on which the electrical charge is distributed. While there can be no doubt that electrons can be released from the atom or molecule by a variety of agencies and, when in rapid motion, can retain an independent existence, there is still much room for discussion as to the actual constitution of electrons, if such a term may be employed, and of the part they play in atomic structure. There can be little doubt that the atom is a complex system, consisting of a number of positively and negatively charged masses which are held in equilibrium mainly by electrical forces; but it is difficult to assign the relative importance of the rôle played by the carriers of positive and negative electricity. While negative electricity can exist as a separate entity in the electron, there is yet no decisive proof of the existence of a corresponding positive electron. It is not known how much of the mass of an atom is due to electrons or other moving charges, or whether a type of mass quite distinct from electrical mass exists. Advance in this direction must be delayed until a clearer knowledge is gained of the character and structure of positive electricity and of its relation to the negative electron.

The general experimental evidence indicates that electrons play two distinct rôles in the structure of the atom, one as lightly attached and easily removable satellites or outliers of the atomic system, and the other as integral constituents of the interior structure of the atom. The former, which can be easily detached or set in vibration, probably play an important part in the combination of atoms to form molecules, and in the spectra of the elements; the latter, which are held in place by much stronger forces, can only be released as a result of an atomic explosion involving the disintegration of the atom. For example, the release of an electron with slow velocity by ordinary laboratory agencies does not appear to endanger the stability of the atom, but the expulsion of a high speed electron from a radioactive substance accompanies the transformation of the atom.

The idea that the atoms of the elements may be complex structures, made up either of lighter atoms or of the atoms of some fundamental substance, has long been familiar to science. So far no direct evidence has been obtained of the possibility of building up an atom of higher atomic weight from one of lower atomic weight, but in the case of the radioactive substances we have decisive and definite evidence that certain elements show the converse process of disintegration. It may be significant that this process has only been observed in the atoms of highest atomic weights, like those of uranium, thorium and radium. With the exception possibly of potassium, there is no reliable evidence that a similar process takes place in other elements. The transformation of the atom of a radioactive substance appears to result from an atomic explosion of great intensity in which a part of the atom is expelled with great speed. In the majority of cases, an α particle or atom of helium is ejected, in some cases a high-speed electron, while a few substances are transformed without the appearance of a detectable radiation. The fact that the α particles from a simple substance are all ejected with an identical and very high velocity suggests the probability that the charged helium atom before its expulsion is in rapid orbital movement in the atom. There is at present no definite evidence of the causes operative in these atomic transformations.

Since in a large number of cases the transformation of the atoms are accompanied by the expulsion of one or more charged atoms of helium, it is difficult to avoid the conclusion that the atoms of the radioactive elements are built up, in part at least, of helium atoms. It is certainly very remarkable and may prove of great significance, that helium, which is regarded from the ordinary chemical standpoint as an inert element, plays such an important part in the constitution of the atoms of uranium, thorium and radium.

The study of radioactivity has not only thrown great light on the character of atomic transformations, but it has also led to the development of methods for detecting the presence of almost infinitesimal quantities of radioactive matter. It has already been pointed out that two methods—one electrical, the other optical—have been devised for the detection of a single α particle. By the use of the optical or scintillation method, it is possible to count with accuracy the number of α particles when only one is expelled per minute. It is not a difficult matter, consequently, to follow the transformation of any radioactive substance in which only one atom breaks up per minute, provided that an α particle accompanies the transformation. In the case of a rapidly changing substance like the actinium emanation, which has a half period of 3.7 seconds, it is possible to detect with certainty the presence, if not of a single atom, at any rate of a few atoms, while the presence of a hundred atoms would in some cases give an inconveniently large effect. The counting of the scintillations affords an exceedingly power-

ful and direct quantitative method of studying the properties of radioactive substances which expel α particles. Not only is it a simple matter to count the number of α particles which are expelled in any given interval, but it is possible, for example, by suitably arranged experiments to decide whether one, two or more α particles are expelled at the disintegration of a single atom.

The possibility of detection of a single atom of matter has opened up a new field of investigation in the study of discontinuous phenomena. For example, the experimental law of transformation of radioactive matter expresses only the average rate of transformation, but by the aid of the scintillation or electric method it is possible to determine directly by experiment the actual interval between the disintegration of successive atoms and the probability law of distribution of the α particles about the average value.

Quite apart from the importance of studying radioactive changes, the radiations from active bodies provide very valuable information as to the effects produced by high velocity particles in traversing matter. The three types of radiation, the α , β and γ rays, emitted from active bodies, differ widely in character and their power of penetration of matter. The α particles, for example, are completely stopped by a sheet of notepaper, while the γ rays from radium can be easily detected after traversing twenty centimetres of lead. The differences in the character of the absorption of the radiations are no doubt partly due to the difference in type of the radiation and partly due to the differences of velocity.

The character of the effects produced by the α and β particles is most simply studied in gases. The α particle has such great energy of motion that it plunges through the molecules of the gas in its path, and leaves in its train more than a hundred thousand ionised or dissociated molecules. After traversing a certain distance, the α particle suddenly loses its characteristic properties and vanishes from the ken of our observational methods. It no doubt quickly loses its high velocity, and after its charge has been neutralised becomes a wandering atom of helium. The ionisation produced by the α particle appears to consist of the liberation of one or more slow velocity electrons from the molecule, but in the case of complex gases there is no doubt that the act of ionisation is accompanied by a chemical dissociation of the molecule itself, although it is difficult to decide whether this dissociation is a primary or secondary effect. The chemical dissociation produced by α particles opens up a wide field of investigation, on which, so far, only a beginning has been made.

The β particle differs from the α particle in its much greater power of penetration of matter, and the very small number of molecules it ionises compared with the α particle traversing the same path in the gas. It is very easily deflected from its path by encounters with the gas molecules, and there is strong evidence that, unlike the α particle, the β particle can be stopped or entrapped by a molecule when travelling at a very high speed.

When the great energy of motion of the α particle and the small amount of energy absorbed in ionising a single molecule are taken into consideration, there appears to be no doubt that the α particle, as Bragg pointed out, actually passes through the atom, or rather the sphere of action of the atom which lies in its path. There is, so to speak, no time for the atom to get out of the way of the swiftly moving α particle, but the latter must pass through the atomic system. On this view, the old dictum, no doubt true in most cases, that two bodies cannot occupy the same space, no longer holds for atoms of matter if moving at a sufficiently high speed.

There would appear to be little doubt that a careful study of the effects produced by the α or β particle in passing through matter will ultimately throw much further light on the constitution of the atom itself. Work already done shows that the character of the absorption of the radiations is intimately connected with the atomic weights of the elements and their position in the periodic table. One of the most striking effects of the passage of β rays through matter is the scattering of the β particles, *i.e.*, the deflection from their rectilinear path by their encounters with the molecules. It was for some time thought that such a scattering could not be expected to occur in the case of the α particles in consequence of their much greater mass and energy of motion. The recent experiments of Geiger, however, show that the scattering of the α particles is very marked, and is so great that a small fraction of the α particles, which impinge on a screen of metal, have their velocity reversed in direction and emerge again on the same side. This scattering can be most conveniently studied by the method of scintillations. It can be shown that the deflection of the α particle from its path is quite perceptible after passing through very few atoms of matter. The conclusion is unavoidable that the atom is the seat of an intense electric field, for otherwise it would be impossible to change the direction of the particle in passing over such a minute distance as the diameter of a molecule.

In conclusion, I should like to emphasise the simplicity and directness of the methods of attack on atomic problems opened up by recent discoveries. As we have seen, not only is it a simple matter, for example, to count the number of α particles by the scintillations produced on a zinc sulphide screen, but it is possible to examine directly the deflection of an individual particle in passing through a magnetic or electric field, and to determine the deviation of each particle from a rectilinear path due to encounters with molecules of matter. We can determine directly the mass of each α particle, its charge, and its velocity, and can deduce at once the number of atoms present in a given weight of any known kind of matter. In the light of these and similar direct deductions, based on a minimum amount of assumption, the physicists have, I think, some justification for their faith that they are building on the solid rock of fact, and not, as we are often so solemnly warned by some of our scientific brethren, on the shifting sands of imaginative hypothesis.

British Association for the Advancement of

WINNIPEG, 1909.

ADDRESS

TO THE

CHEMICAL SECTION

BY

PROFESSOR H. E. ARMSTRONG, PH.D., LL.D., F.R.S.,

PRESIDENT OF THE SECTION.

It is recorded that, on a certain occasion, after saying, 'I shall not often give arguments but frequently opinions—I trust, with courtesy and propriety, &c.,' a professor of world-wide reputation remarked, 'A man's opinions, look you, are generally of much more value than his arguments. These last are made by his brain and perhaps he does not believe the proposition they tend to prove—as is often the case with paid lawyers; but opinions are formed by our whole nature—brain, heart, instinct, brute life, everything all our experience has shaped for us by contact with the whole circle of our being.'

Of his many charming utterances at the breakfast table, I would select this as one of the most noteworthy and just withal. Chemists especially need to take both opinions and feelings into account, as well as arguments; to appreciate more fully, perhaps, than is now customary the need of cultivating and giving expression to that state of mind which is the main qualification of the expert—the state of mind which, let me insist, in the case of the chemist, is only to be acquired by constantly associating with and constantly handling substances in being and in the making, by constantly striving to become acquainted with their innermost nature and idiosyncrasies. It is safe to say that much of the subject matter of our science is not yet quantifiable and probably never will be; it is even easy to overrate the value of quantitative measurements, as the processes studied, more often than not, are involved operations that can be only with difficulty, if at all, resolved into their factors.

After an interval only a year short of a quarter of a century, it is my privilege again to occupy the chair of this section and that, too, under conditions of special significance. The British Association has never before sought to carry the banner of science so far west into British Dominions—never before was it so clear that the progress of humanity is linked with the progress of science by an indissoluble bond: science defined in a word being *knowledge*, not mere work nor mere lip knowledge, but system-

atised established knowledge, not assumed knowledge—although hypothesis often serves to guide inquiry and truth is arrived at only gradually and slowly by a series of rough approximations. Moreover, science is true knowledge of *every kind*—there is too often a tendency to give a narrow interpretation of the word. One reason probably why the term does not produce any proper effect upon the average British ear is that it is not an English word but a mere adaptation from the Latin—a language which apparently cannot be engrafted upon our Saxon tissues, although, perhaps, it may be that we have so little feeling for it because we have been allowed to learn so little else in our higher schools; monotony of diet ever favours diminutive growth. Germans, I always feel, enjoy a great advantage over us in possessing the popular word *Wissenschaft*—in calling science the *business of knowing*, the *business of gaining wisdom*, of *being wise*.

Coming as we do to Canada to advocate such a cause, to direct attention to the principles on which alone such a business can be learnt and conducted with profit—surely we may count on meeting with the support of the public at large: it is this we desire and claim—not merely the support of a few specialists; moreover, we do not ask for it in any way as a favour but practically demand it as a right—in no way, however, on personal grounds or with any display of arrogance but because we are persuaded that our message is of such infinite importance to the well-being of the community that it is our clear duty to make it of avail. Here it is that opinion, not argument, must count; the language of science is and must remain, in many ways, a strange one to the public; we must therefore ask that they entrust us with their confidence and allow themselves to be guided by the experience we have gained; we must be as the prophets of old: regardless of consequences, we must insist on the overthrow of the idols which a narrow priesthood still attempts to force upon society. We need always remember that, as my good friend the Professor expresses it—‘Man is an idolater or symbol-worshipper by nature, which, of course, is no fault of his, but sooner or later all his local and temporary symbols must be ground to powder, like the golden calf—word-images as well as metal and wooden ones.’ It is, as he says, ‘Rough work, Iconoclasm—but the only way to get at Truth.’

Naturally I am constrained on the present occasion to take stock of the position of our science, to draw a comparison between the condition of affairs chemical when we met in Aberdeen in 1885 and their present state. No like period of human history has been more fruitful of advance; at the same time, no period illustrates more clearly the difficulties that lie in the path of progress—because of the innate conservatism proper to human nature.

It was my privilege in 1885 to discuss a variety of problems which then seemed to be of special importance in relation to the subject of Chemical Change, our main province of study. I find the same problems dominant now—still unsolved but yet nearer solution. The history of progress, of discovery, during the intervening period is wonderfully rich in incident—how rich perhaps few realise, as it is obscured by a mass of blinding detail. If I attempt to bring some of the scattered threads together and in so doing dare to paint a picture which here and there may be startling in its outlines and implications; if I venture to follow the example set by one who has appeared as an autocrat as well as a professor and sometimes give my naked opinions: it will, I trust, be understood that I do so conscious that the sketch I am presenting must be full of the faults to which all such attempts are subject.

In my previous address two very different topics were considered—the Educational Outlook and the Theory of Chemical Change. In dealing with

the former, I drew special attention to the need of creating an atmosphere of research in our colleges—then to the faulty curriculum of our schools and the need of introducing reforms into practically every branch of education, especially medical education. In the interval, considerable progress has been made by way of forming plans for the future, even a foundation stone or two has been put in place, although scarcely 'well and truly laid'—but the actual buildings are hardly marked out. In point of fact, the needs to which I called attention in 1885 are our present and now most urgent needs. But of this more subsequently.

In discussing chemical action, I commented on our failure to arrive at any understanding as to the conditions which determine the occurrence of chemical change—a failure all the more remarkable in view of the clearness of Faraday's early teaching. Basing my remarks on the thesis which he propounded in 1834 that the forces termed electricity and chemical affinity are one and the same, I discussed current views on electrolysis and arrived at a conclusion entirely adverse to the explanation put forward by Clausius that the conductivity of electrolytes was conditioned by the presence of a small proportion of separate ions; this was at a time when the views of Arrhenius were not yet spread abroad, although they had been communicated to the Swedish Academy; I knew of them only from Ostwald. In justice to the attitude of complete antagonism which I have always maintained towards the speculations of the Arrhenius-Ostwald school, I may point out here that in drawing attention to the views expressed by Arrhenius and Ostwald as to the correlation of chemical with electrolytic activity (and I was the first English writer who called attention to them), I took occasion to say: 'There cannot be a doubt that these investigations are of the very highest importance.' In the interval, Ostwald has charged his test-tubes with ink instead of with chemical agents and by means of a too facile pen has enticed chemists the world over into becoming adherents of the cult of ionic dissociation—a cult the advance of which may well be ranked with that of Christian science, so implicit has been the faith of its adherents in the doctrines laid down for them, so extreme and narrow the views of its advocates. At last, however, the criticism which has been far too long delayed is being brought to bear and the absurdity of not a few of the propositions which the faith entails is being made evident; it is to be hoped that we shall soon enter on a period in which commonsense will once more prevail; that ere long an agreement will be arrived at, both as to the conditions which determine it and as to the nature of chemical change in general. The lesson we shall have learnt is one of no slight import if it but teach us the ever-present need of questioning our grounds of belief, if it serve to bring home to us the danger of uncontrolled literary propagandism in science, if it but cause us always to be on our guard against the intrusion of authority and of dogmatism into our speculations.

Before attempting to deal with any of the problems which concerned us at Aberdeen I will first briefly pass the more salient features of advance in review. Few probably are aware how extraordinary is the command we now have of our subject. In 1885, in defending the tendency of chemists to devote themselves to the chemistry of carbon, I could speak of the great outcome of their labours as being the establishment of the doctrine of structure. Everything that has happened in the interval is in support of this contention. It is interesting that in a recent lecture¹ on the Physical

¹ The Wilde Lecture, 1908. By Professor Larmor, Sec. R. S., *Manchester Literary and Philosophical Society Memoirs*.

Aspect of the Atomic Theory, the most prominent living exponent of physical theories has given a not unwelcome recognition of our right-mindedness in saying: 'As time goes on it becomes increasingly difficult to resist the direct evidence for the simple view that, in many cases, chemical combination is not so much a fusion or intermingling of the combining atomic structures as rather an arrangement of them alongside one another under steady cohesive affinity, the properties of each being somewhat modified, though not essentially, by the attachment of the others; and that the space formulæ of chemistry have more than analogical significance.' And again in the following passage, in which a far-reaching confession is made: 'The aim of structural chemistry must go much deeper (than dynamical methods of treatment); and we have found it difficult, on the physical evidence, to gainsay the conclusion that the molecular architecture represented by stereo-chemical formulæ has a significance which passes beyond merely analogical representation and that our dynamical views must so far as possible be adapted to it.' The remark made by Helmholtz in one of his letters, 'that organic chemistry progresses steadily but in a manner which, from the physical standpoint, appears not to be quite rational,' must be regarded as little more than a confession that he was out of his depth. When properly understood, nothing could be more rational and logical than the way in which our theory of structure has been gradually built up on an impregnable basis of fact, with the aid of the very simple conceptions of valency postulated by Frankland and Kekulé. Our security lies in the fact that the postulates of our theory have been tested in an almost infinite variety of cases and never found wanting; this is not to say they are applicable in all cases but merely that whenever we are in a position to apply them we can do so without hesitation. Larmor refers to the habit of physicists of taking comfort in Helmholtz's remark; it will be well if instead they make themselves acquainted with our methods and with the results we have won, with a minimum of speculative effort, by the cultivation of an instinct or sense of feeling which experience shows to be an effective guide to action. Now that physical inquiry is largely chemical, now that physicists are regular excursionists into our territory, it is essential that our methods and our criteria should be understood by them. I make this remark advisedly, as it appears to me that of late years, while affecting almost to dictate a policy to us, physicists have taken less and less pains to make themselves acquainted with the subject-matter of chemistry and especially with our methods of arriving at the root-conceptions of structure and of properties as conditioned by structure. It is a serious matter that chemistry should be so neglected by physicists and that the votaries of the two sciences should be brought so little into communion.

The central luminary of our system, let me insist, is the element carbon. The constancy of this element, the firmness of its affections and affinities, distinguishes it from all others. It is only when its attributes are understood that it is possible to frame any proper picture of the possibilities which lie before us, of the place of our science in the Cosmos. But, as Longfellow sings of the sea in his poem 'The Secret of the Sea,' 'Only those who brave its dangers comprehend its mystery'—only those who are truly conversant with the root conceptions of organic chemistry are in a position to attempt the interpretation of the problems of our science as a whole or even to understand the framework upon which it is built up. And yet we continue to withhold the knowledge of the properties of carbon from students until a late period of their development; indeed, when I insisted recently that organic and inorganic chemistry should be taught as one subject to medical

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students,¹ I was told that it could not be; that the attempt had been made with disastrous consequences. I trust that ere long the futility of such an attitude will be generally realised.

It is remarkable how much our conceptions are now guided by geometrical considerations. The development by van't Hoff of the Pasteur hypothesis of geometrical asymmetry has been attended with far-reaching consequences during the period under review, the completeness with which the fundamental properties of the carbon atom are symbolised by a regular tetrahedron being altogether astounding.

Our present conception is that the carbon atom has tetrahedral properties in the sense that it has four affinities which operate practically in the direction of the four radii proceeding from the centre towards the four solid angles of a regular tetrahedron.

More than analogical significance—to use Larmor's expression—must be accorded to this symbol on account of its remarkable accordance with the facts generally, whether derived from the study of asymmetric optically active substances or from observation of the activity of ring structures of various degrees of complexity. Nothing is more surprising than the completeness with which the vast array of facts included in organic chemistry may be ordered by reference to the tetrahedral model. In the future, when our civilisation is gone the way of all civilisations and strangers dig on the sites of our ruined cities for signs of our life, they will find the tetrahedron and the benzene hexagon among the mystic symbols which they have difficulty in interpreting; if, like the ancient Egyptians, we made our tombs records of our wisdom, such symbols would long since have acquired sacred significance and the public would probably have learnt to regard them with awe and to respect them as totems. Chemists might at least wear them on aprons in imitation of the Freemasons; perhaps no two other symbols have so great a significance—they reach into life itself.

It would seem that carbon has properties which are altogether special, the influence which it exercises upon other elements in depriving them of their activity is so remarkable. In their recent discussion of the relation of crystalline form to structure, in which valency is represented as a function of the volume sphere of influence exercised by an element, Barlow and Pope arrive at the remarkable conclusion that carbon is probably the only element the atom of which has a volume sphere of influence four times that of the hydrogen atom; although it combines with four atoms of hydrogen, silicon apparently has only half the volume sphere of influence of carbon. This may, in a measure, account for the very great dissimilarity in behaviour of the two elements, which is most pronounced in their oxides, the single atom of carbon all but dominating two atoms of oxygen in carbon dioxide (which is consequently gaseous), whilst the atom of silicon in silicon dioxide in no way eclipses the two atoms with which it is associated but leaves both charged with residual affinity which enables them to form complex collocations of remarkable fixity in the fire. At bottom the differences between organic and inorganic nature are to be regarded as very largely the expression of this difference. Ropes of sand are proverbially treacherous; yet without sand, if silica had been a gaseous substance, our world might have worn a strangely different aspect.²

¹ 'The Reform of the Medical Curriculum.'—*Science Progress*, January and April 1907.

² The solid model of silica which Barlow and Pope have constructed has very remarkable attributes, in that the oxygen atoms appear to be uniformly related and in intercommunication throughout its mass: so that a mass of silica, whatever its size, may almost be regarded as a single molecular complex. A similar view may be taken of plastic metals such as those of the platinum group,

The mineral world apparently owes its rigidity to the fact that the metals and certain other elements are so imperfectly capable of dominating oxygen that oxides generally polymerise with great readiness, giving rise to substances which do not even fuse easily. The organic, on the other hand, appears to be plastic by reason of the close approach to neutrality which is conditioned by association with carbon.

Nothing is more striking than the remarkable diversity of properties manifest both in the materials which at present we are content to call elements and in the compounds formed by their interaction; the range of variation met with in the case of the compounds of carbon with hydrogen and oxygen alone is almost infinite. We are almost compelled to attribute this diversity more to differences in the complexity and structure of the molecules than to differences in their material composition. The chemist, of necessity, must be a dreamer, knowing as he does that things are not as they seem to be. But this is not sufficiently remembered; indeed, students are systematically trained up in an atmosphere of pretence. The beginner is allowed to regard *elementary oxygen*, for example, as a colourless gas, which is generally harmless until things are presented to it in a more or less heated condition, whereat it takes umbrage and burns them up. He would regard elementary carbon as a soft black substance, which if smeared on the face of the white man makes him look like a nigger, were it not that he also learns that at times it is the hardest and whitest substance known; of organic chemistry, which alone can give him honest ideas of carbon, he is not allowed to hear as I have said. The sting of awakening conscience is salved by the introduction of a long Greek word when he is told that the two substances, soot and diamond, are *allotropic* forms of the element carbon; nevertheless he regards them both as elementary carbon. Gradually, perhaps, he awakens to a sense of the wrong that he has suffered at the hands of his teachers, as he realises that from no one substance can he gather what the properties of an element are, that after all the elementary substance is but an ideal—in other words, a mere concept. If appreciative, he then learns to think of the blandness of water, the sweetness of sugar, the sourness of vinegar, the causticity of soda, indeed every distinctive property of every known oxygen compound as more or less a property of, more or less conditioned by, the element oxygen; he is brought back, in fact, to the position from which Lavoisier started, as he realises that the oxygen *gas* which he inhales is not elementary oxygen;

gold, silver and copper. Whether when rendered brittle by association with small amounts of impurity these are resolved into simpler molecular complexes or whether the molecules merely become separated by substances which promote discontinuity and brittleness, it is impossible to say at present. The cause of hardness in mineral materials is, however, a question of no slight interest and importance. The property is strikingly exemplified in the diamond. It is difficult to understand the intense hardness of this material, on the assumption that the diamond is composed of paraffinoid carbon—that is to say, carbon with all its affinities satisfied. At present we appear to have no clue to the manner in which affinity acts in promoting the formation of such solids. But it is obvious that all solids are possessed of some degree of 'surface affinity,' as they not only grow when placed in solutions but determine the separation of solid from a solution at a degree of saturation which is often considerably below that at which the solution is actually saturated with the substance; and such surface affinity, moreover, is selective, as the determinative effect is exercised only upon the substance itself or substances isomorphous with it—although exception must be made in favour of water, which all surfaces appear to attract. Sir James Dewar's observations on the condensation of gases by charcoal at low temperatures afford most striking illustrations of surface affinity.

he can then perhaps appreciate the wonderful acumen which this greatest of chemical philosophers displayed when he wrote: 'Nous avons donné à la base de la portion respirable de l'air le nom d'oxygène en le derivant de deux mots grecs ὀξύς, acide, γένεσθαι, j'engendre, parce qu'en effet une des propriétés les plus générales de cette base est de former des acides en se combinant avec la plupart des substances. Nous appellerons donc gaz oxygène la reunion de cette base avec le calorique.' We have allowed a century to pass without recognising the wonderfully accurate powers of prevision displayed by Lavoisier; what is worse, we have been so far led astray that instead of regarding oxygen as the characteristic and attractive element in acids, hydrogen has been allowed to usurp the position: the extent to which the cult of the hydrogen ion now dominates the text-books is well known; in days to come, when the history of our times is written, it will be referred to as a remarkable example of chemical shortsightedness.

Names are needed for the elements which would serve to distinguish the ideal elementary substances from the forms in which they are known to us. No more appropriate name than oxygen could possibly be selected for the fundamental material; if the *gen* terminal could be applied to elementary materials generally, it would be an advantage; it would not be easy, however, if this were done, to devise an appropriate separate name applicable to the active constituent of air.¹

¹ In naming the inert gas in air, which he ultimately termed *azotic gas*, having proposed the name *acote* for the element, Lavoisier had in view as alternatives the terms *alcaligen* and *nitrogen*. As there was no proof that the element was a constituent of alkalies other than ammonia, he rejected the former name on the ground that it might convey too broad an impression; in course of time the latter is become the popular name, except in France, where motives of piety have prevailed; but the French practice has been justified by the universal use of the term *azo* in connexion with many nitrogen derivatives.

Had Lavoisier realised that the alkalies and basic oxides generally owe their basicity to oxygen as much as acids and acidic oxides generally owe their acidity to oxygen—the one being oxygen tempered by metal, the other oxygen tempered by non-metal—as the number of basic oxides far outweighs the number of acidic oxides, he might well have chosen the name *alcaligen* rather than oxygen. The choice he made was a particularly happy one and striking evidence of his genius and sense of euphony—for oxygen is *par excellence* the acid-forming element and is most truly called *sour-stuff*, the stuff of which sour things are made—for whatever the properties of the initial oxide of a series, as the proportion of oxygen is increased, the acidic qualities are invariably strengthened.

The choice of a terminal connoting the elementary radicles which would be applicable generally and also acceptable is very difficult. If usage do not forbid change, probably our ears will decline to allow us to be systematic. The terminal *gen* is not applicable to many present names. In the interest of euphony, exception may be taken to the adoption of *ion* as a final syllable. In English ears most of the words with this ending have an ugly sound if pronounced so as to make it significant; moreover, our object is to secure a term which is applicable to the elementary material, whatever its state; the term *ion* is suggestive of a particular state—a state of chemical activity; and at present there is no agreement as to the nature of an ion. The terms atom, radicle (simple and compound), ion and molecule now all have their separate meaning and value and are indispensable.

The only terminal which seems in any way likely to be generally satisfactory in use is the terminal *yl*, which is already applied to organic radicles; its use might well be extended to radicles generally.

I may add here that it is unfortunate that certain disturbers of the peace have advocated of late a reversion to the spelling *radical*, mainly on the ground that the term is of French origin and was thus spelt originally. But there is no reason to give a French spelling to a word when it becomes English; and the genius of our language is against the proposal, apart from the fact that it introduces unnecessary confusion. We make clear distinction between *principal* and *principle*; it is most desirable, in like manner, to distinguish between *radical* and

In 1885 I closed my address with a reference to the structure of the elements which implied that their behaviour was that of compound substances; the feeling that this is the case has long been general among chemists. Our present attitude towards this problem is a curious one and not altogether satisfactory—it is impossible to deny that we have somewhat lost sense of proportion, even if our methods have not savoured of the unscientific. The discovery of radium appears to have upset our balance—we have been carried away by the altogether mysterious and unprecedented behaviour of this weird and wondrous substance. But may we not ask: Is radium an element? Has it not been too generally, too hastily assumed that it is? Little as we know of it, does not its behaviour straightway out-class it as an element? Surely it does! Is not the established fact that an emanation proceeds from it, which in turn decomposes and gives rise to helium, a proof of its compound nature? Again, is the evidence of such a character as to justify us in asserting that uranium is the parent of radium? If it be such, must not uranium also disappear from the list of elements; must it not indeed be removed on the ground that it gives rise to uranium without any reference to its supposed relationship to radium?

radicle; the latter is in harmony with *particle* and *participle* and being suggestive of a rootlet, it is eminently significant. *Radical* is almost misleading, as a radical in these days is apparently one whose tendency is to go to extremes while contemplating the surface only instead of going to the root of things.

It would be to the advantage of students also if we were far more systematic in our use of formulæ as well as in matters of nomenclature. At present no proper distinction is made between empirical and molecular formulæ in the case of the elements. Notwithstanding that we acknowledge our indebtedness to Avogadro and to Canizzaro his prophet, it is still not unusual to find gaseous hydrogen represented by the symbol H and gaseous oxygen by the symbol O; the text-books generally pay little heed to such matters. We are far too careless of consequences in our teaching and do not sufficiently appreciate the value of system and ritual. If it were made the practice to represent molecular formulæ in some special manner—by Clarendon or thick type, for example—attention would then be called to the fact that in the majority of cases the substances used are of unknown molecular composition.

If a student see sodium chloride always represented as NaCl or, what is worse, in accordance with a growing evil custom, learn to speak of it as *En-ay-see-el*, it becomes difficult to persuade him that probably such a formula is a misleading expression—at all events, in no way the expression of known fact. Nothing could be worse than the tendency which is coming over us to speak of substances in terms of their formulæ instead of by name. It is difficult to understand what can be gained by referring, for example, to carbon dioxide as *Cee-oh-too*. Such vulgarisms and also the substitution of formulæ for written or printed names should be discountenanced on every possible occasion.

The reproach is not unfrequently levelled at us that scientific workers lack literary style and that they do not take sufficient pains in describing their work clearly and concisely—too often with justice. At all events, as the complaint has been made from the Chair at several recent anniversary meetings at the Royal Society, some notice should be taken of it.¹

Solecisms are only too abundant in our literature. It is sheer carelessness to speak of compounds 'adding' this or that, instead of saying that they combine with it; the statement that a substance 'analyses' is inexcusable. The use of such expressions is proof that no thought has been exercised in writing.

An old writer has expressed an eternal truth in saying: 'All soche Authors as be fullest of good matter and right judgement in doctrine be likewise always most proper in wordes, most apte in sentence, most plain and pure in uttering the same.'

¹ Complaint is made even in Germany. Compare von Lippmann: 'Ueber den Stil in den deutschen chemischen Zeitschriften,' *Chemiker-Zeitung*, May 6, 1909, p. 489.

The answers given to such questions must depend on our definition of an element. At present we seem to be without one.

The conception that the break-down of radium is spontaneous and apart from all external impulse or control is also one which should be received with caution. There is reason to suppose that in all ordinary cases in which compounds undergo decomposition spontaneously, the decomposition is conditioned by an impurity; the effect, moreover, is usually cumulative. This is true of highly explosive substances, such as chloride of nitrogen and gun-cotton, for example. It might be supposed that something similar would happen in the case of radium—but apparently such is not the case; it is assumed that occasionally a molecule explodes spontaneously, not only without being incited thereto but also without in any way affecting its neighbours.

The alternative explanation that radium in some way acts as a receiver, transforming energy from some external source to which ordinary substances fail to respond and being thereby stimulated to decompose, is at present out of favour, although perhaps more in accordance with its peculiar behaviour.¹

The liberation of helium as a product of radio-active change is in itself a significant fact, in view of the possibility that helium may be an element of intense activity. Nothing in connexion with the problem is more surprising, however, than the apparent production, in course of time, of a whole series of degradation products which differ greatly in stability—such behaviour is entirely without precedent and not at all becoming in elements.

No such remarkable and inspiring problem has ever before been offered for solution. We can only wonder at the results and admire the genius which some have displayed in interpreting them, Rutherford in particular. Yet outsiders may well hold judgment in suspense for the present: whilst it is permitted to workers to make use of hypothesis in every possible way in extending inquiry, the public are in no wise called upon to accept such hypothesis as fact.

But apart from the suggestion that elements may give rise to others spontaneously, we have been entertained of late with stories of elements being converted into others under the influence of the energy let loose by the breakdown of radium. There is reason, however, to suppose that the powers of radium may have been greatly overpainted; energy of almost any degree of intensity in the form of high tension electricity is now at our disposal and the effect which radium produces on living tissues, glass, &c.,

¹ I may here put on record the opinion Lord Kelvin expressed on this question in a letter to me dated September 13, 1906:—

‘Ever since, nearly four years ago, we heard of the hundred calories per hour given out by radium, I have had on my mind the question of some possible mechanism such as that which you suggest by which energy from surrounding matter (far or near) could automatically come into radium to supply the energy of the heat which it gives out. The more I think of the question the less I see of that possibility. At present I can see nothing else than that the energy given out is taken from a previously existing store of potential energy of repulsive force between separable constituents of radium.’

‘The “disintegration of the radium atom” is wantonly nonsensical. It is nonsense very misleading and mystifying to the general public, because, if what is at present called radium can be broken into parts, it is not an atom.’

‘“Energy of an atom” implies a thorough misunderstanding of the meaning of the word energy, which is capacity for doing work.’

‘I admire most sincerely and highly the energy of the workers in Radioactivity and the splendid experimental results which they have already got by resourceful and inventive experimental skill and laborious devotion. I feel sure that as things are going on we shall rapidly learn more and more of the real truth about radium.’

is of the same character as that effected by the Röntgen ray discharge, the only difference being that the effect is produced somewhat more rapidly; it is not to be imagined, therefore, that the discovery of radium has put any very novel intensity of power into our hands.

It is right that the public should understand that the statements published have been based on preliminary observations which lack verification, such as would never have been divulged in days gone by when a sterner sense of duty pervaded our ranks. Until the elementary nature of radium has been placed beyond question, we must hold judgment in suspense even as to the possibility of 'elements' undergoing decomposition 'spontaneously'; at present, the possibility of elements being decomposed or transmuted by means of radium need not be entertained until evidence is forthcoming of a more convincing character than that with which we have been favoured.

We have been living in a time of sensational discovery—in a period when advertisement is favoured and the desire for notoriety rampant. Unhappily that caution which appeared to be regarded as a priceless prerogative of the scientific worker in the earlier part of the last century, of which Faraday was so pre-eminent an exponent, is no longer our recognised watchword. I fear I am one of those who are old-fashioned enough to lament the way in which our claim to be safe and honest guides of public opinion is being endangered—who lament the manner in which the reputation of scientific workers is likely to be besmirched if we do not see the evil of our ways and mend them. It is impossible to avoid noticing how the cancer grows—how the example is spreading among the younger men and loose habits of work and thought are being engendered. I know that not a few who have laboured steadfastly and seriously in an old-fashioned, exact and painstaking way, have been deeply hurt by the manner in which their efforts fail to meet with encouragement whilst those who have thrown caution to the winds are favoured; the feeling is beginning to arise that only sensational discovery is appreciated by the public. We need to return to the healthy times when fearless and frank criticism of all work was deemed desirable. We cannot substantiate the claims that are made on behalf of science unless our own attitude be above reproach—unless we are both logical and philosophical—unless we remain the sternest advocates of truth in its most rigid form.

I pass to the consideration of the classification of the elements. The recognition of certain properties, the association of certain ideals with the several elements, is a necessary step in classifying the elements in accordance with Mendelejeff's great generalisation—or rather it may be said to be both involved in and an outcome of Mendelejeff's conception.

Until recently our difficulty was to understand the relationship of the metallic and the non-metallic elements; now we are confronted with another problem—that of the existence of inert 'paraffinoid' elements. It is commonly assumed that these are monatomic but the evidence on which this assumption is based is absolutely unconvincing and would be generally admitted to be so were we in the habit of looking before we leapt to conclusions. Assuming that the elements are compounds, the formation of inert compounds does not appear to be out of place, in view of the existence of practically inert hydrocarbons. But on the other hand, in view of the properties of nitrogen, which is one of the most active of substances in the monatomic state, although an inert gas in the diatomic condition, it may well be that the inertness of helium and the other members of the argon group is also simulated. Sir James Dewar's observations have shown that helium and charcoal have no inconsiderable affinity at the boiling point of the former, which is within five degrees of the absolute zero, the molecular

heat of absorption (apart from that due to liquefaction) of helium at that temperature being apparently as high as about sixty calories. The proof lies has also given that helium alone does not convey an electric discharge is also of significance since the passage of a discharge through it under ordinary conditions is an indication that it can be included with other substances in a conducting system. Such evidence as there is therefore points to the elements under discussion being different from the others only in the degree of stability of their molecules.

Of late years the difficulty of classifying the elements has been increased rather than diminished, not merely because of the discovery of the inert gases but also on account of the apparent impossibility of ordering the position of an element such as tellurium in accordance with its atomic weight. There appears to be little room left for doubt that the value cannot be far removed from that of iodine; it should be considerably lower. It may be pointed out that the accepted value of selenium is closer to that of bromine than would be expected if a relationship were maintained corresponding to that between chlorine and sulphur. It would seem that Mendeleeff's original conception of the elements as a simple series in which the properties are periodic functions of the atomic weights must be abandoned in favour of some more comprehensive scheme. From the chemist's point of view, it is impossible to abandon the guiding principle underlying the arrangement in family groups, which dates back to Dumas; perhaps insufficient attention has been paid in the past to the maintenance of this principle.

Taking into account this principle, it is impossible to arrange a long series of elements such as the rare earths continuously in order of atomic weight, as they would be brought into every family in the table by such a procedure; the difficulty has been got over by Brauner, who has proposed to arrange a large number of the rare earths in a single vertical series under barium. Biltz has made a similar proposal.

The principle had been advocated by me previously in an article written for the 'Encyclopædia Britannica.'¹

In the arrangement I have proposed, it is not only assumed that there may be as many as sixteen vertical series of elements of which the elements from hydrogen to oxygen are initial terms, some series being at present unrepresented, it is also suggested that groups of elements occur in perhaps four of these series, numbers 4, 8, 12 and 16, the largest being that of the so-called rare earths in series 8.

The principle which is assumed to be in operation is that which is so clearly manifest in the case of hydrocarbons: successive vertical series of elements correspond to successive isologous series of homologous hydrocarbons. In the case of the hydrocarbons, the passage from one isologous series to another often takes place from a term several places removed from the origin of the series—for example, from benzene, C_6H_6 , which may be regarded as primarily a derivative of hexane to naphthalene, $C_{10}H_8$, which is not an immediate derivative of benzene but of butylbenzene. It is conceivable that at the genesis of the elements a process was at work corresponding to that by which a hydrocarbon such as naphthalene is derived from benzene and by which the former then serves in turn as the point of departure for more complex hydrocarbons of other series. There is no reason, from this point of view, why progression should not take place along a particular line and that terms should exist in a series through which this line passes but below it—for example, that antimony and iodine may bear a direct linear relationship but that tellurium, instead of being the element in the progression series in the oxygen group, is a homologue of greater weight. The

¹ Cf. *Roy. Soc. Proc.*, 1902, vol. lxx. pp. 86-94.

same view may be taken of selenium. In this way, it would be possible to maintain selenium and tellurium in the oxygen-sulphur series, from which they cannot well be separated, whilst retaining Mendelejeff's conception of a genetic relationship along the series. The only departure involved is in assuming that instead of forming a single linear series ascending regularly in spiral progression—a series which can, as it were, be strung on a single spirally wound cord—the elements closely simulate a series of homologous isologous hydrocarbons. From this point of view, it is easy also to understand that some vertical series are unrepresented.

In discussing the chief attributes of the elements none is so difficult to deal with as that of valency, using the term in the broadest possible sense, not merely as indicative of the number of units of affinity but as including the, at present, all but incomprehensible problems of residual affinity and elementary character. I discussed the subject somewhat fully in my former address, dwelling especially on the properties of negative elements and their power of acting as linking agents; this view has met with ample confirmation in the interval and will, I believe, be found to be of wide application in the future. I have already referred to the manner in which it is exemplified by silica.

The greatest advance in the discussion of the problems of valency in recent years is that made by Barlow and Pope, as their method of treatment is one which applies to solid substances—the correlation of structure with crystalline form which it effects promises to be of far-reaching importance.

Apart from hydrogen, carbon is the one element of certain character, always acting as a tetrad—its affinities may be only incompletely satisfied but they are always exercised, it may be supposed, even in ethenoid and similar compounds; carbon monoxide apparently is the only exception to this rule, its relative inactivity being one of the most puzzling enigmas of our science, especially as the oxide becomes one of the most active of known substances when only two atoms of hydrogen are added to it. Most other elements (non-metallic) seem to vary in valency, the valency beyond a certain minimum being dependent on the nature of the association. Of late years, attention has been drawn in particular to the quadrivalency of oxygen in many of its compounds.

The quadrivalency of sulphur in substances such as trimethylsulphonium iodide, Me_3SI , having been proved to demonstration by the production of optically active compounds of this type (Pope and Peachy), it can no longer be supposed that in such cases we are dealing with compounds in which the negative constituents of the parent molecules are conjoined, e.g., $\text{MeI} : \text{SMe}_2$. And yet we must contemplate the existence of such compounds as possible—in the case of nitrogen, for example, as ammonia must be supposed to form the compound $\text{NH}_3 : \text{OH}_2$ in preference to the hydroxide $\text{NH}_4 \cdot \text{OH}$, the latter being only a very minor constituent, the former the major component of the aqueous solution of the gas; hydrogen chloride, on the other hand, appears only to afford one product with ammonia, viz. $\text{NH}_4 \cdot \text{Cl}$. The existence of such differences affords clear proof in the case of the nonmetallic elements other than carbon that valency is not merely a variable but also a reciprocal or dependent function.

There is no reason to suppose that hydrogen ever acts otherwise than as a simple monad; and the behaviour of the alkalis and alkaline earths in salts would seem to justify the conclusion that they have no tendency to vary in valency, were it not for the existence of well-defined non-volatile hydrides of these metals which are clearly substances of some degree of molecular complexity. Such compounds are illustrations of the difficulties which surround the subject. It has long been clear that the exhibition of

the higher valency by an element is a process of a different order from that manifest when it exerts only its lower proper valency measured in terms of positive radicles such as H or C_nH_{2n-1} radicles. What that difference is we are not able at present to decide—carbon (together with silicon) differs from almost all other elements especially in combining with hydrogen and analogous radicles to the extent of its maximum valency.

The proposition I made in 1888 (*Phil. Mag.*, Series V., 25, 21) that the valency lines should, in some cases, be represented as passing through the atom, so that each is capable of acting in two directions, is the only consistent mode of expressing varying valency which has been devised, the only one, moreover, by which attention is drawn to the great difference.

In many cases probably there has been a tendency to exaggerate the valency value—in the case of chlorine, for example, in assuming that it functions as a heptad in the perchlorates. In this and many other instances, it suffices to assume that the chlorine and oxygen atoms are united in a closed ring, the chlorine functioning as a triad. Some such explanation will doubtless be given of the structure of the metallic ammonias and similar compounds. The co-ordination values introduced by Werner serve only to establish certain empirical relationships and are useful for the purposes of classification. The perhaps more rational plan of dealing with such compounds suggested by Abegg has a similar value.

It is to the advantage of the hypothesis formulated by Barlow and Pope that the elements are represented as of constant valency in so far as their relative volume spheres of influence are concerned—the compound in which the higher valency is manifest being derived from that of lower valency by the opening out of the close packed arrangement and the insertion of certain new elements; but the fact that in such cases the volume is altered not in one direction alone in the crystalline structure but proportionately in all directions would seem to show that the volume sphere of atomic influence does actually change; the change is one, however, which affects all the atoms in the complex proportionately.

At present, unfortunately, our methods of treating the problems of valency are such that we cannot in any way give expression to the energy side of the phenomena.

Of late there has been talk of electrons in this connexion but what is said is little more than superficial paraphrase, in the advanced scientific slang of the day, of the ideas which have long been current. When, following Odling, we represent valency by dashes written after the elementary symbol, we give clear expression by means of a simple convention to certain ideas that are well understood by all among us who are versed in the facts; to speak of electrons and use dots instead of dashes may serve to mislead the unwary, who hang on the lips of authority, into a belief that we have arrived at an explanation of the phenomena but those who know that we have reached only the let-it-be-granted stage and who feel that the electron is possibly but a figment of the imagination,¹ will remain satisfied with a symbolic system which has served us so long and so well as a means of giving simple expression to facts which we do not pretend to explain. Not a few of us who listened to the discussion of the nature of the atom at Leicester could not but feel that the physicists knew nothing of its structure and

¹ In my opinion the experimental evidence is in no way satisfactory. It appears to me to be desirable that in studying the phenomena of electric discharge in gases and especially in vapours of complex substances, the horrible pitfalls should be taken into account with which the field of work is studded; unless every precaution to secure purity—precautions such as Baker and Dewar have taught us to use—be taken at every step, the conclusions based on all such observations must be open to grave doubt.

were wildly waving hands in the air in the endeavour to grasp at an interpretation which would permit of mathematical interpretation being given to the facts. Until the credentials of the electron are placed on a higher plane of practical politics, until they are placed on a practical plane, we may well rest content with our present condition and admit frankly that our knowledge is insufficient to enable us even to venture on an explanation of valency.

In 1885 and again in 1888, I ventured to call in question the interpretation of valency which Helmholtz had given in the Faraday lecture in 1881. On the present occasion, I would insist still more emphatically on the insufficiency of the atomic charge hypothesis; especially that it affords no satisfactory explanation of variable valency and of those fine shades of difference which are manifest, especially in the case of nitrogen, when the radicle attached to the dominant element is varied. In 1885 I discussed this question with reference to the nature of electrolytes and questioned the conclusion Helmholtz arrived at that electrolytes belong to the class of typical compounds the constituents of which are united by atomic affinities, not to the class of molecular aggregates. The opinion I then ventured to give was as follows:—

'The current belief among physicists would appear to be that primarily the dissolved electrolyte—the acid or the salt—is decomposed almost exclusively. We are commonly told that sulphuric acid is added to water to *make it conduct* but the chemist desires to know why the solution becomes conducting. It may be that in all cases the "typical compound" is the actual electrolyte—i.e. the body decomposed by the electric current—but *the action only takes place when the typical compounds are conjoined and form the molecular aggregate*, for it is an undoubted fact that HCl and H_2SO_4 dissolve in water, forming "hydrates." This production of an "electrolytical system" from dielectrics is, I venture to think, the important question for chemists to consider. I do not believe that we shall be able to state the exact conditions under which chemical change will take place until a satisfactory solution has been found.'

The position is not very different now. Although the propagation of the ionic dissociation cult has assumed the form of a fine art, we are still as far as ever from agreement as to the nature of chemical change; the speculation has not helped us in the least to clarify our ideas; at most we learn that interactions are between ions and even these, as a rule, are supposed to remain apart until they enter into the solid state. Throughout all these years I have never varied my opinion that the dissociation hypothesis is incompatible with the facts. On more than one occasion I have stated definite reasons which induce me to deny its usefulness¹ and these arguments have never been met; in fact, there has been little but a conspiracy of silence on the part of the upholders of the creed.

A large amount of work bearing on the subject has been done, chiefly by H. Brereton Baker. Strangely enough, no proper notice of his results has been taken outside England and even there the importance of the observations has not been sufficiently appreciated. Perhaps the most remarkable feature in the situation is that Baker himself scarcely seems to be alive to the meaning of the evidence which he has supplied; the attitude which he has displayed in his recent Wilde lecture can only be described as halting. Baker has shown, in case after case, that the occurrence of change is dependent on the presence of moisture, his greatest feat perhaps being the observation that it is possible not only to prepare nitrous

¹ Compare *Chem. Soc. Trans.* 1895, 1122; *Roy. Soc. Proc.* 1886, 40, 268; 1902, 70, 99; 1903, 72, 258; 1904, 73, 537; 1906, 78, 264; 1907, 79, 586; 1908, 81, 80; *Science Progress*, April 1909.

anhydride in the solid and liquid states but to volatilise it unchanged if only water be excluded.

I venture to think there is only one point of view from which the problem of chemical change can be approached, that, namely, which we owe to Faraday—to which hitherto justice has in no way been done—on which I dwelt persistently in my previous address: that the forces termed chemical affinity and electricity are one and the same. In every case of chemical change there is a coincident electrical change, an electric flux; on the other hand, every case of electrical change is accompanied by chemical change, some alteration in molecular configuration is effected; the force of chemical affinity is in some way disturbed by a momentary displacement of the molecules when a current passes through a conductor. Such being the case, the conditions determinative of chemical change can only be those which permit of an electric flux. Two substances in apposition do not give rise to a current; at least three are required to determine a slope of potential. Chemical change can only take place if one of the three be an electrolyte. In all cases apparently the chemical change supervenes upon the electrical, the electrolyte being resolved into its ions, one of which at least combines coincidentally with the adjacent electrode. Apparently these considerations are applicable to changes generally. And it should be added that, according to this view, the catalyst actually determines the occurrence of change.

The only other criterion which it is necessary to apply in order to decide whether change be possible in any given case is to consider if the change contemplated be one involving development of energy. It is important to remember also that a change which could not otherwise take place becomes possible when a suitable depolariser is introduced into the circuit.

The evidence that similar considerations apply to the gaseous and the liquid states cannot well be gainsaid. Before framing a theory of chemical change it is therefore necessary to formulate a definition of an electrolyte. It is doubtful if any single substance be an electrolyte; the conductivity of fused salts may well be and probably is conditioned by some admixture. Aqueous solutions of alkalis, acids and salts without exception are electrolytes. *Everything points to the fact that in such solutions the solvent and solute act reciprocally; the contention that the solute alone is active cannot be justified.* As water is altogether peculiar in its activity as a solvent and is a solvent which gives rise to conducting solutions, an explanation of its efficiency must be sought in its own special and peculiar properties.

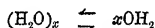
Since 1886 this conclusion has been impressed upon me with indisputable force and I have frequently ascribed the effect produced by the one constituent upon the other in a solution to the residual affinity of the negative elements in the two compounds which act reciprocally. It was only recently, however, that I saw my way to postulate a complete theory which would serve to account for the properties of solutions and generally that I realised how the reciprocal effect might be produced.

I would substitute for the misleading conception that liquids are comparable in their behaviour with gases the idea that the liquid state is one in which the residual affinity of the negative elements in particular always comes into play and causes the formation of molecular aggregates of various degrees of complexity; moreover, that the alteration in the properties of any given solvent by the dissolution in it of another substance is largely and, in some cases, mainly due to a disturbance of the equilibrium natural to the solvent by an alteration in the proportion in which the several aggregates are present. The alteration in some particular property pro-

duced in a *given mass of the solvent* may, from this point of view, be taken as the measure of the activity of a substance, just as the alteration in the pressure of a particular volume is taken as the measure of the alteration produced in a gas. In the case of non-electrolytes, if only a small amount of the solvent be withdrawn by combination with the solute, the alterations may be regarded as almost entirely due to the 'mechanical' interference of the substance introduced, opportunity being given for the simpler, more attractive molecules of the solvent to exist in greater proportion because of the diminution of the chance of reuniting which is conditioned by the presence of practically inert molecules of another kind; if a more or less considerable amount of the solvent become associated with the solute the conditions become more complex but similar considerations apply. From such a point of view a liquid is rendered more active by the addition of any soluble substance. Its vapour pressure is therefore diminished; the internal 'osmotic' stresses are raised; its freezing-point is lowered.

Although it is generally admitted that water is not a uniform substance but a mixture of units of different degrees of molecular complexity, the degree of complexity and the variety of forms is probably underestimated and little or no attention has been paid to the extent to which alterations produced by dissolving substances in it may be the outcome and expression of changes in the water itself. The attempt to extend the 'laws' which are applicable to the gaseous state to liquids has led us away from the truth by narrowing our conceptions. If the contention be justifiable that the alterations attending dissolution are very largely alterations in the character of the water, attention has been directed of late far too exclusively to the dissolved substance.

To give emphasis to the view, I have advocated¹ the restriction of the name *water* to the liquid mixture and have proposed that the simple molecule represented by the symbol OH_2 be termed *Hydrone*. The generalised expression



may be considered to be representative of the state of equilibrium in water—that is to say, of the character of the change which it undergoes when the conditions are varied either physically or by dissolving substances in it—in the sense that it pictures the resolution of the more complex into simpler forms and *vice versa*, without taking into account the variety of molecular forms ($x, x^1, x^2 \dots$) which are present.

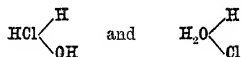
It is probable that the agreement between 'theory' and practice on which reliance has been placed, particularly in interpreting osmotic phenomena, is more often than not only apparent and fictitious and but the outcome of counterbalancing effects which have been left out of account. We are too prone to believe in constants; we need to remember that, except perhaps in the case of the perfectly gaseous state, *constants are dependent variables*. To take an example, it is assumed that glucose and cane sugar produce like osmotic effects when used in equivalent proportions; indeed, it has been the fashion of late years to treat non-electrolytes as harmless neutrals: in point of fact they differ as much in behaviour as do electrolytes and such a conclusion must be viewed with the gravest suspicion. Recently Dr. Eyre and I have been able to show that three substances so similar as methylic, ethylic and propylic alcohols produce effects in precipitating salts from solution which are markedly different, propylic alcohol being the most effective although the least soluble. It is clear that the precipitant does not act mainly by itself combining with and withdrawing water in direct com-

¹ Roy. Soc. Proc. 1908, 31, 80; *Science Progress*, Jan. 1909.

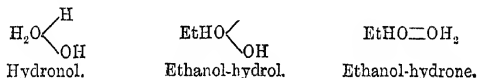
petition with the salt; but that it promotes the *dissociation of water* by the mechanical interposition of its molecules: in fact, that the 'dehydrating' powers of the water are enhanced owing to the increase in the proportion of simple molecules in the liquid conditioned by the presence of the solute.

The same effect is obvious when the reduction of the electric conductivity of a salt such as potassium chloride by equivalent quantities of the three alcohols is considered. This amounts to about 6 per cent. in the case of methylic, 12 in that of ethylic and 17 in that of propylic alcohol; the reduction effected by glucose, however, amounts to about 27 and that effected by cane sugar to no less than 42 per cent. In these two latter cases the amount of water actually withdrawn from the solution by the sugar is probably considerable and the 'mechanical effect' of the solute is therefore exercised in a more concentrated solution—more concentrated, that is to say, than those in which the alcohols act. If, therefore, solutions of glucose and cane sugar of equivalent strength produce like osmotic effects, it is because unperceived compensating factors are at work in the solutions which in algebraic sum have the same aggregate influence.

To explain the effect produced by substances which give rise to conducting solutions when dissolved in water (acids, alkalies and salts), it is necessary to consider the special nature of the changes which may be supposed to attend dissolution in such cases. Why, it may be asked, is an aqueous solution of hydrogen chloride a conductor whilst that of alcohol is a non-conductor? I believe the answer to be that it is because, in the former case alone, the two components of the solution are *reciprocally distributed*; that it is because two correlative systems—



are produced which interact under the influence of the electric stress.¹ In the case of alcohol no such interchange takes place. It may be that the alcohol is hydrolated to some slight extent but the hydrol must be less basic than hydronol; probably, like ammonia, alcohol exists in solution for the most part in the hydronated state:—



Much more must be learnt of the properties of solutions before definite decisions can be arrived at with regard to such delicate and refined issues.

I would apply the interpretation here given of the nature of conducting solutions generally to the explanation of all cases of chemical change: in other words, I assume that in all cases correlative systems are present which are formed by the reciprocal distribution of the interacting substances. From this point of view the solvent is no mere medium but an active participant in the series of interchanges of which, as a rule, only the final product is noticeable.

The solution thus offered of the complex problem discussed very fully in my Address in 1885, which has ever since occupied my thoughts, will, I trust, be found to be helpful, although by no means complete in all its details.

¹ I would repeat the plea I put forward in 1885 that the use of the term hydrochloric acid as applied to hydrogen chloride is undesirable if not unjustifiable; the solution of the gas may be said to contain *chlorhydric acid*, $\text{HCl}(\text{OH}_2)x$. From my point of view, oxygen is a constituent of all acids.

In effect, the doctrine makes no demand which the chemist should not be able to grant forthwith, as it is generally supposed that hydrols are easily formed—to give an example, in the case of the conversion of chloral, $\text{CCl}_3\text{.COH}$, into chloraldehydrol (chloral-hydrate), $\text{CCl}_3\text{.CH(OH)}_2$. The novelty of the conception lies in supposing that the occurrence of electrolysis involves the interaction of the hydrol and its correlative and the explanation which it affords of the difference between electrolytes and non-electrolytes.

It is essentially an association theory, although it involves the dissociation of the interacting substances but never the production of separated ions. In the case of aqueous solutions the amount of the distributed substances may be taken as the measure of the activity—of the degree of ionisation, so-called. A wrong view prevails that the so-called molecular conductivities are measures of activity; they are in reality only measures of the relative activities under corresponding conditions of the substances to which they refer. The molecular conductivity of an acid is at a maximum in its weakest solutions: presumably it is then present to the maximum extent in its simplest state and in the active hydrolated state; but as a hydrolytic agent its activity is at a maximum near to the opposite end of the scale. In other words, the hydrolytic activities of a series of acids are in the order of their molecular conductivities in solutions of comparable strength but molecular-hydrolytic and molecular-electrolytic activity run in opposite directions; the maximum electrolytic conductivity of an acid solution, which is manifest at a particular degree of concentration—presumably at the point at which the two forms of the distributed materials most nearly balance—is also in no way identical with maximum molecular hydrolytic activity. On these assumptions not a few of the deductions based on the ionic dissociation hypothesis are clearly fallacious.

It has been asserted that the association hypothesis does not admit of quantitative treatment and that therefore it is at a disadvantage; but if the quantitative meaning given to various results in accordance with the tenets of the dissociation hypothesis be more often than not one which is inadmissible, little is gained by applying the speculation quantitatively. As already remarked, the only cases in which chemical and electrolytic activity can be compared by the methods proposed are those in which the comparison is made between solutions of comparable or equivalent strength—that is to say, between compounds arranged in vertical series in the order of their activity.¹ Electrolytes are comparable in most, if not in all, their properties when the comparison is made in this way; but order of activity is one thing, actual activity another. It is in this sense, and this sense only, that we may agree with Arrhenius in his statement, '*L'activité électrolytique se confond avec l'activité chimique.*'

The ionic dissociation hypothesis is a beautiful mare's-nest, which fails apparently to fit the facts whenever it is examined. 'And the moral of *that is*,' to quote the words of the classical Duchess so well known to children, 'we must not use the words *ion* and *ionisation* in any speculative sense but confine their application to cases such as were contemplated by Faraday when he introduced the term *ion*; the conception of activity

¹ Solutions of acids and alkalies have maximum conducting power at certain relatively high degrees of concentration. Hydrolytic activity also increases steadily in the case of acids as the solution becomes more concentrated; whether it attains to a maximum and whether this coincides with the conductivity maximum is uncertain at present; it is very difficult to decide this point experimentally, as the rate of change is so rapid in strong solution; moreover, the action takes another course in strong solutions, as compounds are formed by the interaction of the hydrolyte and hydrolyst, so that two changes are superposed which cannot well be followed separately.

whether electrolytic or chemical, should alone be attached to such words; no idea of actual, separate, individual existence should enter into our minds in using them: the ion is to be thought of merely as the potentially active, transferable radicle in a compound, not as a separated particle enjoying independent existence.' It is so easy to speak of dissociation when it is desired to give expression to the idea; the first thing the scientific speaker or writer should guard against is ambiguity.

The subject of gaseous interchanges must not be left out of account, although it is impossible to do justice to it. Mendeleeff's contention that gaseous interchanges are usually bimolecular has been defended by Dixon and Larmor of late. But the facts must be faced. The almost inconceivable frequency of the molecular impacts must not be forgotten. The extraordinary attractive power of the hydrone molecule is also to be remembered—this would tend to promote the formation of aggregates with which the necessary third substance would every now and then form a bimolecular system—which, however, would in reality be at least trimolecular. The proportion of hydrone molecules in a dried gas has probably been under-estimated—the density of hydrone being very low (9)—as no dehydrating agent can be supposed to remove all such molecules or even nearly all; the hydrated substance must have a certain pressure of dissociation. Sir James Dewar's appears to be the only method which is in any way deserving of the epithet absolute; the results he has obtained with helium in a radiometer are strongly in favour of my view. Lastly, the gradual growth in velocity of the explosive wave up to the point of detonation as the compression becomes greater is clear indication that reduction in volume and increase of opportunity for the formation of systems of the proper degree of complexity is a matter of great consequence. Even the behaviour of cordite is significant, particularly the projection of unburnt rodlets of the material from the gun: apparently it is not decomposed by shock intramolecularly but is decomposed by heat into gases, which interact explosively.

Having dealt with the subjects of chemical change and the nature of solutions, however inadequately, I must now endeavour to justify my opening reference to the importance of the organic side of our science.

The province of organic chemistry is so vast that it may appear to be difficult to distinguish the main lines of advance from the by-paths which intersect the field of inquiry in every direction. In reality this is not the case; certain salient features stand out which must attract attention if once attention be drawn to them. The efforts of the chemist to elucidate structure and to correlate structure with function have been extraordinarily successful. In the first place, as already remarked, the student of the subject now has his attention concentrated on the tetrahedron as the symbol of the *functional activity* of carbon; however numerous the compounds, he knows that certain simple rules can be laid down as applicable to all. It is established beyond question that carbon atoms have a remarkable tendency to form ring systems. The affinities of the atom seem to act almost rigidly in certain directions, which appear to be those of lines drawn from the middle point of a regular tetrahedron to its angular points. Rings containing either five or six atoms of carbon are therefore those which are most readily formed and of maximum stability; carbon atoms may and do unite in pairs, threes and fours but compounds of this order are far less permanent than those containing either five or six atoms, as the affinities appear to meet in such a manner that they do not satisfy each other and consequently the compounds enter somewhat readily into combination with other substances. When the number of carbon atoms exceeds six, not only

is there less tendency to form a ring system but the stability of the system is slight; when the number is considerable, stability is attained by the formation of complex systems, consisting of several rings conjoined (camphor, naphthalene, anthracene, &c.).

The behaviour of carbon compounds generally, in so far as this may be regarded as dependent on the condition of the carbon, is extraordinarily simple and may be summed up in the statement that it is either paraffinoid, ethenoid or benzenoid.¹ Paraffinoid carbon is incapable of combining with other substances and but slightly attractive, so that the hydrogen atoms are by no means easily displaced from paraffinoid compounds²; ethenoid carbon combines readily with various substances, forming compounds of paraffinoid type; in the benzenoid state, carbon appears to combine somewhat readily with a variety of substances but the products enjoy only an ephemeral existence and usually escape notice, as they at once break down, giving rise to benzenoid substitution derivatives, so that in this last form carbon simulates the paraffinoid state but is more active.

In earlier years our attention was concentrated on benzene and the benzenoid compounds; much was done to elucidate the structure of these hydrocarbons and of their derivatives; meanwhile these latter have proved to be of extraordinary significance technically, notably as dye-stuffs but also on account of their medicinal value, as perfumes and in photography.

The structure of benzene has been the subject of much discussion during the period under consideration. I trust I shall not be accused of parental bias if I urge that the centric³ formula is the best expression of the *functional activity* of the hydrocarbon benzene and its immediate derivatives; the attempts which have been made of late years to resuscitate the Kekulé oscillation hypothesis in one form or another appear to me to be devoid of practical significance. Any formula which represents benzene as an ethenoid must be regarded as contrary to fact. But in considering the properties of benzenoid compounds generally, it is necessary to make use of the Kekulé conception as well as the centric expression. The model of benzene devised by Barlow and Pope subserves a somewhat different and complementary purpose, being primarily of importance on the geometric side in discussing the relation of form to structure.⁴

¹ A fourth condition requiring recognition is that of the carbon in acetylene; at present the acetylene compounds are so few in number, however, that this form may be left out of account.

² The displacement of the hydrogen associated with carbon is in all probability a secondary phenomenon; it is likely that this is true generally and that hydrogen is never merely removed or attracted away but always has its place taken by a radicle which becomes temporarily attached to the multivalent atom with which the hydrogen is associated.

³ I have discussed this matter somewhat fully in a recent essay, with reference to the nature of amorphous carbon, in connexion with the remarkable work of Sir James Dewar on the absorption of gases by charcoal at low temperatures (*Journal of the Royal Institution*).

⁴ The time is now approaching when it will be possible to extend the study of benzenoid compounds beyond the formal and superficial stage; hitherto we have been content to develop the methods of preparing such substances and to determine their number and their distinctive properties. Everything has to be learnt as to the exact character of the changes which attend their formation from the parent substance benzene and as to the exact nature of their inter-relationship. The impression produced by benzene, in my mind, is that of an eminently plastic system capable of responding to every slight change that may be impressed upon it. Nothing is more remarkable than the difference between benzene and its homologues, so obvious in the extraordinary increase in activity which attends the introduction of hydrocarbon radicles in place of one or more hydrogen atoms. But such plasticity is not characteristic of benzene only: if the properties of

The discovery of trimethylene by Freund and the subsequent introduction of synthetical methods of preparing polymethylenes by W. H. Perkin, jun., mark the onset of a new era, opening out as they did the possibility of understanding the structure of camphor and the terpenes and other constituents of the volatile oils from plants.

Chemist after chemist had attempted in vain to solve the riddle presented by camphor. Suddenly, in a moment of inspiration, a satisfactory solution of the problem was offered by Bredt. The acceptance of the bridged ring, the special feature of the Bredt formula of camphor, marks the introduction of a new moment into organic chemistry.

The recognition of similar rings in several hydrocarbons of the terpene class, mainly in consequence of the masterly work of von Baeyer, has contributed in no slight degree to an understanding of these compounds; nevertheless, much remains to be learnt and there are many and serious difficulties to be overcome before we shall be in a position to appreciate the genetic relationship of all the substances included in the group. When the account of the work is written it will form one of the most striking and fascinating chapters in the history of our science.

Among the many names of those who have contributed to its development the first to be mentioned is that of Wallach, to whose unwearied efforts, continued during a long series of years, so much is owing. The synthetic work carried out with brilliant success in recent years by W. H. Perkin may also be referred to as of extraordinary promise but of wellnigh inconceivable difficulty.

Before leaving this chapter reference should be made to the almost protean character of camphor, as disclosed by the work of inquirers such as Kipping, Pope, Forster, Lapworth and Lowry; no other substance has lent itself to use in quite so many directions and with such fruitful results. Special mention may be made of the demonstration which Pope has given, with the aid of the camphorsulphonic acids, that nitrogen, sulphur, selenium and tin give rise to optically active substances in all respects analogous to those furnished by carbon. The success with which Kipping's arduous labours have been crowned is also very noteworthy, taking into account the many difficulties he has overcome in preparing optically active silicon compounds. The extension of the Pasteur-van't Hoff theory of asymmetry inferentially to all elements which are at least quadrivalent, now accomplished, is of superlative importance.

Lowry's refined observations on the conditions which determine the interconversion of isodynamic forms of some of the camphor derivatives

benzene-sulphonic acid be contrasted with those of the various substituted benzene-sulphonic acids, it is clear that every variation meets with some response from the sulphonic group; what is still more remarkable, if the hydrogen in the hydroxylic group in the phenolsulphonic acids be displaced by other radicles, not only does the oxygen atom to which the radicle is attached seem to respond to the change but the benzenoid system and the still more distant sulphonic system are also affected. It is well known that the physical constants are all variables in the case of benzenoid compounds. Perhaps the most remarkable confirmation of the view here advanced, however, is that afforded by the conclusion arrived at by Barlow and Pope that in the case of benzene derivatives, although the spheres of influence of the carbon and hydrogen atoms are relatively the same as in the parent compound, the spatial arrangement of the component spheres of atomic influence remaining practically unchanged, nevertheless the actual volumes of the spheres of influence of both carbon and hydrogen alter proportionally to the alteration in molecular volume. Thus they maintain that in the case of the conversion of benzene (molecular volume 77.4) into tetrabromobenzene (molecular volume 130.2), the volumes of the spheres of influence of both carbon and hydrogen expand in the ratio of 77.4 : 130.2. Such a conclusion is very noteworthy.

may also be cited as of special value as a contribution to the study of metamerism and the conditions which determine chemical change generally.

Not the least interesting feature of camphor is the light thrown by its behaviour on the influence which oxygen exercises as an attractive element and on the part which spatial configuration may play in determining directions of change. It is clear that, whatever the agent, the attack is always delivered from the oxygen centre and that the direction in which the attack becomes effective depends on the position which the agent can take up relatively to the various sections of the molecule.¹

It must be confessed that our efforts to penetrate behind the veil in the case of the higher carbohydrates—starch and cellulose in particular—have not been rewarded with success.

Moreover, though much has been done of late years to unravel the nature of the vegeto-alkaloids, substances such as quinine are still only partially deciphered and not one of the more complex alkaloids has been produced synthetically. In view of the fact that quinine is still the one effective and practically safe anti-malarial medicine, the disclosure of its constitution is much to be desired. The isolation of adrenaline from the suprarenal capsule and the discovery that this alkaloid—which is an extraordinarily active substance physiologically—plays a most important part in controlling vital processes is of supreme interest. Other glands—the pituitary gland, for example—appear to contain peculiar active substances, which are of particular consequence in regulating animal functions. The discovery of such substances affords clear proof that life is largely dependent on what may be termed chemical control.

In addition to indigo, the simpler yellow and red natural colouring-matters have now been thoroughly examined but this class of substance still affords abundant opportunity to investigators. Kostanecki's comprehensive studies of the xanthone group may be referred to as of particular value.

Attention may be directed here to the investigation of brazilin and haematoxylin by W. H. Perkin and his various co-workers, not merely as being full of interest and importance as a contribution to our knowledge of the relation between colour and structure and as a brilliant example of technical skill but because of the illustration it affords of the extreme intricacy of such inquiries and of the vast amount of labour they entail. The general public probably has not the slightest conception of the difficulties which attend such research work and of its costliness.

As an investigator of vegetable colouring-matters, no one has been more assiduous or has displayed greater skill of late years than A. G. Perkin. His recent refined investigation of the colour-yielding constituents of the indigo plant is of exceptional value at the present time, although it is to be feared that it comes too late to save the situation in India. The work of the brothers Perkin, it may be pointed out, is of exceptional interest on the human side as well as from the scientific standpoint, as their enthusiasm and wonderful manipulative skill afford a striking and noteworthy example of hereditary genius.

Two substances of commanding interest which have long resisted attack—the red colouring-matter of the blood and leaf-green—are at last going the way of all things chemical, as the secret of their nature is being wrung from them. In Willstätter's skilful hands chlorophyll is proving to be by no means the fugitive material it was supposed to be; the complexity of the problem it offers, however, seems to be far beyond anything that could have been anticipated; so much greater will be the interest attaching to the final solution. The discovery that green chlorophyll is a magnesium salt is of

¹ Cf. *Chem. Soc. Trans.*,

special importance, as the first clear indication of the manner in which magnesium salts are of service to plants.

Apart from the special interest which attaches to the investigation of vegetable colouring-matters on account of their being coloured substances, such inquiries are of value as furnishing material for the discussion of the metabolic activity of plants.¹

Even colloids are being brought into line. Studded as they are with active centres (oxygen or nitrogen atoms), they seem to be able to attract and retain hydrone molecules at their surfaces in ways which give them their peculiar glue-like attributes: as a consequence living tissue appears to be little short of animated water.

To the present generation of students, the organo-metallic compounds must have appeared to belong to the past; the discovery of methides of platinum and gold by Pope will not only serve to reawaken interest in this group of compounds but is of primary importance as a contribution to our knowledge of the valency of these elements; the stability of the platinum derivatives is altogether astonishing.

The discovery announced in June last, at the International Congress of Chemistry, by Mond of compounds of carbonic oxide with ruthenium and uranium is a striking and most welcome extension of his previous labours, which had placed us in possession of carbonyls of nickel, iron and cobalt. The metallic carbonyls possess altogether remarkable properties: at present, these defy explanation; nickel carbonyl in particular seems to be an exception to all rules. The complex iron carbonyls made known by Dewar and Jones also have most fascinating attributes, the variety of colours they display being specially interesting. The marked individuality of the members of the iron group as exemplified in their carbonyl derivatives is in striking contrast with the tendency they display to behave as related elements; the deeper problems of valency are clearly exposed for consideration in such peculiarities.

The discoveries of the special activity of magnesium as a synthetic agent and of the superior value of nickel as a catalyst in fixing hydrogen are other illustrations of the individuality of metallic elements. We are greatly indebted to the French chemists for the invaluable preparative methods they have based on the use of these two agents.

¹ But a note of sadness pervades the story. The effect of learning to understand Nature always appears to be that we at once brush her aside when we have wrested from her the secrets which she has so long preserved inviolate. No sooner did we learn the nature of the madder colouring-matters than we proceeded to prepare them artificially—thus putting an end to the cultivation of a valuable crop. Indigo is meeting with a like fate, a catastrophe which might well have been avoided had scientific assistance been called in at the proper time. Not content with making natural colouring-matters, we set to work to outrival the rainbow in our laboratories and the feminine world is decked with every variety of colour in consequence, although unfortunately our blends too often lack the beauty of those of truly natural origin, which rarely, if ever, offend the eye. We congratulate ourselves on our cleverness in thus imitating Nature but no idea of thrift possesses us: moreover, our attempts to imitate if not to undo her work are never direct but are always made with her aid, with Nature's product—coal; we are no longer content to ride on horseback but must rush through space and instead of watching the birds fly seek to emulate them but always with the aid of fuel won by Nature from the soil and air in days long past. Too much is being done in every direction to waste natural resources, too little to conserve them, too little to employ man in his proper place—as tiller of the soil. Here lies the chemist's opportunity. At no very distant date, perhaps, when petrol is exhausted, toll will be taken from the sun in the form of starch or sugar and this will be converted into alcohol.

Although satisfactory progress has been made in almost every direction, many of the nitrogen compounds are still not properly understood. It is clear that we are as yet in no way seized with understanding of the attributes of this element as we are of those of oxygen and carbon, particularly in the case of mixed carbon-nitrogen compounds: we can make nothing of the physical data such substances afford. Nitrogen, in fact, is an extraordinary element, far more remarkable than any other; its 'temper' appears to vary more than that of any other element according to the character of its associates—nothing could be more remarkable, for example, than the change in properties from ammonia, NH_3 , through hydrazine, NH_2NH_2 , to azoimide, N_3H . No other element can be so poisonous, so immediately fatal to life. We lack a model symbolic of its functions—which means that we are unable to fathom its vagaries and reduce them to simple order.

The oximes and the diazo-compounds in particular have given rise to much dispute. Stereo-chemical formulæ have been assigned to these but probably they have little relation with the truth; although they have been of service by supplying symbols which can be offered up at examinations, by confining attention they have served to sterilise inquiry. No better illustration could be given of the truth of the remark made by my friend the Professor that man is an idolater by nature, a fact that chemists should always bear in mind.

The compounds in question are difficult substances to handle, far too prone to undergo change without invitation—it is to be feared that many of the conclusions which have been arrived at are based on incomplete if not unsatisfactory evidence.¹ When I think of the state of our knowledge, I am reminded of the father of diazo-chemistry, Peter Griess and of his marvellous experimental gifts; there is great need of such a man to reinvestigate the whole subject.

If we inquire as to the general effect of the increase of knowledge of organic compounds, it is clear that the lessons which emerge from all modern inquiries are such as to justify Larmor's remark that our conceptions of structure must be granted more than analogical significance. Everything tends to show that function and structure are most closely connected—odour, taste, colour, physiological effect, are specific rather than general properties, each conditioned in its special variety by some special structure; we are approaching very closely to a time when it should be possible to discuss such properties with considerable confidence.

Still it must not be forgotten that the problems they offer are all valency problems and that the nature of valency eludes us entirely at present.

The greatest advance which chemists may pride themselves upon having made during the past decade or two remains to be considered. In 1885, I spoke as follows:—The attention paid to the study of carbon compounds may be more than justified both by reference to the results obtained and to the nature of the work before us; the inorganic kingdom refuses any longer to yield up her secrets—new elements—except after severe compulsion; the organic kingdom, both animal and vegetable, stands ever ready before us. Little wonder, then, if problems directly bearing upon life prove the more attractive to the living. The physiologist complains that probably 95 per

¹ Since this was written, Thiele's discovery of 'Azomethane,' $\text{McN} : \text{NMe}$, has been announced. This is described as being, in the solid state, a distinctly coloured, very pale yellow substance. There can be little if any doubt, therefore, that, as Robertson and I have argued, the colourless so-called syn- and anti-diazo-salts cannot possibly be compounds of the $-\text{N} : \text{N}-$ or diazene type: such compounds would all be at least yellow in colour.

cent. of the solid matters of living structures are pure unknowns to us and that the fundamental chemical changes which occur during life are entirely enshrouded in mystery. It is in order that this may no longer be the case that the study of carbon compounds is being so vigorously prosecuted. Our weapons—the knowledge of synthetical processes and of chemical function—are now rapidly being sharpened but we are yet far from ready for the attack.'

My forecast has been more than justified; indeed, the advance to be recorded is nothing short of marvellous: the great problems of vital chemistry appear now no longer to be unattainable to our intelligence—their cryptic character seems to have disappeared almost suddenly. Many have contributed in greater or less degree but none in such measure as Emil Fischer, whose work both in the sugar group and in connection with the albuminoids must for ever rank as monumental.

It is difficult to appreciate the extent to which the practical genius of this chemist has carried us—difficult alike for those who understand the subject and those who do not; the significance of his labours is only apparent when the bearing of his results on the interpretation of vital phenomena is fully considered. In 1885, we were disputing as to the structure of substances such as glucose and galactose; now we not only are satisfied that they belong to the group of aldhexoses (aldoses) derived from normal hexane but, taking into account the monumental discoveries of Pasteur, to which precision has been given by van't Hoff's great generalisation, we are in a position to assign fully resolved structural formulæ not only to the natural products but to the nine other isomeric aldhexoses which Fischer has prepared artificially.

It is a striking fact that only three of the sixteen possible aldhexoses and but a single ketohexose (fructose), of which many are possible, are met with naturally. Nature is clearly most sparing, most economical, in her use of materials. And not only is this true of the hexoses, as very few of the possible lower and higher homologous carbohydrates occur in vegetable or animal materials and the condensed carbohydrates (cane sugar, starch, &c.), are all formed apparently from the hexoses and pentoses which occur naturally. The albuminoids, the alkaloids, the terpenes are also optically active substances; in other words, only a limited number of the possible forms are present. There is reason to suppose that the compounds of natural occurrence stand in close genetic connection and belong with few exceptions to the same series of enantiomorphs; in no other way is it possible to account for the occurrence of one only of the pair of enantiomorphous isomerides and for the relatively small number of compounds. Moreover, not only the sugars and most of the other products of the disintegration of the albuminoids but also the amino-acids, in like manner, are derivatives of compounds containing at most six atoms of carbon; the fats alone are of a considerably higher degree of complexity but they are probably collocations of the simpler units.

The terpenes and essential oils are mostly C_{10} derivatives; the alkaloids have complex formulæ but the units of which they are composed are simple; as all of them are optically active, it is clear that only some of the possible enantiomorphous combinations are present.

We are bound, therefore, to assume that a large proportion of the changes which occur in living organisms—which constitute vital metabolism—are directed changes. What is the nature of the directive power? We are already able to go far in explaining this, although our knowledge is mainly of analytical changes, the nature of synthetic changes being, at present, only inferentially disclosed to us.

It has long been known that under natural conditions many complex

of maltose into starch must take place in some similar manner. The recent observation that cellobiose is a β -glucoside enables us to realise that the formation of cellulose differs from that of starch in that the glucose molecule, instead of being converted into the β -glucoside maltose, becomes changed into the correlated β -glucoside, a membrane being thus secured which can resist the diastatic enzymes by which starch is attacked.

The formation of the albuminoid substances may be regarded from a similar point of view. At present, however, there is no satisfactory evidence to show at what stage nitrogen is introduced into the molecule. As the plant takes up nitrogen in the form of nitrate, not as ammonia, it is probable that the nitrate is reduced to hydroxylamine and that this rather than ammonia is the active synthetic agent. Formaldehyde and hydroxylamine would yield formaldoxime, which would easily pass into methylamine on reduction; the interaction of formaldoxime and formaldehydrol might give rise to a higher aldoxime which would be easily convertible into amino-acetic acid (glycine). Higher glycines might be formed from glycine by syntheses similar to those Erlenmeyer has effected; but to account for the formation of asymmetric amino-acids it is necessary to assume that the action is controlled at this stage and that the glycine is formed against a template perhaps under the influence of an enzyme.

Another conceivable mode of formation is by the fermentative degradation of glucosamine.

Until we know more of the order in which the amino-acid radicles are united in the various albuminoids and of the character of the associations other than those which are characteristic of polypeptides, we can consider the formation of albuminoids only from a very general point of view; but taking into account the very different proportions in which amino-acids and other cleavage products are formed on hydrolysing substances of different origin, it is clear that the several sections of the molecule must be differently ordered in the different proteins; again, therefore, it is necessary to assume that the formation of such substances is directed. We may picture molecule after molecule as being 'brought into line' against a template and the junctions which are required to bind the whole series together as being made through the agency of the enzymic dehydrating influence before referred to.

Attention has been called to the relatively simple way in which the hydrocarbons are constructed, that even the paraffins are not to be visualised as so many ducks strung upon a ramrod, Münchhausen fashion but as forming curls, owing to the natural set of the affinities. This probably is true of complex substances such as the proteins.

Protoplasm, in fact, may be pictured as made up of large numbers of curls, like a judge's wig—all in intercommunication through some centre, connected here and there perhaps also by lateral bonds of union. If such a point of view be accepted, it is possible to account for the occurrence in some sections of the complex series of interchanges which involve work being done upon the substances brought into interaction, the necessary energy being drawn from some other part of the complex where the interchanges involve a development of energy.

The conclusions thus arrived at may be utilised in discussing the problem of heredity. The inheritance of parental qualities, the need to assume continuity of the germ plasm and the comparative unimportance from the standpoint of heredity of somatic qualities, as well as the non-inheritance of mere environmental effects (acquired characters), are all necessary consequences of the view I have advanced.

The general similarity of structure throughout organised creation may well be conditioned primarily by properties inherent in the materials of

which all living things are composed—of carbon, of oxygen, of nitrogen, of hydrogen, of phosphorus, of sulphur. At some early period, however, the possibilities became limited and directed processes became the order of the day. From that time onward the chemistry prevailing in organic nature became a far simpler chemistry than that of the laboratory; the possibilities were diminished, the certainties of a definite line of action were increased. How this came about it is impossible to say; mere accident may have led to it. Thus we may assume that some relatively simple asymmetric substance was produced by the fortuitous occurrence of a change under conditions such as obtain in our laboratories and that consequently the enantiomorphous isomeric forms of equal opposite activity were produced in equal amount. We may suppose that a pool containing such material having been dried up dust of molecular fineness was dispersed; such dust falling into other similar pools near the crystallisation point may well have conditioned the separation of only one of the two isomeric forms present in the liquid. A separation having been once effected in this manner, assuming the substance to be one which could influence its own formation, one form rather than the other might have been produced. An active substance thus generated and selected out might then become the origin of a series of asymmetric syntheses. How the complicated series of changes which constitute life may have arisen we cannot even guess at present; but when we contemplate the inherent simplicity of chemical change and bear in mind that life seems but to depend on the simultaneous occurrence of a series of changes of a somewhat diverse order, it does not appear to be beyond the bounds of possibility to arrive at a broad understanding of the method of life. Nor are we likely to be misled into thinking that we can so arrange the conditions as to control and reproduce it; the series of lucky accidents which seem to be required for arrangements of such complexity to be entered upon is so infinitely great.

The ovum and the spermatozoon must be supposed to have all the directive influences stored up in them which are subsequently brought into play in the development of the organism; they may be looked upon as bundles of templates of very definite structure. As both paternal and maternal qualities may be handed on to the offspring and as there is so near an equality of the sexes in the higher organisms, it appears likely that the male and female elements are produced in equal numbers by both parents, either the one or the other becoming developed at conception, according to the accidents of the moment, whereby both the sex of the offspring is determined and whether it be primarily derived from the one or the other parent.

There cannot well be complete interpenetration of the two elements: rather is it to be supposed that surface contacts are established which lead to transferences from one to the other chromosome; to use vulgar terms, that eyebrows are pencilled, the nose straightened, narrowed or broadened, hair made fair or dark; that by interpenetration of the curls this or that other quality is modified by a molecule being plastered on here, another smoothed off there, while cross-connections are established in some directions but broken in others.

Such a picture cannot be regarded as extravagant. We may even claim literary appreciation in support of our temerity—no less a writer than Emerson, for example, as witness the following passage in his 'Uses of Great Men':—

'Light and darkness, heat and cold, hunger and food, sweet and sour, solid, liquid and gas, circle us round in a wreath of pleasures and, by their agreeable quarrel, beguile the day of life. The eye repeats every day the first eulogy on things: "He saw that they were good." We know

And yet we never tire of preaching the value of classical learning and of history—the real history that counts and that training in scientific method which would tend to broaden and inform the mind are in no way thought of by the literary triflers who pretend to guide the destinies of our youth. Surely some effective means must be devised that will enable us to revolt without further delay against our unnatural and generally worthless system of school and university training.

No problem can compare in importance with that of the future of our race. To consider it is the one plain duty before us and the need becomes daily a more urgent one. Not only do we encourage deterioration at the lower end of the scale of intelligence in the manner pointed out in the passage quoted from Darwin, we are now through our system of higher education courting failure also at the upper end. Herbert Spencer forcibly drew attention many years ago to the tendency which the development of individuality must have to depress fertility and to the evil effects of severe mental labour on women especially. It has been stated that in the United States of America the higher education of girls has been proved to sterilise them. Many of us probably have experience within our own circle of observation which would justify such a conclusion; there are so many ways in which education operates to retard marriage, even if it have no direct effect on the organism.

Even if man-stuff and woman-stuff be in no fundamental way different materials, there are essential differences between the sexes which must be taken into consideration. During the active period of her life the woman is subject at intervals to influences which do not affect the man; various excitants (so-called hormones) come into operation and produce effects which are altogether remarkable; her mental condition is consequently in a state of continued flux. Cause and effect in these cases are undoubtedly chemical in their nature. The changes which attend puberty are probably brought about by the more or less sudden outpouring of peculiar secretions which direct metabolism into new and special channels. It is clear that mental states have an important influence on metabolism and that if influences are brought to bear from which the organism has been exempt in the past, effects must be produced the nature of which it is impossible to predict; therefore the creation of new interests may well be a source of most serious danger. The most disquieting feature of the times is the revolt of women against their womanhood and their claim to be on an equality with man and to compete with men in every way. There should be no question of equality raised; when comparison is made between complementary factors the question of equality does not and cannot come into consideration. It is clear that should the struggle arise—and it is to be feared that it is coming upon us—there can be but one issue: woman must fail and in failing must carry man with her to destruction, for she will

soon eliminated; and those that survive commonly exhibit a vigorous state of health. We civilised men, on the other hand, do our utmost to check the process of elimination; we build asylums for the imbecile, the maimed and the sick; we institute poor-laws; and our medical men exert the utmost skill to save the life of everyone to the last moment. There is reason to believe that vaccination has preserved thousands who from a weak constitution would formerly have succumbed to small-pox. Thus the weak members of civilised societies propagate their kind. No one who has attended to the breeding of domestic animals will doubt that this must be highly injurious to the race of man. It is surprising how soon a want of care or care wrongly directed leads to the degeneration of a domestic race; but excepting in the case of man himself, hardly anyone is so ignorant as to allow his worst animals to breed.'

inevitably cease to exercise her specific womanly functions with effect, so delicate is the adjustment of her mechanism. The evolution of the two sexes has been on different lines and different qualities have been developed in them; it is probable that the germinal differences are profound. And education cannot remove the difference; although education may condition functional disturbances, it must be powerless to modify the structure and mechanism. Man is in no way what he is to-day in virtue of the education he has received during a few generations past; the education of the race throughout time has been something entirely different from what is thought of now as education. 'Nature, the dear old nurse,' not man, has done the work by a severe and drastic process of selection—by picking out men capable of doing men's work and by picking out women capable of doing women's work: she has constituted them helpmates and has had no thought of their being so silly as to wish to get in one another's way; this is a state brought on by an artificial, unsuitable system of education.

The subject has been brought before the chemical world in England recently by the application of a number of women to be made Fellows of the Chemical Society. Many of us have resisted the application because we were unwilling to give any encouragement to the movement which is inevitably leading women to neglect their womanhood, which is in itself proof that they do not understand the relative capacities of the two sexes and the need there is of sharing the duties of life. If there be any truth in the doctrine of hereditary genius, the very women who have shown ability as chemists should be withdrawn from the temptation to become absorbed in the work, for fear of sacrificing their womanhood; they are those who should be regarded as chosen people, as destined to be the mothers of future chemists of ability. The argument is applicable generally; it is surely desirable in all cases of declared ability that the education of girls should be directed so as to produce not merely minimum disturbance of the woman's attributes and charms but full understanding of the unique position of responsibility she occupies in the scheme of life.

Questions such as I have raised are of the utmost importance as bearing on educational policy. Our ideas of education are in almost as inchoate a state as they were in 1835. We have been led, it is true, to recognise that our scheme of popular elementary education is a terrible failure, that its whole tendency has been to emasculate our population; yet at the very time that we are making this discovery we are beginning to force our higher education along lines which experience shows must be ineffective—along literary lines. I should be the last to deny that there is an undercurrent of improvement perceptible but this is directed only by sporadic influences and is in no way favoured by most of those in authority. We are still suffering at the hands of those who have been our persecutors in the past—the clerics, who control most of the schools and whose outlook is almost as narrow as it ever was. The saving grace of science has in no way entered into their souls—how can it? The Universities make no attempt to secure their redemption. London of late years has even reversed the enlightened policy the University so long pursued and has allowed Latin to figure as alternative to science, not as the complement of science.

Our Association seems to have little or no effect on the public conscience. And the explanation is not far to seek. Our interests are too special; we are all too much wrapped up in our own affairs; too inconsiderate to co-operate effectively. We forget or do not realise, that 'Something is wanting in science until it has been humanised'; we make no attempt to organise our forces and make good the claim we put forward to be the possessors of superior knowledge. A complete change of attitude on our part is required; we need to play the part of propagandists. We are almost unknown as

popular writers and the days of popular lectures are past. Practically nothing is done to train the public mind and school science is in no way effective.

To speak particularly of my own subject, it is impossible to rate chemistry at too high a value in Canada. The maintenance of the fertility of your fields, the proper utilisation of your vast mineral wealth, the purity of your food supplies¹ will depend mainly on the watchful care and skill of chemists; but the educational value of the subject may also be set very high. If properly taught in your schools, it will afford a means superior to all others, I believe, of training faculties which in these days should be developed in every responsible citizen. No other subject lends itself so effectively as a means of developing the experimental attitude of mind—the attitude of working with a clearly conceived purpose to a desired end, which is so necessary to success in these days; and if care be taken to inculcate habits of neatness and precision and of absolute truthfulness, if care be taken to teach what constitutes evidence, the moral value of such work is incalculable. But to be effective it must be done under proper conditions, systematically; the time devoted to the work must be adequate; I would even advocate that the subject be allowed to come before conventional geography and history and other unpractical subjects, assuming that the training is given in a practical way and with practical objects in view, not in the form of mere lessons learnt by rote; if taught in the form of mere didactic lessons it is as worthless as any other subject as mental discipline. Let me add that I would confine the teaching to a narrow range of problems but make it very thorough with reference to these.

Five-and-twenty years ago I made my appearance as an advocate of what has been dubbed the heuristic method—the method which entails putting the learner in the attitude of inquirer, in training the pupil to inquire always into the meaning of what is learnt. I believe it to be in principle the only true method of learning. The idea has found favour almost generally but the progress made in applying it has been slight—and this was to be expected, as teachers were few and far between who could carry the method into execution; moreover, so few teachers will allow their pupils to learn: they are too impatient and insist on teaching them and on doing the work of teacher and learner—in fact, in these days, the learner is a rarity, examinations have almost destroyed the breed. If here you desire that your children shall grow up virile men and women with some honesty of purpose left in them, you will end and not mend a system which is sucking the very life-blood out of the youth in the mother country—you will insist that your children shall be taught little but learn much.

When studied as a special object, chemistry, in particular, is one of the subjects which must be worked at long and persistently—mere technical skill counts for so much and so few seem to possess the ability to become skilful chemists; in no other science does the element of understanding and an indefinable power of appreciating the character of changes as they occur play so conspicuous a part—in no other science is the faculty of judgment more necessary. In practice, the chemist in works is constantly called upon to exercise his judgment—he is only too often called upon to judge from appearances of conditions which are deep-seated; he is everywhere the works physician in fact. It is therefore necessary that he should be highly trained

¹ I should like to take this opportunity of saying that it is impossible to overrate the public value of the great work which Dr. Wiley has undertaken in the United States in endeavouring to secure the supply of food free from deleterious ingredients. At home we certainly need some one to preach a similar crusade and to free us from doctored infants' foods and the innumerable host of medicines by which even our fair fields are disfigured.

and thoroughly versed in the art of inquiry. The men who in my experience have been successful are those who have learnt to think for themselves and who have been capable independent workers—sufficiently broad-minded and sufficiently practised in their art to be able to turn their attention in any desired direction; I should add that they have been men who have learnt to read—a much neglected art. Much has been said and written of late on the subject of technical training which is of value as bringing out the various points of view; the problem is a very difficult one, owing to the great number of interests to be considered and more especially the very uneven and often inferior quality of the material to be trained. The great danger of specialised technical training is the tendency to make it too narrow. Success in practice depends not merely on knowledge of subject but also, if not mainly, on the possession of certain human qualities which are not usually developed in the technical school and which cannot be tested by examination—it is unnecessary to specify them. It is undeniable that in England for many years past chemistry has suffered from the recognised fact that there has been little money in it—parents have been led therefore to prefer other careers for their sons and the subject has not secured its due proportion of intelligence and is suffering in consequence. Too many of those who have entered works have had neither the intelligence nor—to speak plainly—the presence and manners that are required to secure confidence. The presence of men of gentlemanly bearing and instincts, who have received thorough training in science, is urgently needed at the present time in many of our manufacturing establishments, to take the place of foremen of the old type, who have learnt all they know in the works and whose conceptions necessarily lack breadth; it is almost impossible to convince such men that improvements are possible, too often they adopt a selfish attitude and advisedly retard progress. Another direction in which an approach of interests is required is between chemist and engineer. The latter has too long occupied a dominant position in many works and in not a few cases has done his utmost to exclude the chemist, fearing his competition apparently. The gas industry perhaps affords the most striking illustration of the effects of such a policy: on the engineering side it has been carried to a high pitch of perfection but on the chemical it has ever fallen, year after year, to a lower state; now the quality of coal gas is such, especially since the withdrawal of the sulphur clauses from the Acts of Parliament by which the industry is regulated, that gas is almost unusable.¹

But the iron industry is an even more striking case. The appliances are wonderful examples of constructive skill but the engineer is clearly nonplussed when he seeks to understand the processes he nominally controls; the chemist has been kept so closely confined to his bench in the laboratory that he has had no proper opportunity of studying the processes of manufacture systematically. No systematic study of steel has yet been made! Considering the magnitude of the industry and its importance, our knowledge of the subject is phenomenally slight; what we do know

¹ Had not chemists entirely unconnected with the industry vastly improved the methods of burning it, gas would long since have fallen into disuse. At last, when almost too late, the industry is taking some notice of our science. It needs reformation and reorganisation root and branch. That an industry should exist whose business it is to sell, as the primary product of manufacture, so minor a constituent of coal as the gas we burn is an anachronism. We should gain vastly in our cities if we burnt soft coke instead of smoke-yielding coal. Lastly, it is now imperative that none of the valuable constituents of coal should be wasted. Combining these three considerations, it is obviously desirable that, in future, all coal should be coked and both gas and coke supplied to the public instead, whilst the valuable residuals are used in other ways. No improvement has been effected by municipalities who have taken over the supply of gas; in the public interest some of these might well initiate such a change as is here suggested.

of the relation of strength and structure to composition is due to the pioneer labours of the late distinguished Dr. Sorby, an amateur unconnected with the industry—and to a fruitful conjunction of the labours of engineers and chemists outside the works, who in self-protection have tested the materials before use.

In Germany the chemist and the engineer have been placed on an equality and required to work together, with results which are altogether satisfactory. We need to adopt a similar practice. Any attempt to fuse the two into one will meet with failure, I am persuaded; they are called upon to work from different points of view—they need to be in sympathy and to understand one another but their work is complementary. I have watched engineering students closely during years past and am satisfied that, on the average, they represent a type of mind different from that of the chemist—the tendency of the one is to be constructive and of the other to be reflective: the analytical work done by the chemist in the laboratory is but the means to an end in the same way that the work done by the engineer in the drawing office is. Our future engineers should study chemistry and chemists should study engineering, in order that they may understand one another and work together—not in order that they may supplant one another. The chemist has to some extent allowed himself to be pushed into the background—perhaps because the average chemist in the past has been too tame a person; moreover, being forced to work in his laboratory, he has had less opportunity of gaining freedom and breadth of outlook than the more fortunate engineer, whose work has carried him out into the world.

You will be wise in Canada if you take care to select no small number of your abler students—young men of promise physically as well as intellectually—and train them as chemists. Of late years attention has been called from every side to the inconsiderate manner in which raw materials, especially coal, iron and wood, are being used up in all civilised countries; it is difficult to interest the public in such a subject, as few can appreciate the consequences, owing to the general ignorance of science and to the existence of an optimistic belief in the power of scientific discovery. But nothing will compensate for the exhaustion of your coal and iron supplies. It is your bounden duty to economise these in every possible way. The chemist and engineer will be required to help you in effecting economies by improving present methods of treatment. But the further question arises whether it be not also your duty, here in Canada particularly, without loss of time, to effect still greater economies by utilising the vast stores of energy in your possession, in the form of uplifted water, which now run to waste. The falls of Niagara are the most glorious and entrancing sight in the world I have witnessed next to the total eclipse of the sun, yet I question whether it be permissible to allow any part of their available energy to be dissipated—whether the claims of posterity do not forbid us to allow æsthetic considerations to prevail in such a case.

To conclude, I have treated my subject very widely and at times vaguely, having ranged over a great variety of subjects—somewhat from the point of view of modern opera, perhaps; indeed, I am willing to confess that I have been much influenced of late years by music and by the recognition of the obvious desire reflective musicians have shown to secure breadth of effect and harmonious development of all the elements which go to compose a dramatic situation. Chemistry touches the drama of life at every point: if ever we are to understand life and regulate our actions in accordance with understanding, it will be in no slight measure because we appreciate the lessons which chemistry alone affords.

British Association for the Advancement of Science.

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ADDRESS TO THE GEOLOGICAL SECTION

BY

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THE circumstances of the present meeting very clearly determine the subject of a general address to be expected from a student of extinct animals. The remarkable discoveries of fossil backboneed animals made on the North American continent during the last fifty years suggest an estimate of the results achieved by the modern systematic methods of research; while the centenary celebration of the birth of Darwin makes it appropriate to consider the extent to which we may begin deducing the laws of organic evolution from the life of past ages as we now know it. Such an address must, of course, be primarily biological in character, and treat of some matters which are not ordinarily discussed by Section C. The subject, however, can only be appreciated fully by those who have some practical acquaintance with the limitations under which geologists pursue their researches, and especially by those who are accustomed to geological modes of thought.

There has been an unfortunate tendency during recent years for the majority of geologists to relinquish the study of fossils in absolute despair. More ample material for examination and more exact methods of research have altered many erroneous names which were originally used; while the admission to scientific publications of too many mere literary exercises on the so-called 'law of priority' has now made it necessary to learn not one, but several names for some of the genera and species which are commonly met with. Even worse, the tentative arrangement of fossils in 'genetic series' has led to the invention of a multitude of terms which often serve to give a semblance of scientific exactitude to the purest guess-work, and sometimes degenerate into a jargon which is naturally repellent to an educated mind. Nevertheless, I still hope to show that, with all these difficulties, there is so much of fundamental interest in the new work that it is worth while to make an effort to appreciate it. Geology and palæontology in the past have furnished some of the grandest possible contributions to our knowledge of the world of life; they have revealed hidden meanings which no study of the existing world could even suggest; and they have started lines of inquiry which the student of living animals and

plants alone would scarcely have suspected to be profitable. The latest researches are the logical continuation of this pioneer work on a more extensive scale, and with greater precision; and I am convinced that they will continue to be as important a factor in the progress of post-Darwinian biology as were the older studies of fossils in the philosophy of Cuvier, Brongniart, and Owen.

In this connection it is necessary to combat the mistaken popular belief that the main object of studying fossils is to discover the 'missing links' in the chain of life. We are told that the idea of organic evolution is not worthy of serious consideration until these links, precise in character, are forthcoming in all directions. Moreover, the critics who express this opinion are not satisfied to consider the simplest cases, such as are afforded by some of the lower grades of 'shell-fish' which live together in immense numbers and have limited powers of locomotion. They demand long series of exact links between the most complex skeletal frames of the backboned animals, which have extreme powers of locomotion, are continually wandering, and are rarely preserved as complete individuals when they are buried in rock. They even expect continual discoveries of links among the rarest of all fossils, those of the higher apes and man. The geologist, on the other hand, knowing well that he must remain satisfied with a knowledge of a few scattered episodes in the history of life which are always revealed by the merest accident, marvels that the discovery of 'missing links' is so constant a feature of his work. He is convinced that, if circumstances were more favourable, he would be able to satisfy the demand of the most exacting critic. He has found enough continuous series among the mollusca, for example, and so many suggestions of equally gradual series among the higher animals, that he does not hesitate to believe without further evidence in a process of descent with modification. The mere reader of books is often misled by the vagaries of nomenclature to suppose that the intervals between the links are greater than in reality; but for the actual student it is an everyday experience to find that fossils of slightly different ages which he once thought distinct are linked together by a series of forms in which it is difficult to discover the feeblest lines of demarcation. He is therefore justified in proceeding on the assumption that in all cases the life of one geological period has passed by a natural process of descent into that of the next succeeding period; and, avoiding genealogical guesswork which proves to be more and more futile, he strives to obtain a broad view of the series of changes which have occurred, to distinguish between those which denote progress and those which lead to stagnation or extinction. When the general features of organic evolution are determined in this manner, it will be much easier than it is at present to decide where missing links in any particular case are most likely to be found.

Among these general features which have been made clear by the latest systematic researches, I wish especially to emphasise the interest and significance of the persistent progress of life to a higher plane, which we observe during the successive geological periods. For I think palaeontologists are now generally agreed that there is some principle underlying this progress much more fundamental than chance-variation or response to environment however much these phenomena may have contributed to certain minor adaptations. Consider the case of the backboned animals, for instance, which I happen to have had special opportunities of studying.

We are not likely ever to discover the actual ancestors of animals on the backboned plan, because they do not seem to have acquired any hard skeleton until the latter part of the Silurian period, when fossils prove them to have been typical and fully developed, though low in the backboned scale. The ingenious researches and reasoning of Dr. W. H. Gaskell,

however, have suggested the possibility that these animals originated from some early relatives of the scorpions and crustaceans. It is therefore of great interest to observe that the Eurypterids and their allies, which occupy this zoological position, were most abundant during the Silurian period, were represented by species of the largest size immediately afterwards at the beginning of the Devonian, and then gradually dwindled into insignificance. In other words, there was a great outburst of Eurypterid life just at the time when backboned animals arose; and if some of the former were actually transformed into the latter, the phenomenon took place when their powers both of variation and of multiplication were at their maximum.

Fishes were already well established and distributed over perhaps the greater part of the northern hemisphere at the beginning of Devonian times; and then there began suddenly a remarkable impulse towards the production of lung-breathers, which is noticeable not only in Europe and North America, but also probably so far away as Australia. In the middle and latter part of the Devonian period, most of the true fishes had paddles, making them crawlers as much as swimmers; many of them differed from typical fishes, while agreeing with lung-breathers, in having the basis of the upper jaw fused with the skull, not suspended; and some of them exhibited both these features. Their few survivors at the present day (the Crossopterygians and Dipnoans) have also an air-bladder, which might readily become a lung. The characteristic fish-fauna of the Devonian period, therefore, made a nearer approach to the land animals than any group of fishes of later date; and it is noteworthy that in the Lower Carboniferous of Scotland—perhaps even in the Upper Devonian of North America, if footprints can be trusted—amphibians first appeared. In Upper Carboniferous times they became firmly established, and between that period and the Trias they seem to have spread all over the world; their remains having been found, indeed, in Europe, Spitzbergen, India, South Africa, North and South America, and Australia.

The Stegocephala or Labyrinthodonts, as these primitive amphibians are termed, were therefore a vigorous race; but the marsh-dwelling habits of the majority did not allow of much variation from the salamander-pattern. Only in Upper Carboniferous and Lower Permian times did some of their smaller representatives (the Microsauria) become lizard-like, or even snake-like in form and habit; and then there suddenly arose the true reptiles. Still, these reptiles did not immediately replace the Stegocephala in the economy of Nature; they remained quite secondary in importance at least until the Upper Permian, in most parts even until the dawn of the Triassic period. Then they began their flourishing career.

At this time the reptiles rapidly diverged in two directions. Some of them were almost exactly like the little *Sphenodon*, which still survives in some islands off New Zealand, only retaining more traces of their marsh-dwelling ancestors. The majority (the Anomodonts or Theromorphs) very quickly became so closely similar to the mammals that they can only be interpreted as indicating an intense struggle towards the attainment of the higher warm-blooded grade; and there is not much doubt that true mammals actually arose about the end of the Triassic period. Here, again, however, the new race did not immediately replace the old, or exterminate it by unequal competition. Reptiles held their own on all lands throughout the Jurassic and Cretaceous periods, and it was not until the Tertiary that mammals began to predominate.

As to the beginning of the birds, it can only be said that towards the end of the Triassic period there arose a race of small Dinosaurs of the lightest possible build, exhibiting many features suggestive of the avian skeleton; so it is probable that this higher group also originated from an

intensely restless early community of reptiles, in which all the variations were more or less in the right direction for advancement.

In short, it is evident that the progress of the backboneed land animals during the successive periods of geological time has not been uniform and gradual, but has proceeded in a rhythmic manner. There have been alternations of restless episodes which meant real advance, with periods of comparative stability, during which the predominant animals merely varied in response to their surroundings, or degenerated, or gradually grew to a large size. There was no transition, for instance, between the reptiles of the Cretaceous period and the mammals which immediately took their place in the succeeding Eocene period: those mammals, as we have seen, had actually originated long ages before, and had remained practically dormant in some region which we have not yet discovered, waiting to burst forth in due time. During this retirement of the higher race the reptiles themselves had enjoyed an extraordinary development and adaptation to every possible mode of life in nearly all parts of the globe. We do not understand the phenomenon—we cannot explain it; but it is as noticeable in the geological history of fishes as in that of the land animals just considered. It seems to have been first clearly observed by the distinguished American naturalist, the late Professor Edward D. Cope, who termed the sudden fundamental advances 'expression points' and saw in them a manifestation of some inscrutable inherent 'bathmic force.'

Perhaps the most striking feature to be noticed in each of these 'expression points' is the definite establishment of some important structural character which had been imperfect or variable before, thus affording new and multiplied possibilities of adaptation to different modes of life. In the first lung-breathers (*Stegocephala*), for example, the indefinite paddle of the mud fishes became the definite five-toed limb; while the incomplete backbone reached completeness. Still, these animals must have been confined almost entirely to marshes, and they seem to have been all carnivorous. In the next grade, that of the reptiles, it became possible to leave the marshes; and some of them were soon adapted not only for life on hard ground or in forests, but even for flight in the air. Several also assumed a shape of body and limbs enabling them to live in the open sea. Nearly all were carnivorous at first, and most of them remained so to the end; but many of the Dinosaurs eventually became practically hoofed animals, with a sharp beak for cropping herbage, and with powerful grinding teeth. In none of these animals, however, were the toes reduced to less than three in number, and in none of them were the basal toe-bones fused together as they are in cattle and deer. It is also noteworthy that the brain in all of them remained very small and simple. In the final grade of backboneed life, that of the mammals, each of the adaptive modifications just mentioned began to arise again in a more nearly perfected manner, and now survival depended not so much on an effective body as on a developing brain. The mammals began as little carnivorous or mixed-feeding animals with a small brain and five toes, and during the Tertiary period they gradually differentiated into the several familiar groups as we now know them, eventually culminating in man.

The demonstration by fossils that many animals of the same general shape and habit have originated two or three times, at two or three successive periods, from two or three continually higher grades of life, is very interesting. To have proved, for example, that flying reptiles did not pass into birds or bats, that hoofed Dinosaurs did not change into hoofed mammals, and that Ichthyosaurs did not become porpoises; and to have shown that all these later animals were mere mimics of their predecessors, originating independently from a higher yet generalised stock, is a remark-

able achievement. Still more significant, however, is the discovery that towards the end of their career through geological time totally different races of animals repeatedly exhibit certain peculiar features, which can only be described as infallible marks of old age.

The growth to a relatively large size is one of these marks, as we observe in the giant Pterodactyls of the Cretaceous period, the colossal Dinosaurs of the Upper Jurassic and Cretaceous, and the large mammals of the Pleistocene and the present day. It is not, of course, all the members of a race that increase in size; some remain small until the end, and they generally survive long after the others are extinct; but it is nevertheless a common rule that the prosperous and typical representatives are successively larger and larger, as we see them in the familiar cases of the horses and elephants of the northern hemisphere, and the hoofed animals and armadillos of South America.

Another frequent mark of old age in races was first discussed and clearly pointed out by the late Professor C. E. Beecher, of Yale. It is the tendency in all animals with skeletons to produce a superfluity of dead matter, which accumulates in the form of spines or bosses as soon as the race they represent has reached its prime and begins to be on the downgrade. Among familiar instances may be mentioned the curiously spiny Graptolites at the end of the Silurian period, the horned Pariasaurians at the beginning of the Trias, the armour-plated and horned Dinosaurs at the end of the Cretaceous, and the cattle or deer of modern Tertiary times. The latter case—that of the deer—is specially interesting, because fossils reveal practically all the stages in the gradual development of the horns or antlers, from the hornless condition of the Oligocene species, through the simply forked small antlers of the Miocene species, to the largest and most complex of all antlers seen in *Cervus sedgwicki* from the Upper Pliocene and the Irish deer (*C. giganteus*) of still later times. The growth of these excrescences, both in relative size and complication, was continual and persistent until the climax was reached and the extreme forms died out. At the same time, although the palæontologist must regard this as a natural and normal phenomenon not directly correlated with the habits of the race of animals in which it occurs, and although he does not agree with the oft-repeated statement that deer may have 'perfected' their antlers through the survival of those individuals which could fight most effectively, there may nevertheless be some truth in the idea that the growths originally began where the head was subject to irritating impacts and that they so happened to become of utility. Fossils merely prove that such skeletal outgrowths appear over and over again in the prime and approaching old age of races; they can suggest no reasons for the particular positions and shapes these outgrowths assume in each species of animal.

It appears, indeed, that when some part of an animal (whether an excrescence or a normal structure) began to grow relatively large in successive generations during geological time, it often acquired some mysterious impetus by which it continued to increase long after it had reached the serviceable limit. The unwieldy antlers of the extinct Sedgwick's deer and Irish deer just mentioned, for example, must have been impediments rather than useful weapons. The excessive enlargement of the upper canine teeth in the so-called sabre-toothed tigers (*Machærodus* and its allies) must also eventually have hindered rather than aided the capture and eating of prey. The curious gradual elongation of the face in the Oligocene and Miocene Mastodons, which has lately been described by Dr. Andrews, can only be regarded as another illustration of the same phenomenon. In successive generations of these animals the limbs seem to have grown continually longer, while the neck remained short, so that the head necessarily became

more and more elongated to crop the vegetation on the ground. A limit of mechanical inefficiency was eventually reached, and then there survived only those members of the group in which the attenuated mandible became shortened up, leaving the modified face to act as a 'proboscis.' The elephants thus arose as a kind of after-thought from a group of quadrupeds that were rapidly approaching their doom.

The end of real progress in a developing race of backboned animals is also often marked by the loss of the teeth. A regular and complete set of teeth is always present at the commencement, but it frequently begins to lack successors in animals which have reached the limit of their evolution, and then it soon disappears. Tortoises, for instance, have been toothless since the Triassic period, when they had assumed all their essential features; and birds have been toothless since the end of Cretaceous times. The monotreme mammals of Australasia, which are really a survival from the Jurassic period, are also toothless. Some of the latest Ichthyosaurs and Pterodactyls were almost or quite toothless; and I have seen a jaw of an Upper Cretaceous carnivorous Dinosaur (*Genyodectes*) from Patagonia so completely destitute of successional teeth that it seems likely some of these land reptiles nearly arrived at the same condition.

Among fishes there is often observable still another sign of racial old age—namely, their degeneration into eel-shaped forms. The Dipnoan fishes afford a striking illustration, beginning with the normally shaped *Dipterus* in the Middle Devonian, and ending in the long-bodied *Lepidosiren* and *Protopterus* of the present day. The Palaeozoic Acanthodian sharks, as they are traced upwards from their beginning in the Lower Devonian to their end in the Permian, also acquire a remarkable elongation of the body and a fringe-like extension of the fins. Among higher fishes, too, there are numerous instances of the same phenomenon, but in most of these the ancestors still remain undiscovered, and it would thus be tedious to discuss them.

Finally, in connection with these obvious symptoms of old age in races, it is interesting to refer to a few strange cases of the rapid disappearance of whole orders of animals, which had a practically world-wide distribution at the time when the end came. Local extinction, or the disappearance of a group of restricted geographical range, may be explained by accidents of many kinds; but contemporaneous universal extinction of widely spread groups, which are apparently not affected by any new competitors, is not so easily understood. The Dinosaurs, for instance, are known to have lived in nearly all lands until the close of the Cretaceous period; and, except perhaps in Patagonia, they were always accompanied until the end by a typically Mesozoic fauna. Their remains are abundant in the Wealden formation of Western Europe, the deposit of a river which must have drained a great continent at the beginning of the Cretaceous period; they have also been found in a corresponding formation which covers a large area in the State of Bahia, in Brazil. They occur in great numbers in the freshwater Upper Cretaceous Laramie deposits of Western North America, and also in a similar formation of equally late date in Transylvania, South-East Europe. In only two of these regions (South-East England and West North America) have any traces of mammals been found, and they are extremely rare fragments of animals as small as rats; so there is no reason to suppose that the Dinosaurs suffered in the least from any struggle with warm-blooded competitors. Even in Patagonia, where the associated mammal-remains belong to slightly larger and more modern animals, these fossils are also rare, and there is nothing to suggest competition. The race of Dinosaurs seems, therefore, to have died a natural death. The same may be said of the marine reptiles of the orders Ichthyosauria, Plesiosauria, and

Mosasauria. They had a practically world-wide distribution in the seas of the Cretaceous period, and the Mosasauria especially must have been extremely abundant and flourishing. Nevertheless, at the end of Cretaceous times they disappeared everywhere, and there was absolutely nothing to take their place until the latter part of the Eocene period, when whales and porpoises began to play exactly the same part. So far as we know, the higher race never even came in contact with the lower race; the marine mammals found the seas vacant, except for a few turtles and for one curious Rhynchocephalian reptile (*Champsosaurus*), which did not long survive. Another illustration of the same phenomenon is probably afforded by the primitive Carnivora (the so-called Sparassodonta), which were numerous in South America in the Lower Tertiary periods. They were animals with a brain as small as that of the thylacines and dasyures which now live in Tasmania. They appear to have died out completely before they were replaced by the cats, sabre-toothed tigers, and dogs, which came down south from North America over the newly emerged isthmus of Panama at the close of the Pliocene period. At least, the remains of these old carnivores and their immigrant successors have never yet been found associated in any geological formation.

These various considerations lead me to think that there is also deep significance in the tendency towards fixity in the number and regularity (or symmetry) in the arrangement of their multiple parts, which we frequently observe in groups of animals as we trace them from their origin to their prime. It is well known that in certain of the highest and latest types of bony fishes the vertebræ and fin-rays are reduced to a fixed and practically invariable number for each family or genus, whereas there is no such fixity in the lower and earlier groups. In the earliest known Pycnodont fishes from the Lower Lias (*Mesodon*) the grinding teeth form an irregular cluster, while in most of the higher and later genera they are arranged in definite regular rows in a symmetrical manner. Many of the lower backboned animals have teeth with several cusps, and in some genera the number of teeth seems to be constant; but in the geological history of the successive classes the tooth-cusps never became fixed individual entities, readily traceable throughout whole groups, until the highest or mammalian grade had been attained. Moreover, it is only in the same latest grade or class that the teeth themselves can be treated as definite units, always the same in number (forty-four), except when modified by degeneration or special adaptation. In the earlier and lower land animals the number of vertebræ in the neck depends on the extent of this part, whereas in the mammal it is almost invariably seven, whatever the total length may be. Curiously constant, too, in the modern even-toed hoofed mammals is the number of nineteen vertebræ between the neck and the sacrum.

I am therefore still inclined to believe that the comparison of vital processes with certain purely physical phenomena is not altogether fanciful. Changes towards advancement and fixity which are so determinate in direction, and changes towards extinction which are so continually repeated, seem to denote some inherent property in living things, which is as definite as that of crystallisation in inorganic substances. The regular course of these changes is merely hindered and modified by a succession of checks from the environment and Natural Selection. Each separate chain of life, indeed, bears a striking resemblance to a crystal of some inorganic substance which has been disturbed by impurities during its growth, and has thus been fashioned with unequal faces, or even turned partly into a mere concretion. In the case of a crystal the inherent forces act solely on molecules of the crystalline substance itself, collecting them and striving, even in a disturbing environment, to arrange them in a fixed geometrical shape. In the case of

a chain of life (or organic phylum) we may regard each successive animal as a temporary excrescence of colloid substance round the equally colloid germ-plasm which persists continuously from generation to generation. The inherent forces of this germ-plasm, therefore, act upon a consecutive series of excrescences (or animal bodies), struggling not for geometrically arranged boundaries, but towards various other symmetries, and a fixity in number of multiple parts. When the extreme has been reached, activities cease, and sooner or later the race is dead.

Such are some of the most important general results to which the study of fossils has led during recent years; and they are conclusions which every new discovery appears to make more certain. When we turn to details, however, it must be admitted that modern systematic researches are continually complicating rather than simplifying the problems we have to solve. Professor Charles Depéret has lately written with scant respect of some of the pioneers who were content with generalities, and based their conclusions on the geological succession of certain anatomical structures rather than on a successive series of individuals and species obtained from the different layers of one geological section; but even now I do not think we can do much better than our predecessors in unravelling real genealogies. At least Professor Depéret's genealogical table of the Lower Tertiary pig-like Anthracotheriidae, which he publishes as an illustration of 'évolution réelle,' seems to me to be no more exact than several tables of other groups by previous authors which he criticises. His materials are all fragmentary, chiefly jaws and portions of skulls; they were obtained from several isolated lake-deposits, of which the relative age cannot be determined by observing the geological superposition; and they represent a group which is known to have lived over a large part of Europe, Asia, Northern Africa, and North America. There is therefore no certainty that the genera and species enumerated by Professor Depéret actually originated one from the other in the region where he happened to find them; he has demonstrated the general trend of certain changes in the Anthracotheriidae during geological time, but really nothing more.

Even when a group of animals seems to have been confined to one comparatively small region, where the series is not complicated by migration to and from other parts of the world, modern research still emphasises the difficulty of tracing real lines of descent. The primitive horned hoofed animals of the family Titanotheriidae, for example, are only known from part of North America, and they seem to have originated and remained there until the end. As their fossil skeletons are abundant and well preserved, it ought to be easy to discover the exact connections of the several genera and species. Professor Osborn has now proved, however, that the Titanotheres must have evolved in at least four distinct lines, adapted 'for different local habitat, different modes of feeding, fighting, locomotion, &c., which took origin, in part at least, in the Middle or Upper Eocene.' They exhibit 'four distinct types in the shape and position of the horns, correlated with the structure of the nasals and frontals, and indicative of different modes of combat among the males.' The ramifications of the group are indeed so numerous that the possibility of following chains of ancestors begins to appear nearly hopeless.

Among early reptiles the same difficulties are continually multiplied by the progress of discovery. About twenty years ago it began to appear likely that we should soon find the terrestrial ancestors of the Ichthyosauria in the Trias; and somewhat later a specimen from California raised hopes of obtaining them by systematic explorations in that region. During more recent years Professor J. C. Merriam and his colleagues have actually made these explorations, and the result is that we now know from the Californian

Trias a multitude of reptiles, which need more explanation than the Ichthyosauria themselves. Professor Merriam has found some of the links predicted between Ichthyosaurs and primitive land reptiles, but he has by no means reached the beginning of the marine group; and while making these discoveries he has added greatly to the complication of the problem which he set out to solve.

Serious difficulties have also become apparent during recent years in determining exactly the origin of the mammals. For a long time after the discovery of the Anomodont or Theromorph reptiles in the Permian-Trias of South Africa, it seemed more and more probable that the mammals arose in that region. Even yet new reptiles from the Karoo formation are continually being described as making an astonishingly near approach to mammals; and, so far as the skeleton is concerned, the links between the two grades are now very numerous among South African fossils. Since these reptiles first attracted attention, however, they have gradually been found in the Permian and Trias of a large part of the world. Remains of them were first met with in India, then in North America, and next in Scotland, while during the last few years Professor W. Amalitzky has disinterred so many nearly complete skeletons in the north of Russia that we are likely soon to learn more about them from this European country than from the South African area itself. Quite lately I have received numerous bones from a red marl in Rio Grande do Sul, Southern Brazil, which show that not merely Anomodonts, but also other characteristic Triassic land reptiles were likewise abundant in that region. We are therefore now embarrassed by the richness of the sources whence we may obtain the ancestors of mammals. Whereas some years ago it appeared sufficient to search South Africa for the solution of the problem, we are now uncertain in which direction to turn. We are still perhaps inclined to favour the South African source; but this is only because we know nothing of the Jurassic land animals of that part of the world, and we cherish a lingering hope that they may eventually prove to have included the early mammals for which we have so long sought in vain.

The mystery of the origin of the marine mammals of the order Sirenia and Cetacea appears to have been diminished by the discoveries of the Geological Survey of Egypt, Dr. Andrews and Dr. Fraas in the Eocene and Oligocene deposits of the Mokattam Hills and the Fayum. It is now clear that the Sirenians are closely related to the small primitive ancestors of the elephants; while, so far as the skull and dentition are concerned, we know nearly all the links between the early toothed whales (or Zeuglodonts) and the primitive ancestors of the Carnivora (or Creodonts). The most primitive form of Sirenian skull hitherto discovered, however, is not from Egypt, but from the other side of the world, Jamaica; and exactly the same Zeuglodonts, even with an associated sea-snake, occur so far away from Egypt as Alabama, U.S.A. The problem of the precise origin of these marine mammals is therefore not so simple as it would have appeared to be had we known only the Egyptian fossils. The progress of discovery, while revealing many most important generalities, has made it impossible to vouch for the accuracy of the details in any 'genealogical tree.'

Another difficulty resulting from the latest systematic researches is suggested by the extinct hoofed mammals of South America. The llamas, deer, and peccaries existing in South America at the present time are all immigrants from the northern continent; but during the greater part of the Tertiary period there lived in that country a large number of indigenous hoofed mammals, which originated quite independently of those in other regions. They seem to have begun in early Eocene times much in the same manner as those of the northern hemisphere; but as they became gradually

adapted for life on hard ground, they formed groups which are very different from those with which we are familiar in our part of the world. Some of them (*Proterotheriidae*) were one-toed mimics of the horses, but without the advanced type of brain, the deepened grinding teeth, the mobile neck, or the really effective wrist and ankle. Others (*Toxodontidae*) made some approach towards rhinoceroses in shape and habit, even with a trace of a horn on the nose. Until their independent origin was demonstrated, these curious animals could not be understood; and it is probable that there are innumerable similar cases of parallel development of groups, by which in our ignorance we are often misled.

It would be easy to multiply instances, but I think I have now said enough to show that every advance in the study of fossils reveals more problems than it solves. During the last two decades the progress in our knowledge of the extinct backboned animals has been truly astonishing, thanks especially to the great explorations in North America, Patagonia, Egypt, Madagascar, and South Africa. Whole groups have been traced a long way towards their origin; but with them have been found a number of previously unknown groups which complicate all questions of evolution to an almost bewildering extent. Animals formerly known only by fragments are now represented by nearly complete skeletons, and several which appeared to have a restricted geographical range have now been found over a much wider area; but while this progress has been made, numerous questions have arisen as to the changing connections of certain lands and seas which previously seemed to have been almost settled. The outlook both of zoology and of geology has, therefore, been immensely widened, but the only real contribution to philosophy has been one of generalities. Some of the broad principles to which I have referred are now so clearly established that we can often predict what will be the main result of any given exploration, should it be successful in recovering skeletons. We are no longer bold enough to restore an entirely unknown extinct animal from a single bone or tooth, like the trustful Cuvierian school; but there are many kinds of bones and teeth of which we can determine the approximate geological age and probable associates, even if we have no exact knowledge of the animals to which they belong. A subject which began by providing material for wonder-books has thus been reduced to a science sufficiently precise to be of fundamental importance both to zoology and to geology; and its exactitude must necessarily increase with greater and greater rapidity as our systematic researches are more clearly guided by the experience we have already gained.

British Association for the Advancement of Science

WINNIPEG, 1909.

ADDRESS

TO THE

ZOOLOGICAL SECTION

BY

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PRESIDENT OF THE SECTION.

I.

Charles Darwin.

THIS is the year of centenaries. Perhaps in no other year in history were so many men born destined to impress their genius on the literature, the politics, and the science of the world as in 1809. The number of literary men who first saw the light in that *annus mirabilis* is almost too long to mention—Mark Lemon, the genial editor and one of the founders of *Punch*; ‘Crimean’ Kinglake; John Stuart Blackie, till lately a well-known figure in Edinburgh; Monckton Milnes, the first Lord Houghton, ‘poet, critic, legislator, the friend of authors’ and the father of Lord Crewe who at present presides over that most important of all Government offices—that of the Colonies. One could prolong the list, and one must at least mention the names of Louis Braille, the inventor of the Braille type for the blind, of Fanny Kemble and of Elizabeth Barrett Browning, before passing on to remind you that this year is also the centenary of Tennyson who, with Browning, formed the twin stars of poetry during the reign of Queen Victoria, and who from his intimate knowledge of natural history and his keen power of observation was essentially the poet of Darwinism. Of his long-life friend, born the same year, Edward Fitzgerald, the translator—one feels almost inclined to say author—of Omar Khayyám, and of the gifted musician Mendelssohn there is no time to speak.

On this side of the Atlantic, and yet not wholly on this side, for he spent five impressionable school years at Stoke Newington, we have that

'fantastic and romantic' genius Edgar Allan Poe.¹ Later he studied at West Point, where surely he must have been as incongruous a student as James Whistler himself. We have also that kindly, humorous physician Oliver Wendell Holmes, a nature 'sloping towards the southern side' as Lowell has it. Amongst many recollections of literary men I cherish none more dearly than that I once entertained him in my Cambridge and once visited him in his.

Three other names stand out. William Ewart Gladstone, that leader of men, a politician and a statesman capable more than most men at once of arousing the warmest affection of his followers and the bitterest hatred of those who went the other way. Cultured as he was and widely read, he had his limitations, and although his tenacious memory was stored with the humanities of all the ages, he was singularly devoid of any knowledge of science. If we may paraphrase the words of Lord Morley in his estimate of Gladstone's writings, we would say that his place is not in science, 'nor in critical history, but elsewhere.'

Abraham Lincoln, the greatest man born on this continent since the War of Independence, was some ten months older than Gladstone. Both men were great statesmen, both men were liberators; for we must not forget that in many minds the help Gladstone gave to Italy in her struggle for freedom and union remains the most enduring thing he achieved.

Yet in externals how different! One the finished, cultured product of the most aristocratic of our public schools and the most ancient of our universities, the other little read in the classics or in mediæval and ecclesiastical lore, yet deeply versed in the knowledge of men and how to sway them. Rugged, a little rough if you like, humorous and yet sad, eminently capable, a strong man, and at heart 'a very perfect gentleman.'

On the same day, February 12th, upon which Lincoln first saw the light, was born at the 'Mount,' Shrewsbury, a little child destined as he grew up to alter our conceptions of organic life perhaps more profoundly than any other man has ever altered them, and this not only in the subjects he made his own, but in every department of human knowledge and thought.

Being as I am a member of Charles Darwin's own college, coming as I do straight from the celebration in which the whole world united to do his memory honour, it would seem meet that I should in this year of the centenary of his birth devote this address to a consideration of his life and of his work, and of such confirmation and modification of his theories as the work of the last fifty years has revealed.

As to the man, I can but quote two estimates of his character, one by a college companion who lived on terms of close intimacy with Darwin when at Christ's, the other the considered judgment of one who knew and loved and fought for Darwin in later life.

Mr. Herbert says:—

'It would be idle for me to speak of his vast intellectual powers . . . but I cannot end this cursory and rambling sketch without testifying, and I doubt not all his surviving college friends would concur with me, that he was the most genial, warm-hearted, generous, and affectionate of friends; that his sympathies were with all that was good and true; and that he had a cordial hatred for everything false, or vile, or cruel, or

¹ Poe lived from his eighth to his thirteenth year at the 'Manor House School,' Stoke Newington, at that time a village, now swallowed up by the metropolis. Poe described the place as he knew it, and his schoolmaster, Dr. Bransby, in *William Wilson*.

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mean, or dishonourable. He was not only great, but pre-eminently good, and just, and lovable.'

Professor Huxley, speaking of his name, says:—

'They think of him who bore it as a rare combination of genius, industry, and unswerving veracity, who earned his place among the most famous men of the age by sheer native power, in the teeth of a gale of popular prejudice, and uncheered by a sign of favour or appreciation from the official fountains of honour; as one who, in spite of an acute sensitiveness to praise and blame, and notwithstanding provocations which might have excused any outbreak, kept himself clear of all envy, hatred, malice, nor dealt otherwise than fairly and justly with the unfairness and injustice which was showered upon him; while, to the end of his days, he was ready to listen with patience and respect to the most insignificant of reasonable objectors.'¹

It has been somewhat shallowly said—said, in fact, on the day of the centenary of Darwin's birth—that 'we are upon very unsafe ground when we speculate upon the manner in which organic evolution has proceeded without knowing in the least what was the variable organic basis from which the whole process started.' Such statements show a certain misconception, not confined to the layman, as to the scope and limitations of scientific theories in general, and to the theory of organic evolution in particular. The idea that it is fruitless to speculate about the evolution of species without determining the origin of life is based on an erroneous conception of the true nature of scientific thought and of the methods of scientific procedure. For science the world of natural phenomena is a complex of procedure going on in time, and the sole function of natural science is to construct systematic schemes forming conceptual descriptions of actually observed processes. Of ultimate origins natural science has no knowledge and can give no account. The question whether living matter is continuous or not with what we call non-living matter is certainly one to which an attempted answer falls within the scope of scientific method. If, however, the final answer should be in the affirmative, we should then know that all matter is living; but we should be no nearer to the attainment of a notion of the origin of life. No body of scientific doctrine succeeds in describing in terms of laws of succession more than some limited set of stages of a natural process; the whole process—if, indeed, it can be regarded as a whole—must for ever be beyond the reach of scientific grasp. The earliest stage to which science has succeeded in tracing back any part of a sequence of phenomena constitutes a new problem for science, and that without end. There is always an earlier stage and to an earliest we can never attain. The questions of origins concern the theologian, the metaphysician, perhaps the poet. The fact that Darwin did not concern himself with questions as to the origin of life nor with the apparent discontinuity between living and non-living matter in no way diminishes the value of his work. The broad philosophic mind of the great master of inductive method saw too fully the nature of the task he had set before him to hamper himself with irrelevant views as to origins.

No well instructed person imagines that Darwin spoke either the first or the last word about organic evolution. His ideas as to the precise mode of evolution may be, and are being, modified as time goes on. This is the fate of all scientific theories; none are stationary, none are final.

¹ *Life and Letters of Charles Darwin*, Vol. II. 1887, p. 179.

The development of science is a continuous process of evolution, like the world of phenomena itself. It has, however, some few landmarks which stand out exceptional and prominent. None of these is greater or will be more enduring in the history of thought than the one associated with the name of Charles Darwin.

I cannot, indeed, attempt to weigh or estimate the influence and the far-reaching import of the work which all the world has been weighing and estimating during this year, the centenary of his birth and the jubilee of the *Origin of Species*. I cannot, to my intense regret, give you any personal recollections of Darwin, for though I think I once saw him in the streets of Cambridge, I have to my sorrow never been absolutely sure that this was so.

But in reading his writings and his son's most admirable Life, one attains a very vivid impression of the man. One of his dominant characteristics was simplicity—simplicity and directness. In his style he was terse, but he managed to write so that even the most abstruse problems became clear to the public. The fascination of the story he had to tell was enhanced by the direct way he told it.

One more characteristic. Darwin's views excited at the time intense opposition and in many quarters intense hatred. They were criticised from every point of view, and seldom has a writer been more violently attacked and abused. Now what seemed to me so wonderful in Darwin was that—at any rate as far as we can know—he took both criticism and abuse with mild serenity. What he wanted to do was to find the truth, and he carefully considered any criticism, and if it helped him to his goal he thanked the critic and used his new facts. He never wasted time in replying to those who fulminated against him; he passed them by and went on with his search.

In the development of the theories associated with Darwin's work the New World played a prominent part. Darwin's 'Wanderjahre' were spent on this side of the Atlantic. The central doctrine of evolution through natural selection was forced upon his mind by the studies and researches he made in South America during the voyage of the *Beagle*. The numerous observations in all departments of natural science and the varied forms of life he came across in this classical journey were the bricks with which he built many of his later theories. The storm of controversy which the *Origin of Species* awoke was at least as violent in America as in Great Britain, and we must not forget the parts played by men like Hyatt, Fiske, Osborn, and many others, and above all by Asa Gray and by Brooks of Baltimore, whose recent death has robbed America of perhaps her greatest Darwinian.

It is a somewhat remarkable fact that whilst the works of Darwin stimulated an immense amount of research in biology, this research did not at first take the line he himself had traced. With some exception the leading zoological work of the end of the last century took the form of embryology, morphology, and palæontology, and such subjects as cell-lineage, 'Entwickelungsmechanik'; the minute structure of protoplasm, life-histories, teratology, have occupied the minds of those who interest themselves in the problems of life. Along all these lines of research man has been seeking for the solution of that secret of nature which at the bottom of his heart he knows he will never find, and yet the pursuit of which is his one-abiding interest. Had Frank Balfour lived we should, I think, have sooner returned to the broader lines of research as practised by Darwin, for it was his intention to turn himself to the physiology—using the term in its widest sense—of the lower animals. Towards the end of the nineteenth century, stimulated by Galton, Weldon began those series

of measurements and observations which have culminated in the establishment, under the guidance of his friend and fellow-worker, Karl Pearson, of a great school of eugenics and statistics in London. With the beginning of the twentieth century came the rediscovery of Mendel's facts, and with that an immediate and enormous outburst of enthusiasm and of work. Mendel has placed a new instrument in the hand of the breeder, an instrument which, when he has learnt to use it, will give him a power over all domesticated animals and cultivated crops undreamt of before. We are getting a new insight into the working of heredity and we are acquiring a new conception of the individual. The few years which have elapsed since men's attention was redirected to the principles first enunciated by the Abbot of Brunn has seen a great school of genetics arise at Cambridge under the stimulating energy of Bateson, and an immense amount of work has also been done in France, Holland, Austria, and especially in the United States. As the work has advanced, new ideas have arisen and earlier formed ones have had to be abandoned; this must be so with every advancing science; but it has now become clear that mutations occur and exist especially in cultivated species, and that they breed true seems now to be established. In wild species also they undoubtedly occur, but whether they are so common (in uncultivated species) remains to be seen. If they are not, in my opinion a most profitable line of research would be to endeavour to determine what factor exists in cultivation which stimulates mutation.

To what extent Darwin's writings would have been modified had Mendel's work come into his hands we can never know. He carefully considered the question of mutation, or, as they called it then, saltation, and as time went on he attached less and less importance to these variations as factors in the origin of species. Ray Lankester has recently reminded us that Darwin's disciple and expounder, Huxley, 'clung to a little heresy of his own as to the occurrence of evolution by saltatory variation,' and there must have been frequent and prolonged discussion on the point. That 'little heresy' has now become the orthodoxy of a number of eager and thoughtful workers who are at times rather aggressive in their attacks on the supporters of the old creed. 'That mutations occur and exist is obvious to everyone, but that they are of frequent occurrence under purely natural conditions is,' Sir William Thiselton-Dyer thinks, 'unsupported by evidence.' The delicate adjustment between an organism and its natural surroundings suggests that sudden change of a marked kind would lead to the extinction of the mutating individual. As far as I can understand the matter in dispute, Darwin and his followers held that evolution had proceeded by small steps, for which we may accept de Vries' term fluctuations; whilst the Mutationists hold that it has advanced by large ones, or mutations. But it is acknowledged that mutations are not all of the same magnitude, some, *e.g.*, albinism; brachydactyly in man; dwarf habit or glabrousness in plants may be large; others, *e.g.*, certain differences in shade of colour or in size, are insignificant, and indeed Punnett has suggested that under the head of fluctuating variation we are dealing with two distinct phenomena. He holds that 'some of the so-called fluctuations are in reality mutations, whilst others are due to environmental influence.' He thinks the evidence that these latter are transmitted is slender, and later states that 'Evolution takes place through the action of selection on these mutations. Where there are no mutations there can be no evolution.' The disagreement about the way in which evolution has proceeded has perhaps arisen from a misunderstanding as to the nature of the two kinds of variation described respectively as mutations and fluctuations. Mutations are variations

arising in the germ-cells and due to causes of which we are wholly ignorant; fluctuations are variations arising in the body or 'soma' owing to the action of external conditions. The former are undoubtedly inherited, the latter are very probably not. But since mutations (using the word in this sense) may be small and may *appear* similar in character to fluctuations, it is not always possible to separate the two things by inspection alone. The whole matter is well illustrated by the work of Johannsen on beans. He found that while the beans borne by any one plant vary largely in size, yet if a large and a small bean from the same plant are sown, the mean size and variability of the beans on the plants so produced will be the same. The differences in size are presumably due to differences of condition and are not inherited. But if two beans are sown, one from a plant with beans of large average size, and one from a bean of small average size, the bean plant whose parent had the high average will bear larger beans than the one from the parent with small average beans. The faculty of producing a high or low mean size is congenital, is a mutation in the sense used above, and is inherited. It is no doubt unfortunate that the word mutation has been used in several different senses, for it seems to have led to most regrettable confusion and misunderstanding.

As I have said, in such a year, and in my position, I ought perhaps to have devoted the whole of this address to the more philosophical side of our subject; but, in truth, I am no philosopher, and I can only say, as Mr. Oliver Edwards, 'an old fellow-collegian' of Dr. Johnson's said to the 'great lexicographer' when they met after nearly half a century of separation: 'I have tried too in my time to be a philosopher, but I don't know how, cheerfulness was always breaking in.'

II.

Organising Zoology.

I now turn to a subject of the greatest moment and of the greatest difficulty, and one on which there is little general consensus of opinion. The question I wish to raise is this—are the zoologists of the world setting about their task in an economic and efficient way?

We live surrounded by a disappearing fauna. Species are disappearing from the globe at a greater rate than even the most ardent mutationist claims they are appearing. To mention but a few striking cases: The European beaver has almost gone, though a few linger on around the periphery of the Continent. Norway, the lower Danube, Eastern and Arctic Russia still harbour them, and a very few are said still to inhabit the Rhine and the Rhone. The European bison is now represented by a few wild specimens in the Caucasus. The American bison is reduced, and that by the deliberate and calculated action of man, to a few herds most carefully preserved by Government; the largest of these, containing some 600 heads, is now at the National Park at Wainwright. Equally deliberate and equally calculated is the destruction of the fur-seal which threatens soon to be complete. The Greenland sealing is almost a thing of the past. In 1860 British vessels killed 68,278 seals; in 1866, 103,758; and this went on until 1895, when the pursuit was abandoned by the British, it being no longer found to pay them, though Norwegians still continue 'sealing.' In 1859 19 vessels sailing from British ports killed 148 whales; in 1881 12 vessels killed 48 whales; last year 6 Dundee vessels killed but 15,

and the year before that but 3. The whalers sailing from Newfoundland ports killed 1,275 whales in 1904, 892 in 1905, and only 429 in 1906.

At the present time certain Norwegian whaling companies have been for the last few years actively at work in the Shetlands, and are killing off as fast as they can the common rorqual (*Balaenoptera musculus* L.), the lesser rorqual (*B. rostrata*), Sibbald's rorqual (*B. sibbaldi* Gray), the cachalot (*Physeter macrocephalus* L.), the humped-back whale (*Megaptera boops* L.), and, when they can reach him, the Atlantic right whale (*Balaena mysticetus* L.). These are killed primarily for their blubber, but the economy of the factories rivals that of the Chicago pork packing industries. Nothing is wasted, the flesh is made into sausages, which are readily eaten in Central Europe, and the bones are ground up to make manure. No animal which produces but few young can withstand such persistent and organised attacks on the part of man and I fear, before many years are passed, many species of whale will be extinct. At the present moment the two right whales seem almost on the verge of extinction, and *Balaena mysticetus* will probably go before *B. australis*. Nothing shows this more clearly than the price of whalebone, which has gone up in the last eighty-four years from 56*l.* per ton to 2,100*l.* per ton, or from 12*cs.* a pound to \$4.90, and in some years to \$5.80 a pound. The number of pounds on sale in the United States has dropped from 2,916,500 in 1851 to 96,600 in 1906. With the whales will disappear the whale-lice and the whole of the very interesting parasitic fauna which inhabit their vast interiors.

The disappearance of the large game from enormous tracts of country in Africa is too well known to delay us. The elephant, except where preserved in the Litzikama Forest, near Mossel Bay, and in the Addo Bush, near Port Elizabeth, is exterminated south of the Limpopo. The price of ivory again is a measure of the nearness of its extinction. The best pieces, which are used for billiard balls, have risen in price from 55*l.* a cwt. in 1882 to an average of 100*l.* a cwt. in 1908. The common and the brindled gnu (*Connochoetes taurinus*) are fated to follow the extinct quagga. The blesbok (*Damaliscus albifrons*), formerly found in thousands in Cape Colony, the Transvaal, and Bechuanaland, is now very rare and seems doomed. The giraffe has long been driven out from South Africa, though it still roams over large tracts of country in East and Central Africa.

Perhaps the most striking case of the disappearance of a mammalian fauna is that presented in Western Australia. Here many districts are now said to be entirely devoid of indigenous mammals, and this depletion is in the main an affair of only the last thirty years, and many of the local extinct forms are still remembered by the older natives and colonists. Mr. Shortridge, a collector who has worked for some years in South-west and Western Australia, writes in a letter: 'The entire disappearance of so many species over such large tracts of country is generally considered to be due to some epidemic perhaps brought into the land by introduced animals. It is to be noted that they have died out chiefly in the dry regions, where, except for the introduction of sheep, there has been very little alteration in the natural conditions. Rabbits, although already very numerous in the Centre and South-east, have not as yet found their way to the North-west.' Amongst the mammals which have almost, if not quite, disappeared from West Australia are the banded wallaby (*Lagostrophus fasciatus*), the hare wallaby (*Lagochestes hirsutus*), the rat-kangaroos (*Potorous gilberti* and *P. platyops*). The indigenous rats and mice of Australia are disappearing even faster than the marsupials, and it seems probable that many will not be heard of again.

A very few years ago the ship employed by the company which is

TRANSACTIONS OF SECTION D.

exploiting the phosphates of Christmas Island introduced the brown rat (*M. decumanus*) there. Within a short time the two indigenous rats first collected by Mr. C. Andrews, of the British Museum, named *Mus macleari* and *Mus novitatis*, were wiped out of existence. The same animal having been introduced in North America, is gradually spreading, and as it spreads the native fauna of Muridae is slowly vanishing.

To adorn our ladies' heads some of the most beautiful of birds are being systematically exterminated. In the London market alone were sold last year some 50,000 sooty terns (*Sterna fuliginosa*, *S. anacantha*, and *S. bonata*), 20,000 specimens of the crowned pigeon (*Goura*) from New Guinea, their sole habitat, immense numbers of 'osprey' feathers, egret and heron, and over 50,000 birds of paradise, or more than double the number of the year before.

I have no time to continue this melancholy record, but it could be prolonged almost indefinitely.

When we reflect how greatly we treasure every scrap of knowledge we can glean about such recently extinct animals as the *Rhytina*—Steller's sea-cow—the dodo, the great auk, we must see that if it be impossible to check the gradual disappearance of those animals doomed to extinction, we should at least monograph them and take every care that what can be permanently kept of their structure should be kept. In respect to the recording of the habits and physical features of a disappearing race, the anthropologists are setting an example which the zoologists would do well to follow.

We are living with a disappearing fauna around us, and numerous as the museums of the world are, and skilled and painstaking as the curators of these museums are, they are both wholly inadequate to deal with the material at hand. Some dozen years ago Dr. Günther made a very careful estimate of the number of species of animals which were known in the years 1830 and 1881. I summarise his table:—

Number of Species known in the years 1830 and 1881.

	1830.	1881.
Mammalia	1,200	2,300
Aves	3,600	11,000
Reptilia and Batrachia	543	3,400
Pisces	3,500	11,000
Mollusca	11,000	33,000
Bryozoa	(40)	120
Crustacea (year 1840).	(1,290)	7,500
Arachnida	1,408	8,070
Myriapoda	450	1,300
Insecta	49,100	220,150
Echinodermata (1838)	(230)	1,843
Vermes (1838)	(372)	6,070
Coelenterata (1834)	500	2,200
Porifera (1835)	(50) <i>say</i>	400
Protozoa (1838—44) <i>say</i>	(305) „	3,300
	73,588	311,653

Taking an average year between 1881 and the present date, but rather nearer the latter, because each year the number of newly described species becomes larger, Dr. Sharp tells me that according to the zoological record 12,449—let us call it 12,450—new species were described in the year 1897.

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Number of new Species described in the year 1897.

Mammalia	285
Aves	105
Reptilia and Batrachia	140
Pisces	148
Mollusca	1,077
Brachiopoda	7
Bryozoa	6
Crustacea	239
Arachnida	659
Myriapoda	275
Insecta	8,364
Echinodermata	491
Vermes	294
Coelenterata	164
Porifera	95
Protozoa	100
	<hr/>
	12,449

This number, however, includes fossils which I do not think were included by Dr. Günther. We might deduct 450 for them if we wish to confine our attention to living animals. This leaves us 12,000. If we multiply this by 27, the number of years which have elapsed since Dr. Günther made his estimate, we find a total of 324,000. This number is possibly too large, as it makes no allowance for synonyms, still it is a rough indication that since 1881 the number of described species has been doubled. Isolated groups, such as the mammals, treated in the same way, give us fairly similar results, so that now we may, I think, say that there are over 600,000 described species of living animals.

It thus appears that during the fifty-one years in the middle of the last century the number of known species grew by some 238,000, giving an average increase of a little under 5,000 per annum. At the present day there are far more workers in the field than there were thirty years ago, museums have multiplied, and there are many more zoologists, and it is now estimated that the number of species annually described and named amounts to some 12,000.

The number, large as it seems, is, however, but small in comparison with the number of species collected and deposited in museums where no one has time to work them out. It is still smaller in comparison with the vast numbers of species as yet uncaptured. Dr. Sharp, in 1895, calculated that there were a quarter of a million known and described insects. This was an increase of 30,000 over Günther's figures of fifteen years before, but he states that in his opinion this quarter of a million is but one-tenth of those which exist.

With the exception of the larger mammalia—though the Okapi warns us the exception may yet prove the rule—there is no group of animals which may not yield us new surprises—no group which we can regard as well worked out, though naturally some are better known than others. What, then, are the zoologists of the world doing to record the animal life around them? One thing of late is certainly an improvement. During last century the great zoological collections were in the main increased and augmented by the chance gifts of hunters and sportsmen, whose chief object in their expeditions was not zoology but what is termed 'sport.' Many valuable gifts are still received from such sources, but it is now recognised that we must not in these matters trust to the sportsman alone.

The plan of attaching trained naturalists and experts in taxidermy to an expedition avowedly meant for other purposes is good, and is well exemplified by Mr. Roosevelt's 'safari' in East Africa at the present time. We may hope that we may never again see an expedition without a single trained naturalist on its staff, such as the last Stanley led across Africa. A still better plan is to send out expeditions of trained naturalists to do definite pieces of work. Such expeditions as Andrews and Foster Cooper and Osborn to the Fayum for fossils, of Cunningham and Boulenger to the same region to investigate the fauna of the lake, or Wollaston and his companions to the Ruwenzori district, yield a harvest one hundred times more abundant than the best of other schemes.

Yet even here I would plead for a little more organisation. One must not suggest too rigid a scheme, and it is to be hoped that in the future, as in the past, there will always be found wealthy men willing to devote their energies to the advancement of zoology. Such work as has been done by Mr. Godman on the fauna of Central America, one of the richest regions in the world, and now, owing to his munificence, one of the best known. The stately array of volumes embodying these results is paralleled by the magnificent monographs in which the results of the Prince of Monaco's marine researches are recorded, and by the monographs of the Princeton Expedition to the Argentine, financed by one of the richest of the millionaires of the United States. We trust that such enterprises will always continue.

With regard, however, to expeditions financed from public funds which are sent out officially, it might be possible to have more international co-operation. Just as the members of the Geodetic Survey meet from time to time and determine the next step to be taken in the triangulation of the world, so it seems to me might the members of the chief museums of the world meet, say, triennially, and draw up certain thought-out plans for the exploration of the zoological world.

With regard to working out the material when collected, the existing museums of the world are too few, and their staffs are too small to deal not only with the huge collections which are constantly pouring into their buildings, but even with the accumulated stores already housed there. In our smaller state museums it is not uncommon to find men who are responsible for the whole of the Arthropoda. Only within the last few months I have had to try and find a curator for a Metropolitan museum who was expected to be a specialist in fishes, molluscs, and arachnids. Now is it possible to expect such men, able and zealous as they are, to accurately determine species in these vast and complex groups? My own feeling is—but I fear I shall carry no one with me—that we must specialise still further. I should like to see each of the great classes of the animal kingdom assigned to one of the great museums of the world. Just as an example—which is only an example, possibly a bad one—I suggest that all the type specimens of Amphibia be sent to one museum, say, if you like, that of Berlin or St. Petersburg; in return for this that museum should distribute to others its types of fish, birds, etc. Then, at this museum there would arise a series of specialists capable of deciding swiftly and accurately on the validity of the claims of any new species of amphibian that may be advanced. Again, a student of Amphibia, instead of wandering round the museums of the world if he wishes to study species, would find all he wants within the four walls of one building. When once the type is described and deposited, it would be the duty of the museum to distribute co-types and accurately named specimens of the same species to other museums in some recognised order. Smaller groups might be allocated to smaller museums, *e.g.*, the fleas to Tring and the ticks to Cambridge—at both of these places there are now

specialists working out world collections of these pests. What I want is a world's Clearing House for animals. I know I shall be told that my suggestions can never be realised, that international jealousies would prevent such a scheme being adopted, that I am proposing to fetter research. I admit the difficulties, but do not regard them as insuperable. When you recall the international Clearing Houses for the Postal and Telegraphic service, for the banking of the world, and when we reflect what private enterprise does, under the name of Lloyd's, for the shipping of the world, how it registers and describes and certifies with a minuteness not surpassed by any maker of species, each ship in the world; how, through its signal stations and by other means, it follows the daily course of each vessel, so that at any hour of any day it can state where, under normal circumstances, that vessel is, it does not seem to me impossible to come to some understanding as to dealing with the animals of the world. Only by some such means can we hope to cope with the problem before us.

One other fruitful source of 'waste of time' I will mention. That is the debateable matter of zoological nomenclature, more especially the questions of synonymy. The British Association at their last meeting passed a resolution on the proposal of Mr. G. A. Boulenger in the following sense:—

'The undersigned zoologists, whilst fully realising the justice and utility of the rule of priority in the choice of scientific names for animals, as first laid down by a committee of the British Association in 1842, wish to protest against the abuse to which it has been put as a result of the most recent codes of nomenclature, and consider that names which have had currency for a great number of years should, unless preoccupied, be retained in the sense in which they have been universally used. Considering the confusion that must result from the strict application of the rule of priority, they would welcome action leading to the adoption of a scheme by which such names as have received the sanction of general usage, and have been invariably employed by the masters of zoology in the past century, would be scheduled as unremovable.'

Mr. G. A. Boulenger expressed disapproval of the extreme application of the rule of priority in zoological nomenclature on the ground that it had already produced much mischief under the pretence of arriving at ultimate uniformity. The worst feature of the abuse of this rule is not so much the bestowal of unknown names on well-known animals as the transfer of names from one to another, as in the case of *Astacus*, *Torpedo*, *Holothuria*, *Simia*, *Cynocephalus*, etc., so that the names which were uniformly used by Cuvier, Johannes Müller, Owen, Agassiz, Darwin, Huxley, and Gegenbaur would no longer convey any meaning; very often they would be misunderstood, and the very object for which Latin or Latinised names were introduced would be defeated.

The International Congress of Zoology takes, I believe, a somewhat sterner view, but they are engaged in drawing up a list of names which they hope will be accepted for all time. I for one am prepared to accept them, and I am prepared to go further. I would ask the International Congress if, instead of drawing up a list of single species, or perhaps in addition to it, they would draw up a list of systematic monographs, the names in which may be regarded as final. After all, modern classification began with a book, and it would take no longer, or very little longer, to sanctify a book which may contain diagnoses of hundreds of species than to sanctify the single species. The idea is due to Mr. Cyril Crossland, and he suggests—he was working at Chaetopods—that such works as Claparède's *Annelides Polychètes du Golfe de Naples*, Ehler's *Die Borstenwürmer*, McIntosh's *Monograph of the British Annelids* be accepted. Possibly whole categories of books might be considered, such as the *Challenger Reports*, and especially

Das Tierreich, the admirable volumes of which we owe to the enterprise of the Berlin Zoological Society. Such a scheme would certainly cause some minor injustices, but every scheme does that. The immense advantage of allowing a researcher to readily determine and give an accepted name to an animal he is investigating without waiting weary days in struggling through a vast and scattered literature for the sake of synonymy would surely far out-balance any temporary injustice.

One last phase of my subject and I have finished with what I want to say on the subject of organising zoology. In Europe the great museums of our metropolitan towns are State museums, endowed by the State, managed by the State, and in Great Britain and Ireland staffed and curated by the State; that is to say, the officials at the museums are Civil Servants. Let us consider for a moment what that means, and let us take the British Museum, which, in its entirety, is second to none in the world as an example of a State museum.

The British Museum was established by an Act of Parliament in the year 1753 (26 Geo. II. cap. xxii). This Act sanctioned the purchase of collections and library of Sir Hans Sloane, that prince of collectors, for the comparatively insignificant sum of 20,000*l*. In fact, Sir Hans left his magnificent collection of natural objects, which, twenty years before his death, amounted to just under 70,000 specimens, his library of 40,000 printed volumes and 4,100 manuscripts, to the nation, on condition that 20,000*l*., about one-fourth of the estimated value of the collections, be paid to his executors. Under the above-mentioned Act 10,000*l*. were paid to each of Sir Hans Sloane's daughters, Mrs. Stanley and Lady Cadogan. The same Act provided 10,000*l*. for the purchase from the Duchess of Portland, heiress of the second Earl of Oxford, of the Harley collection of charters and manuscripts, which were then in the market, and other moneys for the purchase and repair of Montagu House, Bloomsbury, and for maintenance. The Act incorporated with the Museum the Cottonian Library at Westminster, which, by an Act of Parliament of William III.'s reign, was under the care of trustees, chief amongst whom were the Archbishop of Canterbury, the Lord Chancellor, and the Speaker; the money was raised by a lottery, and the museum was opened in January 1759, just 150 years ago.

Now it will be noticed that at its formal birth the museum consisted of about two equal parts—on the one hand books and manuscripts, and on the other what used to be called 'natural objects.'

The 'General Repository,' as the Act of George II. called it, was placed in the hands of a body of trustees, now forty-nine in number, three of them relics of William III.—namely, the Archbishop of Canterbury, the Lord Chancellor, and the Speaker of the House of Commons, are trustees by virtue of office. These three are known as the principal trustees; there are twenty-one other trustees in virtue of their office *e.g.*, the Bishop of London, the President of the Royal Society and Royal Academy, and so on—one is appointed by the Crown, nine represent the families of donors, and fifteen are co-opted. So large and unwieldy a body cannot, as a whole, transact the business of a great museum, and they have largely delegated their functions to a standing committee of the three principal trustees and fifteen annually appointed representatives.

Now the manner of appointing to the museum is this. The junior members of the staff are selected as the result of examination, and when appointed they become Civil Servants. Not a bad thing in itself, but bad for a man of science. He, through no fault of his own, becomes entangled in red tape; above all, he must not make himself a nuisance; *trop de zèle* must be avoided, his enthusiasms tend to become checked, he is perpetually observing what is called 'official reticence,' and he perforce spends his days

in performing routine work during routine hours. No amount of skill and ability—and the staff at the museum is both skilled and able—hastens his promotion. This is a matter almost entirely of seniority. In fact, the conditions of the Civil Service are incompatible with that freedom to research in any line that proves most suggestive, and with that absence of outside control which alone makes scientific research on a large scale possible.

The appointment to the senior staff, the keepers or heads of departments, the Director of the Natural History Museum, and the chief librarian, are vested in the three chief principal trustees. This takes us back to the reign of William III. and the Cotton Library at Westminster. No doubt the then Archbishop, the then Lord Chancellor, and the then Speaker, both from propinquity and from their abilities and training, were quite the best men who could be found for this position of trust towards this library. Probably the present holders of these exalted positions—positions which they most worthily fill and which gives two of them precedence after royalty in all Britain—are most fully endowed with the qualities which fit them to elect the senior staff for the library and for the collections of works of art and of antiquities at Bloomsbury. I doubt if the same eminent qualities enable them to deal equally satisfactorily with the higher posts in the Natural History Museum. If Parliament, or indeed any other body, were framing a scheme for the management of a great museum of science at the present time, I do not think it would occur to anyone that the holders of the exalted offices I have mentioned were specially fitted, either by the knowledge of the pressing scientific needs and problems of the moment or by their intimacy with the men of science of to-day, to be the most competent electoral body to choose keepers in geology, mineralogy, botany, and zoology. And, indeed, the existing arrangement has broken down. I do not know how long before Sir E. Ray Lankester's resignation of the joint posts of Director and Keeper in Zoology, in December 1907, it became known to the trustees that that resignation was imminent, but I do know that it was talked about and written about months before that date. Yet after the resignation took effect one whole year elapsed before the trustees appointed a Keeper in Zoology; for twelve months there was no head of a department which contains collections unrivalled in the world. It took the trustees about six months longer to find a Director, and for about eighteen months the charge of this great museum of natural history was vested, under the trustees, in the Chief Librarian at Bloomsbury.

As Professor Ronald Ross could testify, after scientific research has placed it within the power of man to exterminate so deadly a disease as malaria, the real fight begins; and the real fight is to persuade the authorities to adopt and enforce the measures which are offered them gratis. There is a case in point, if I am not misinformed, on this continent at the present time. It has been known since the time of the making of the St. Gothard Tunnel that lasting and often fatal disease is caused by a small intestinal worm, known as the tunnel-worm or hook-worm. Within the last few years Dr. Wardell Stiles has shown quite clearly that the unhappy condition of 'poor white' of the Southern United States is due largely to their being affected by this hook-worm. Their bodies and their intellects are arrested in their development, and the adults amongst them are unable to understand the prophylactic measures he advocates, but the children can be taught if the proper organisation existed for teaching them. Many of the Southern States are friendly to the movement, and I know of no greater service that the central Government of the United States could confer upon the inhabitants of these southern States, in which, as is well known, President Taft takes the deepest interest, than that of detailing Dr. Stiles for several months a year to organise and control this movement.

If this could be done, I believe—and I am here speaking of those things that I do know—the United States Government would confer on their own people a benefit as great as they conferred on other nations when they freed Havana of yellow fever and Panama of malaria.

In concluding this part of my address I wish to say as emphatically as I can that if science is to take its proper place in the polity of the nation we must endeavour to have men of scientific training, or at least of scientific sympathies, in the Government and also in the Government offices.

I cannot recollect the name of one single Minister trained in natural or physical science amongst the numerous members of His Majesty's Governments of the last thirty-five years. It is not so very long ago—I am glad to say that one of the actors of my little story is still with us—that Sir Joseph Hooker, then Director of the Royal Gardens at Kew, was walking through the grounds with Mr. Ayrton, President of the Board of Works, which in those days was the Government Department responsible for Kew. They happened to run across Mr. Bentham, the great authority upon the classification of plants. Sir Joseph introduced him to the President, adding, 'he works in our herbarium.' 'Dear me,' said the President, 'I hope you don't get your feet wet.' Now I do not want for a moment to suggest that our present genial President of the Board of Works—whose official connection with Kew has been long severed—would not readily distinguish between an herbarium and an aquarium, but what I do wish to emphasise is that this ignorance of some of the most elementary details of scientific method exists in many of our rulers and in many of our permanent officials—not in all by any means, I know of some most notable exceptions—but in many. It is but human to distrust what we cannot understand, and it is this lack of understanding which is largely retarding progress at the present day.

III.

International Ocean Research.

As an example of international co-operation in scientific research I may take the investigations which have been going on for the last seven years in the Baltic Sea, in the North Sea, and in that greater Norwegian Sea which stretches from the western coast of Norway north to Spitzbergen and westward beyond Iceland and the Faroes. In this inquiry no less than ten nations—in fact, all those whose shores touch these seas—have had a share—England, Scotland, Norway, Sweden, Finland, Russia, Germany, Denmark, Holland, and Belgium—and since most of these countries have a special steamer equipped for research and under the command of men trained in scientific methods, it has been possible to collect a mass of facts connected with the seas of Northern Europe such as has never been got together before for any similar area of the ocean.

The aim of those responsible for the scheme of work was to obtain as complete a survey as possible of the physical and biological conditions of the seas in question. They wanted to know the direction of ocean currents, both superficial and along the bottom; the variations in the degree of salinity of the water in time and in space; the nature of the sea-bottom, and whether this could be correlated with the fauna, sessile or moving, found upon it, and whether this fauna reacted on the prevalence or absence of food-fishes; the influence of depth, salinity, and temperature on the fauna; the seasonal variations and fluctuations of the small floating organisms often called the plankton; the life-history of our food-fishes, where and when they deposited their ova; what became of the ova; the

distribution of the larval stages ; the age at which the fish become mature, and their average length of life.

Then, again, it was hoped that much could be learned about the influence of man's activity on the sea. The relative depletion of the fish population caused by different modes of fishing ; the intensity of trawling ; how often does the trawl pass over the same ground in a given time ? The question whether or no the seas are being over-fished, and, if so, what measures can be taken to lessen this evil, either by close time, limiting the size of fish captured, or by artificial fish-breeding. Many of these last-named problems concern the legislator as much as the man of science. The function of the latter is to provide facts upon which the administrator may act.

Such a vast task as was set out by the International Council in 1902 has necessitated an immense organisation. Some eight or ten steamers are employed making periodic voyages, under the direction of trained men of science. Enormous numbers of temperature-readings, investigations into the speed and direction of currents, and chemical analyses of sea-water have been recorded, and thousands of samples of the bottom, of the animals and plants living thereon, of fish in all stages, millions of fish ova, have been collected and accurately determined. To work up such an amount of material has occupied the attention of a large number of naturalists. Each country has at least one large laboratory devoted to this work, and their results are co-ordinated and generalised by the central bureau. The English part of the work was entrusted by the Lords of the Treasury to the Marine Biological Association, and has been carried out under the direction of Dr. E. J. Allen and Professor Walter Garstang at our laboratories at Plymouth and Lowestoft.

Although all the ten countries are working upon what is, broadly speaking, a common plan, each has had its own special problems. In addition to carrying out the broad outlines of an international scheme, they have specialised along lines indicated by their own needs, and have attacked problems whose solution affected their own special food supply. Thus Norway, where the old open fishing-boat is being replaced by the modern, decked trawler, has especially studied the cod and the saithe, the haddock and the herring, and has devoted much time and labour to the discovery of new fishing grounds, and has successfully done this along the Norse coast, in the Arctic circle, and on the banks between the Faroe Islands and Iceland. They have further established a trade in *Pandalus borealis*, allied to the prawns, which are taken in the deep waters off Norway, and are now to be bought in most fishmongers' shops in Great Britain.

In a similar way the Danes have tracked the eels as they leave the estuaries of the great rivers of Central Europe across the North Sea to the deep Atlantic off the West of Ireland, just beyond the 1000 fathom line. In these depths they spawn and the resulting larval form, the *Leptocephalus*, long thought to be a separate genus, lives there for a while, until, gradually changing into an elver, it retraces by some mysterious instinct its parents' path across the ocean and regains the fresh-water rivers which those parents had left.

The English share of the investigation is limited to that part of the North Sea which lies south of the latitude of Berwick, and for the most part to the western half of these seas and to the English Channel ; the latter, as we shall see, is a very important area. The work, so far as it has been specialised, deals, in the North Sea, largely with the plaice, with the food of fishes generally, and with the character of the deposits forming the sea-floor, with the creatures growing thereon. In the Channel the English worker is entirely responsible for the study of the hydrography of

the water, which, entering the North Sea through the Straits of Dover, contributes greatly to its mass.

As a result of Professor Garstang's investigations, an important spawning ground of the plaice has been located in the southern bight of the North Sea; the migration of both sexes has been traced to these grounds on the advent of the spawning season, and their return to their feeding grounds in the spring has been followed. During the spawning season it is usual to catch more males than females on the spawning grounds, possibly because at this time the female is inert and elusive, whilst the male is unusually active.

The course of the ova has been traced, chiefly by the Dutch investigators, as they drift towards the shallow fringe of coastal water, by far the greater number along the continental coast. Here the young fry grow up, and after attaining a certain size they leave the shallow coastal waters for the deeper seas off shore. Comparatively few of these, however, reach the feeding ground of the Dogger Bank, and Garstang has been able to show that by carrying the young plaice in steamers and transplanting them at the proper time on to this rich feeding ground, their rate of growth can be greatly accelerated and thus their market value largely increased, just as Dr. Petersen has done in the case of plaice on Thisted Breeding.

A few years ago there was no reliable method of determining the age of fish. Petersen's method of arranging the measurements of a large number of specimens in a scale according to size, when they resolved themselves into certain groups, which were considered to coincide with age classes, has been superseded by the discovery of Reibisch, Heincke, and others, that many of the bones, the scales, and the otoliths of fishes show annual age rings, like those found in the trunk of a tree or in the horns of cattle. By laboriously counting the rings on the otoliths of thousands of plaice, Dr. Wallace and others have been able to determine their rate of growth, and to show that some specimens attain the age of twenty-five and even twenty nine years. Similar investigations have shown that the sexes have a different rate of growth. The age at maturity is found to differ in different regions, but in the majority of cases Wallace found that the males are sexually mature (four to five years) a year before the female is capable of spawning (five to six years). We can now correlate age with size and with weight.

The migrations of the plaice and of other fish and their rate of growth depend, amongst many other factors, upon their food supply. And the nature of the food of fishes has recently been re-investigated in the North Sea. I give some of Todd's results, which were made by the examination of some thousands of fish of thirty-one species. Of these I select three—the cod, the plaice, and the dab.

Percentages of stomachs containing various kinds of food.

Cod.

Size of fish in cm.	0-15	15-30	30-60	60
Pisces	0 p.c.	11 p.c.	52 p.c.	67
Mollusca	0	2	16	4
Crustacea	100	95	67	63
Polychaeta	0	9		26

Plaice.

Size of fish in cm.	0-10	10-20	20-30	30
Pisces	0 p.c.	1 p.c.		5 p.c.
Mollusca	17		76	84
Crustacea	57		13	11
Polychaeta	38	37	51	42
Echinoderma	0	20	13	6

Dabs.

Size of fish in cm.	0-10	10-20	20-30	30+
Coelenterata	0 p.c.	18 p.c.	18 p.c.	20 p.c.
Echinoderma	0	26	25	2
Polychaeta	30	22	20	10
Crustacea	70	30	35	61
Mollusca	2	48	57	65

These tables show what, of course, was more or less known before, that as a rule the young fry live very largely, and in many cases solely, on crustacea. To a great extent the supply of suitable food dominates the movement of the young fry, for nowhere is the truth of the Frenchman's definition of life, 'I eat, thou eatest, he eats,' with its terrible correlative, 'I am eaten, thou art eaten, he is eaten,' more true than in the sea. Later in life the fishes' taste alters, and with increased size they can tackle animals whose calcareous deposits would seem to render them highly indigestible.

Very careful investigations have been made and are being made by Mr. Borley and Mr. Todd as to the distribution of the fauna of the middle and southern parts of the North Sea, and its relation to the depth of water, the varying degree of salinity, and to the texture of the bottom deposits. These results, however, have not been published, but I may go as far as to say that the inquiry shows that within the area investigated the texture of the sea floor has, on the whole, more influence on the distribution of the invertebrates of the bottom fauna than has depth, and that depth in the area in question seems to have more influence than salinity.

With regard to the character of the bottom deposits, it has been found by Mr. Borley that off shore and on the gently shelving continental coast the sea bottom is of a uniform character over wide areas, though on the western side it is more patchy; and it has proved possible to divide the samples taken into some nineteen main types, each characteristic of one or more of the areas into which the region has been split up. Only one or two details of this laborious work can be mentioned. One is that the texture or degree of coarseness of the ground in various parts of the sea is such as to suggest that the distribution of the finer grades of material, the finer sands and silts, is greatly influenced by the joint action of currents and tides. It is, for instance, known that in the southern part of the North Sea the main direction of the bottom current is to the north and then to the east; and examination of the deposits shows a regular diminution in the proportion of the coarser sands, a regular increase in the proportion of finer material, as we proceed from the Straits of Dover in a north-easterly direction. A remarkable fact in this connexion is the complete absence of silt from the sandy bottom west of the mouths of the great rivers Rhine and Maas. There can be no doubt that the presence of broad and shallow stretches of sand on the continental, but not on the English, side of the North Sea is one of the factors which has determined the distribution of the small plaice, which on the continental shores are so extraordinarily abundant, and on the English shores are relatively so scarce.

By means of bottles weighted with shot, so as to have about the same specific gravity as the surrounding sea water, Mr. G. P. Bidder has been able to trace slow currents moving over the bottom of the sea. The bottles are closed, and contain a postcard in many languages, offering a reward to whosoever returns the postcard, recording the latitude and longitude of the place it was trawled at, to our laboratory at Lowestoft. Attached to the neck of the bottle is a copper wire $1\frac{1}{2}$ feet long. This wire trails along the bottom, the bottle itself floating about $1\frac{1}{4}$ feet above the level of the ground.

Slowly as the bottles are swept along, yet the distance they cover is sufficient to sharpen the free end of the wire to a needle point.

By these and by other methods it has been possible to trace the almost imperceptible but steady flow of waters along the bed of the sea. Without doubt these currents influence the distribution of the larval and young forms of all the creatures which live near the bottom, and especially influence the migration of food-fishes in their younger and less active stages, when they are swept helplessly along.

But these bottles have a double lesson to teach us: not only do they enable us to chart the slow streaming of the bottom water, but they give us to some extent a measure of the intensity of trawling in the North Sea. They have been refished in really surprising numbers. Commercial trawlers have retaken them at the rate of 58 per cent. per annum. In one area these bottles cast upon the waters were retaken, not after many days, but after very few. Out of 390, eighty-five were recovered in six weeks, and fifty out of 270 were trawled in five weeks, representing a local intensity of fishing which if continued would give us between 80 per cent. and 90 per cent. of recaptures in a year.

Marked fish which have been liberated and recaptured tell the same story of intensity of fishing.

The intensity of fishing as indicated by the percentage of recaptures within twelve months of liberation is shown by the following table¹:

Off shore	Percentage	
	Fish under 25 cm.	Over 25 cm.
Dutch coast	23.7	20.3
Deep water, Southern Bight	13.0	26.6
Leman Ground (liberated April and May)	18.7	17.4
Leman Ground (liberated December)	—	21.0
Horn Reef outer ground	33.3	23.0

Obviously, since some fish are known to have been captured but not returned to the laboratory, the method gives a minimum estimate.

By applying the same method to the marking experiments of other countries as well as our own, Garstang² gave the percentage recovered within twelve months of liberation of fish over 25 cm. in length as from 4 per cent. on the Fisher Bank to 56 per cent. in the Skager Rak.

When we reflect on the chances of these marked fish dying or being eaten or losing their labels, it is surely a most remarkable fact, full of significance to the practical man, that in the North Sea marked fish of marketable size are recaptured at the rate of between 20 and 30 per cent. each year, and sometimes at a greater rate. It would seem that each square yard of the fishing grounds is swept by the trawl not once, but again and again each year.

Mr. Borley has conducted a large series of experiments to determine the vitality of fish after they have been captured by both the beam and the otter-trawl. It was necessary to determine the degree of injury caused by the actual trawling, the raising of the trawl, and the subsequent exposure on deck. The larger fish of both sexes were capable of resisting the damage to a greater extent than those of smaller size, and the relative resistance of the two sexes varied at different sizes, the male showing a decline in the increase of its vigour as it approaches maturity. One factor which is very deleterious to the fish is the presence of jellyfish in the trawl;

¹ Garstang, *North Sea Fisheries Investigation Committee Southern Area*, Report No. 1.

² *Provisional Report on the Natural History of the Plaice* (Committee B), *Procès verbaux*, vol. iii.

these either smother the fish or possibly sting them to death; at any rate, the mortality of the fish is enormously increased when medusae are present in any numbers. The otter-trawl is also far more harmful than the beam-trawl, and exposure on deck to a hot sun is another constant source of death, one hour's such exposure in one series of experiments killing 99 per cent. of the smaller fish. In the ordinary commercial operation of trawling, whilst the fish are being sorted those that have no market value lie about on the deck of the vessel for at least an average period of one hour; hence it is extremely probable that when shovelled overboard practically all are dead or dying.

The work which has been done by our own special steamer has been supplemented by records carefully kept by certain selected captains of commercial trawlers, which sail from Grimsby or from Lowestoft. In this way the details of some 20,000 hauls have been examined, and their results tabulated by Miss Lee.

I have left myself no time to describe the important hydrographical investigations carried on by Mr. Mathews into salinity, temperature, etc., which show us the conflicting currents at the mouth of the English Channel and how the North Sea in its southern part is supplied with water from the Atlantic through the Channel. The curious ebb and flow of the Gulf Stream, its periodic welling up and subsidence, closely connected as they seem to be with the migrations of the herring, cod, and haddock shoals, is another most important matter of investigation.

Neither can I tell you in detail of the immense amount of work which is being done by the other countries which share in the international scheme, by the Scottish Fishery Board, the pioneer in Great Britain of this sort of research. To the west our Channel work is beginning to get into touch with the more recently established Irish Fishery Board, and with the work carried on under the direction of Professor Herdman in the Irish seas.

The outcome of all this minute and continuous investigation will, in time, tell us whether or no the North Sea fisheries are being exploited in the most profitable way—a very important question for our country, for with a fishing fleet of 27,000 vessels, manned by 90,000 fishermen, who land 900,000 tons of fish a year, valued at 10,000,000*l.*, Great Britain takes 90 per cent. of what is caught in the North Sea. Some statistics indicate that there is a falling-off. The steam trawlers in 1905 landed 25,000 tons of fish less than in 1904, and in 1904 there was a similar shortage on the total of 1903. And yet 1903 was a year in which some crisis took place; the growth of the haddocks and the number of young haddocks were far less than normal, the Norwegian cod fisheries sank to a minimum, the French statistics showed the same feature in their fisheries off Iceland. In 1903, however, there were unusually large numbers of small plaice. The polar ice-field pressed down south, and seals, cetacea, and arctic birds left their usual quarters, and came south in some cases as far as Shetland. The gigantic climatic changes indicated by the above undoubtedly disturbed for a time the rate of increase and the rate of growth of the fish population of the North Sea, but they soon returned to their normal state. Compared with such mighty influences the fishing activity of man seems almost negligible, and Dr. Hjort for one thinks that 'the productiveness of fish' 'may be regarded as independent of the interference or fisheries of man.' I am not sure that this is so. Taking large areas and all fish into consideration, it may be true; especially it would seem to be so of some species, such as the herring, the saithe, and the cod; but in certain areas and with certain fish, such as the sole and the plaice, man's activity has undoubtedly decreased the number.

Although the researches of the last few years have immensely increased

our knowledge of what is going on in the sea, they have, like an ever-widening circle, but increased the number of problems which await solution. It is earnestly to be hoped that the work may go on on at least its present basis. The business man, always on the outlook for a dividend, has sometimes complained that some of our inquiries do not seem to him practical, but he must have patience and faith. A few years ago no knowledge could seem so useless to the practical man, no research more futile than that which sought to distinguish between one species of a gnat or tick and another; yet to-day we know that this knowledge has rendered it possible to open up Africa and to cut the Panama Canal.

And here, if I may quote the words of the author of the Maccabees:

‘And here will I make an end.’

‘And if I have done well, as is fitting the story, it is that which I desired; and if slenderly and meanly, it is that which I could attain unto.

. . . And as wine mingled with water is pleasant and delighteth the taste: even so speech, finely framed, delighteth the ears of them that read the story.’

‘And here shall be an end.’

British Association for the Advancement of Science.

WINNIPEG, 1909.

ADDRESS TO THE GEOGRAPHICAL SECTION

BY

COLONEL SIR DUNCAN JOHNSTON, K.C.M.G., C.B., R.E.,
F.R.G.S., F.G.S.,

PRESIDENT OF THE SECTION.

It has been usual for Presidents of this Section to make some allusion in their addresses to the principal matters of geographical interest which have occurred during the preceding year, and I propose to follow this custom before proceeding with the rest of my address, which would hardly be complete without some allusion to the great geographical achievements of the past year.

I doubt if there has ever been a year in which more important additions to geographical knowledge have been made than those resulting from the journeys of Dr. Sven Hedin, Dr. Stein and Lient. Shackleton.

Dr. Sven Hedin's previous explorations had deservedly gained him such a high reputation as an explorer that it seemed almost impossible for him to increase it, yet his recent expedition in Tibet, extending over two years, has enhanced his already great reputation.

Refused permission to enter Tibet from India, he was not to be deterred. Travelling round to Leh and making that place his starting point, he entered Tibet and traversed in various directions a considerable tract, previously unexplored, of that country, making a good reconnaissance survey of the country he passed through.

A large part of his journey was through a bleak and inhospitable region, where he encountered intense cold and very great privations. At one time he went for eighty-three days without meeting a living soul, and the cold and hardships were such that out of ninety-seven ponies and mules with which he started only six came through. Yet in the following year, in the depth of winter, Dr. Sven Hedin again traversed this terrible country. In doing so he ran imminent risk of starvation, as his last sheep was killed a considerable time before he got through to country where he could obtain fresh supplies.

Dr. Sven Hedin's tact and resource were as great as his fortitude and courage. He made friends wherever he went, and, although the Tibetan Government sent orders over and over again that he should be turned back, he succeeded in spending two years in exploring the country, main-

taining the most friendly relations with the Government officials and others whom he met. Besides exploring and surveying a large tract of previously unexplored country, he investigated the sources of the Brahmaputra, the Indus, and the Sutlej, and in the course of his journeys he accumulated a mass of geographical and other scientific information.

Next comes Dr. Stein's expedition to Chinese Turkestan, by which he has made a most noteworthy contribution to geographical knowledge and antiquarian research.

Dr. Stein, accompanied by that capable surveyor Rai Ram Singh, who was later on relieved by that equally skilful and energetic surveyor Rai Sahib Lal Singh, travelled from India via Chitral and Kashgar. He commenced survey work in the eastern part of the Mustagh-ata range, and carried it along the Kun Lun Mountains, skirting the southern side of the Takla Makan Desert and the Lob Nor Desert to Suchou and Kan-chou. He surveyed a large area of the mountainous region lying westward of Kan-chou, then crossing the desert from Aushi to Hami he returned north of the Tarim River, skirting the southern slopes of the Tian Shan range, to Kashgar. During this very long journey Dr. Stein came across the ancient frontier wall, built about the second century B.C. He traced it west of Suchou, till lost in the desert, for some 250 miles, and he made various incursions into and across the desert, making discoveries of the greatest antiquarian interest.

After his return to Kashgar he surveyed the last unexplored portion of the Kun Lun mountains and the country containing the sources of the Khotan or Yurungkash River, which proved to be flanked on the south by a magnificent range of snowy peaks rising to over 23,000 feet; thence passing the sources of the Keriya River he skirted the southern slopes of this snowy range and finished by connecting this survey with that to the north of this range. The privations and hardships undergone by Dr. Stein and his party were very great, and, just as he completed his last bit of survey, he was unfortunate enough to get his foot badly frost-bitten, and had to hasten to more civilised parts for medical treatment.

Dr. Stein, during his expedition, displayed all the best qualities of an explorer—enthusiasm, determination, skill, and tact. The modest account he has so far given us of his travels, which gives a mere outline of his work, shows that the geographical as well as the archaeological results of his expedition are of the greatest value.

The last completed exploration I propose to mention is Lieut. Shackleton's great journey in the Antarctic Circle, which has raised him to a high position among the gallant explorers of the Polar regions.

Lieut. Shackleton personally arranged and supervised all arrangements for the expedition, his experience in the British Antarctic expedition under Capt. Scott standing him in good stead.

Having landed in McMurdo Sound, a party consisting of Lieut. Adams, Prof. David, and others ascended Mount Erebus, which is over 13,000 feet high, all above snow level.

Later on Lieut. Shackleton and a sledge-party set off southward, and after an arduous journey succeeded in reaching 88° 33' south latitude, over six degrees nearer the Pole than any previous explorer. His party travelled altogether about 1,700 miles, including relays, in 126 days, a splendid performance in a rough and difficult country under very trying climatic conditions. Soon after passing 83° 33' south latitude they lost their last pony, and from this point they had to drag their sledges themselves, although their journey involved the ascent of a plateau 10,000 feet high. They only turned back when their diminishing stock of provisions rendered it imperatively necessary to do so. They were for a considerable time on short rations, and found several times that they had

expended their food supplies before reaching their next dépôt. Had they missed one of these dépôts—no unlikely contingency in such a country—they must have perished by starvation. Altogether the sledge journey was a great feat of pluck and endurance.

Lastly, Lieut. Shackleton's colleague, Prof. David, with others, made a sledge journey to the north-west, reaching the South Magnetic Pole. A good deal of triangulation was carried out, many geological specimens were collected and much scientific information was obtained.

Whether we consider Lieut. Shackleton's skill and energy in organising the expedition, the courage and determination displayed in carrying it out, or the results obtained, his expedition will stand out as one of the greatest of the many great efforts to reach the Poles, and as a British expedition it is one that specially appeals to us.

At first sight it would seem that these great journeys belie the opinion so often expressed of late years that the days of the explorer are numbered, and that in future geographers will have to deal with surveys rather than exploration; but, in fact, these splendid achievements only strengthen this opinion. These explorers have considerably reduced the comparatively small area still unexplored, and other expeditions are helping to diminish the unexplored area.

Among those which are in progress I may mention the following:—Col. Kozloff's expedition to Mongolia, which has already visited Kuku Nor and which is exploring the upper course of the Huang Ho and other parts of Mongolia. Lieut. Boyd Alexander is exploring in West Africa. The Duke of the Abruzzi is investigating part of the mountainous region across our Indian frontier; Dr. Longstaff is exploring another part of that mountain system; Capt. R. E. Peary, U.S.A., and Capt. E. Mikkelsen are leading expeditions in different parts of the Arctic regions, and M. Charcot is exploring in the Antarctic Circle. Lastly, an important British expedition will start before long to explore part of the Island of New Guinea, one of the largest still unexplored land areas. There are other expeditions, either in progress or projected, too numerous to mention.

The best modern explorers are not now content with exploration or even with a rough route traverse and an occasional observation for latitude; they either themselves make careful reconnaissance surveys of the country adjoining their route or they are accompanied by trained surveyors, who make such surveys.

Again, every year the area surveyed on correct scientific principles is extended. The interesting address of my predecessor, Major Hills, will have told you what is being done in this way in the British Crown colonies. In the British self-governing colonies and in the colonies and dependencies of other Powers the area of regular survey is being continually extended, and in more remote regions surveys are being carried out by Boundary Commissions or for railways or other purposes. Along with the increasing appreciation of the value of geography which has taken place of late years, there has been an increasing recognition of the need for regular surveys, and it is probable that the next generation will find that not only is no considerable area of the earth's surface unexplored, but that the area not yet surveyed at least geographically, or for which a regular survey has not been projected, is getting limited.

I propose in the rest of my address to deal with the regular survey and mapping of new areas, and to discuss various questions connected therewith; if I am right in believing that large areas will be regularly surveyed in the near future, such questions merit careful consideration. I shall state on these points the practice of some of the great national

surveys, because their experience seems the best guide for future work; but I recognise that methods suitable for rich and populous countries, such as Germany, France, or Great Britain, may be too costly for many countries and provinces whose survey has still to be made, and mention will be made of less expensive methods which are likely to be much in demand in future.

It would be difficult to say anything new on the subject I propose to deal with, and I lay no claim to do so, still less do I wish to dogmatise as to the best methods. When I express opinions I shall also state the practice of some of the principal surveys of the world, and my hearers having weighed the matter can accept my opinions or not according to their judgment. In either case my object will have been attained if careful consideration is given to the points raised.

Maps may be roughly divided into three classes:—

(1) Geographical maps—*i.e.* those on very small scales.

(2) Topographical maps. The dividing line between these and geographical maps is not very clearly defined. For the purpose of this Address maps between the scales of 4 miles to the inch and $\frac{1}{250000}$ scale will be considered as topographical.

(3) Cadastral maps—*i.e.* maps on large scales mainly for property purposes.

As the time at my disposal will not admit of my discussing all three classes of maps, and as I have on a previous occasion read a paper to this Association on 'Cadastral Surveying,' I propose to limit my remarks to topographical surveys and maps.

In most of the older countries topographical surveys have originally been made to meet military needs, and as a rule they are carried out under military supervision. In order that they may be useful in case of war such surveys must have been made before war breaks out. The use, however, of topographical maps is not limited to military purposes; on the contrary, they have invariably proved of great value for civil requirements. In one respect they are more useful for civil than for military purposes, as a state of war occurs rarely, and hence while the maps are only occasionally used in connection with war, they are constantly used in connection with civil administration and with public and private business of all kinds. The topographical maps of the Ordnance Survey, prepared originally solely for military requirements, have proved extremely useful for civil purposes. Directly or indirectly all the numerous maps prepared by the trade in Great Britain for civil use are based on them. I believe the experience of all other countries is similar to that of the Ordnance Survey. In most countries in which land is of any value, a cadastral survey for land transfer purposes is needed, as well as a topographical survey. In some cases indeed, the need for a property survey has first made itself felt; thus in the Transvaal and in the Cape Colony, neither of which yet has a topographical survey, there has for many years been a Government Survey Department for making property surveys. The question arises whether there should be two separate surveys, one for topographical and one for cadastral maps, or whether there should be only one survey, the topographical maps being prepared by reducing the cadastral survey. Incidentally the further question arises whether, if two separate surveys are made, they should be under one head.

In most countries—the Ordnance Survey of the United Kingdom being an exception—not only are entirely separate surveys made for these two classes of maps, but these surveys are generally under different departments. In some cases the cadastral surveys are isolated farm surveys, showing little detail except property boundaries. Such surveys would, of course, not

answer as a basis for topographical maps. In other cases, however, the cadastral surveys show all necessary detail except ground forms, which can be added by a separate survey. The only cadastral survey, so far as I know, which shows ground forms is the Ordnance Survey, whose 6-inch maps are contoured.

A difficulty in the way of utilising the cadastral survey for the smaller scale maps arises from the fact that a cadastral survey is, from its large size, much slower than a topographical survey. It is often found advisable to take up the survey of the former somewhat irregularly, while it is important for the proper progress of the latter that it should be taken up regularly and methodically. The Ordnance Survey 1-inch map has, since 1824, not had a separate survey of its own, but has been based on the cadastral survey. Ordnance Survey experience has shown that the delays in completing the topographical map, due to this course, have been much greater than one would have expected, and that there are grave disadvantages in having the scale of survey very much larger than that of the finished map. These objections do not apply, or can be overcome, if the cadastral survey of any locality is completed before the topographical map is taken up. This is a condition not likely to be often fulfilled in the case of future topographical surveys. I advocate therefore that, following the general practice, there should be entirely separate topographical and cadastral surveys. I should advocate this even where it is essential to keep the expense as low as possible. More economy would probably result from the adoption of a fairly small scale for the topographical map, from curtailing the small detail to be shown on it, and from showing on the cadastral maps only such detail as is needed for property purposes, than would result from making one survey do for both classes of maps.

On the other hand I consider that, even when separate surveys are made for the two classes of maps, it is advantageous that both should be made under the same head. The more usual course is, however, to have the two surveys independent, and in some cases local circumstances may make the course I advocate inadvisable.

Triangulation.

The first preliminary to any survey should be a triangulation. It is the most satisfactory course, and the best economy in the long run, to carry out with the greatest accuracy possible the primary triangulation on which the survey is to be based. Such a triangulation will remain good for a very long period. For example, the primary triangulation of the Ordnance Survey was commenced in 1791; while some doubts have been expressed whether it is accurate enough to combine with other more recent work for the purpose of investigating the figure of the earth, no one has questioned that even the earliest part of this triangulation is amply accurate enough for map-making purposes.

On the other hand I do not advocate carrying out a primary triangulation until arrangements have been made for basing a survey on it. In South Africa an excellent and very accurate primary triangulation has been carried out. This triangulation was undertaken largely no doubt for scientific purposes. While answering its purpose in that respect it has so far had no surveys of any great extent based on it. An accurate triangulation is now a much quicker and less expensive operation than it used to be. The introduction of Invar tapes and wires has largely expedited and simplified the accurate measurement of base lines, while the improvements effected in theodolites enable equal or greater accuracy to be obtained with the comparatively small and handy instruments now made than could be got for-

merly with large and cumbrous instruments, such as the 36-inch theodolites, with which most of the primary triangulation of Great Britain and Ireland was carried out. Unless observations are rendered difficult by numerous buildings, by trees or by a hazy or smoky atmosphere, a good primary triangulation should not now be very expensive. It is usual to base on the primary triangulation a minor triangulation of several orders, the object being to have an accurate framework of trigonometrical points on which to base the survey. If it is important to keep the expense low, the trigonometrical points may be rather far apart, intermediate points being fixed by plane table; but it should be remembered that it is the truest economy to make the best triangulation which funds admit of. In forests or in wooded and rather flat country, where triangulation would be very expensive, lines of traverse made with every possible accuracy, and starting and closing on trigonometrical points, may be used instead of minor triangulation.

Detail Survey.

Provided the detail survey is based on triangulation, it may be made by any recognised method. Plan tabling is now almost universally resorted to, and is probably as cheap and convenient as any other method. The vertical heights of the trigonometrical points will have been fixed by vertical angles with reference to some datum. The height of intermediate points can be fixed by clinometer lines, especially down spurs and valleys, and even by aneroid, and from these heights the contour lines can be sketched in. Altitudes can be more accurately fixed by spirit-levelling, but this is an expensive method not likely to be much used in the case of topographical surveys. It is possible that in exceptional cases photographic surveying may be resorted to with advantage, and undoubtedly photographic methods sometimes enable work to be done which would not otherwise be feasible. The photographic method suggested by Captain F. V. Thompson, R.E., is an advance on previous methods. In Canada, I understand that a good deal of photographic surveying has been done, and presumably the conditions in Canada have been found suitable for this method. It has been little used elsewhere.

Scale of Map.

The next point for consideration is the scale on which the map is to be published, and it is an important one. Speaking generally, the cost increases with the scale, and cost is therefore one of the main determining considerations. The physical and artificial character of the country, the amount of detail it may be decided to show on the map, the method adopted for representing hills and other detail, and the method of reproduction to be used, all affect the question.

Clearness and legibility are among the first essentials of a good map, and it is desirable that the scale should be such that all detail it may be decided to show on the map can be inserted without overcrowding, or conversely, if the scale is fixed, the amount of detail and method of showing it should be such as to avoid the common fault of overcrowding the map.

In populous countries, such as Belgium, France, and Germany, where buildings, roads, railways, &c., are numerous, a larger scale is, *ceteris paribus*, desirable, than in less populous countries.

All important detail such as roads, railways, canals, forests, woods, &c., should appear on the map, as should the more important names, but it is a matter for consideration how far minor detail such as orchards, marshes, rough pasture, state of cultivation, &c., should be inserted on the map, and to what extent the less important names should be omitted.

PRESIDENTIAL ADDRESS.

In hilly country hachures and contours, especially if in black, tend to obscure the detail and names, and the smaller the scale the greater this tendency.

Methods of reproduction will be dealt with later, but I may here say that more detail and names can be shown clearly on a given scale if the map is engraved on copper than if reproduced in any other way. The scales adopted by different countries vary very much— I give below the scales adopted by some of the principal surveys.

$\frac{1}{250000}$ scale—Switzerland (the more populous parts), Prussia, Baden, Saxony, Bavaria, and Würtemberg (these German maps, although called maps of position, are practically topographical).

$\frac{1}{500000}$ scale—Belgium and Denmark.

$\frac{1}{750000}$ scale—France (the new topographical map), Algeria, Tunis, Holland, Japan, Spain, Switzerland (the less populous parts).

$\frac{1}{1250000}$ scale—the United States (the more populous parts).

$\frac{1}{333333}$ scale (1 inch to a mile)—Great Britain and Ireland, and Canada.

$\frac{1}{750000}$ scale—the Austrian Empire.

$\frac{1}{500000}$ scale—the old staff map of France.

$\frac{1}{1000000}$ scale—the German Empire, Italy, Norway, Portugal, Sweden, and Switzerland (Dufour atlas).

$\frac{1}{1250000}$ scale—the United States (the less populous parts).

$\frac{1}{1250000}$ scale—Russia.

$\frac{1}{2500000}$ scale—the United States (barren districts).

The introduction of cycles, motors, and other rapid means of locomotion has led to a demand for a scale which will show a considerable tract of country on a sheet of moderate size. If the standard map is already on rather a large scale, this demand is best met by publishing a reduction of the standard map. This course is followed by Great Britain and Ireland and by Canada, whose 1-inch map is reduced to and published on the $\frac{1}{2}$ -inch scale; but if only one scale is used a compromise must be arrived at which will meet the reasonable requirements of rapid locomotion, as well as the other essentials of a topographical map.

If I may venture an opinion in a matter in which practice varies so much, it is that for countries using British measures in which, owing to dense population, the detail is close the 1-inch scale ($\frac{1}{333333}$) is a very good one, and that for more open parts the $\frac{1}{2}$ -inch scale may with advantage be adopted. For countries using metrical measures I should advocate $\frac{1}{500000}$ and $\frac{1}{1250000}$ respectively. These scales do not differ largely from those adopted by most of the principal countries, the majority of whom use scales between $\frac{1}{500000}$ and $\frac{1}{1000000}$ for fairly close countries.

Where it is important to keep the cost down I should advocate a half-inch to the mile or a $\frac{1}{1250000}$ scale. All except the most closely populated country can be shown clearly on such scales provided the maps do not show too many names or too much small detail.

The United States have scales of $\frac{1}{250000}$, $\frac{1}{1250000}$, and $\frac{1}{2500000}$, the general closeness of detail in any area determining which of these three scales is adopted. This arrangement is a good one, and would be still better if the areas published on the $\frac{1}{250000}$ scale were also reduced to and published on the $\frac{1}{1250000}$ scale, and if the whole country were published on the $\frac{1}{2500000}$ scale. The principle here advocated of having each scale as far as possible complete for the whole country has been carried out by Great Britain, where the whole country, except some uncultivated areas, is published on the 25-inch ($\frac{1}{250000}$) scale, and the whole country on the 6-inch, the 1-inch, the $\frac{1}{2}$ -inch, the $\frac{1}{4}$ -inch, and other smaller scales.

Scale of Field Survey.

It is usual to make the field survey for small scale maps on a larger scale than that on which the map is to be published with a view to securing greater accuracy of detail, but this should not be overdone. If the field survey is on too large a scale it entails needless expense, also when the surveyor is working on too large a scale he is apt not to realise the effect of reduction on his survey, and is likely to survey so much detail as to overcrowd the map, thus increasing the cost of the work and injuring the map.

When the map is reproduced by photographic methods the fair drawing is usually on a larger scale than the finished map, so as to get finer results on reduction; but in this case also, for somewhat similar reasons to those stated above, there are limits to the amount of reduction which can be made with advantage.

In these respects the practice of different countries varies considerably.

In Austria the field survey is on the $\frac{1}{25000}$ scale; this is reduced to and drawn on the $\frac{1}{100000}$ scale, and this drawing is reproduced by heliogravure on the $\frac{1}{75000}$ scale.

In France the field survey is on the $\frac{1}{10000}$ or $\frac{1}{25000}$ scale. The survey is reduced to and drawn on the $\frac{1}{40000}$ scale. In Algeria and Tunis, both field survey and drawing are on the $\frac{1}{10000}$ scale. In all cases the French maps are now reproduced by heliogravure on the $\frac{1}{50000}$ scale from the $\frac{1}{40000}$ scale drawings.

In Germany the field survey is on the $\frac{1}{25000}$ scale. This is reduced to the $\frac{1}{100000}$, on which scale the maps are engraved on copper.

In Great Britain the 1-inch map is based on the 25-inch and 6-inch survey. These were reduced, and a fair drawing was made on the 2-inch scale in a manner suitable for reduction to the 1-inch scale—i.e., the detail lettering, &c., were drawn so that when reduced to the 1-inch scale they should be in proper proportion. This drawing was reduced and printed by heliogravure on the 1-inch scale, and from these prints was engraved on copper.

In America the field surveys are on the scales of $\frac{1}{48000}$, $\frac{1}{60000}$, and $\frac{1}{100000}$ for the $\frac{1}{25000}$, the $\frac{1}{125000}$, and the $\frac{1}{250000}$ scale maps respectively. The drawings, on the same scale as the field survey, are reduced by photography and engraved on copper.

I consider that the best results are obtained when the field survey is made on double the scale of the finished map; that if reproduction is to be by engraving, the fair drawing should be on the same scale as the finished map; that if, on the other hand, reproduction is to be by photographic methods, the fair drawing should be on the same scale as the survey, i.e. double that of the finished map. The reduction I advocate should conduce to accuracy of detail and, if reproduced photographically, to fineness of detail, while it is not so great that the surveyor and draughtsman should be unable to realise the effect of reduction.

Detail.

The need of considering the amount of detail, &c., to be shown is not always sufficiently realised. The way in which detail is to be represented also needs consideration, as on small scale maps much detail has to be represented conventionally.

Railways have to be shown conventionally, and should be so marked that they catch the eye without being too heavy.

Roads also should be clearly marked. Where different classes of roads

exist they should be distinctively shown, main roads being more prominent than others. It is important to know what roads are fit for fast wheeled traffic in all weathers, and which are fit only for slow traffic. The exact classification of roads must depend on the conditions obtaining in the country. The most elaborate classification is that shown on the French maps, and next that shown on the maps of Great Britain. Provided that important distinctions are represented, the simpler the classification the better.

Forests, woods, marshes, and in some cases pasture, rough pasture, orchards, vineyards, gardens, &c., are shown by conventional signs. While forests, woods, and marshes should certainly be distinguished on the maps, I incline to the opinion that the state of cultivation is better omitted, and that the less small detail shown the better. Such small detail increases the cost and often overcrowds the map. The German 1:100,000 scale shows much small detail, and although the maps are beautifully and delicately engraved on copper, the detail is rather crowded on some sheets. The French Carte Vicinale is, in my opinion, rather crowded with names.

The most difficult question, and that on which opinions differ most, is the method of representing ground forms. Methods which answer well on steep ground are less satisfactory on gentle slopes, and *vice versâ*, and each method is open to some objection.

Ground forms may be indicated by contours, hill shading in stipple, vertical hachures, horizontal hachures, the layer system, or by a combination of some of these.

Ground forms are represented by contours on the 1:100,000-scale maps of the German States, the Swiss Siegfried Atlas, the maps of the United States, the 1-inch map of Canada, the 1:100,000-scale map of Denmark, and the maps of Japan. Where the slopes are steep the contours give almost the effect of hill-shading. Some of these maps give a very good representation of the ground, the best being those in which the contours are in colour.

Hill features are shown by stipple shading on the French Carte Vicinale and the Ordnance Survey four-mile map. In mountainous country stipple shading gives a good pictorial representation of the ground, but it fails in flatter country, and it is often difficult to tell from it which way slopes run.

The Swiss Dufour Atlas (1:100,000 scale) is a good example of vertical hachuring, as are some of the German 1:100,000 scale maps. Vertical hachures are also used on the Austrian and Swedish maps, and in conjunction with contours on the maps of several other countries.

Vertical hachures when well executed give an artistic and graphic representation of the hills. In the Swiss and British maps the pictorial effect is enhanced by assuming a light from the left-hand top corner. In steep ground, especially when the hachures are in black, these are apt to obscure detail and names. I think hachures are better when printed in colour, but many will disagree with me on this point.

Horizontal hachuring, while having some advantages, is less effective and is little used.

The system generally known as the layer system has been used in Great Britain by the well-known Scotch firm of J. Bartholomew & Co., has recently been adopted by the Ordnance Survey for its $\frac{1}{2}$ -inch maps, and is used in the $\frac{1}{2}$ -inch maps of Canada. It consists in indicating by various shades of colour the area lying between certain contours; thus one shade may be given to all ground below the 50-foot contour, another shade to ground between the 50 and 100 foot contour, and so on. This system gives a general indication of ground form and enables the contour lines to be followed more easily. Its shades of colour enable the eye to pick out more

easily all land lying at about the same level. It is most effective in ground with a small range of vertical height, as the vertical depth of layers can then be small and the distinction in colour between successive layers marked. In hilly ground the depth of the layer must be increased, which means that many ground features are ignored on the map, or the number of layers on the map must be large, in which case the distinction in shade between successive layers will be less marked. This method is popular in Great Britain, and enables those who are not versed in reading contours and hachures to realise something of the nature of the ground forms.

A combination of these methods has been used as follows:—

France on her $\frac{1}{250,000}$ -scale maps shows ground forms by contour lines and stipple shading. This gives a very fair representation of the ground, but where the contours are very close together the effect of the coloured contours on the stipple is not pleasant. Nor does the stipple always look well when it falls on colour.

The German coloured $\frac{1}{250,000}$ -scale map, the Italian $\frac{1}{250,000}$, and the British 1-inch show both contours and vertical hachures.

The Norwegian $\frac{1}{250,000}$ -scale map shows the features by contours, vertical hachures and shading.

The new British $\frac{1}{2}$ -inch scale map has both contours, layers and stipple shading.

Opinions differ so much on this subject, and there is so much to be said for and against each method, that I will confine myself to the opinion that contours reasonably close together should form the principal feature of any method of representing ground forms; that contours by themselves give a very fair representation of the ground; that vertical hachures, if printed so as not to obscure the detail and names, or stipple shading when there is not too much colour on the maps, increase the pictorial effect and are useful additions to contours; that ground forms should preferably be in colour, and that where hachures or stipple are used as well as contours both should be in the same colour.

The German coloured $\frac{1}{250,000}$ -scale map (brown hachures and contours), the British 1-inch scale copper-plate printed map (brown hachures and black contours), the British 1-inch coloured map (brown hachures and red contours), and the French $\frac{1}{250,000}$ -scale (grey stipple and brown contours), all give a good representation of the ground, and there are other maps which might be named almost, if not quite, as good.

Vertical Interval of Contours.

The vertical interval between contours should depend partly on the scale, partly on the steepness of the ground. Practice varies considerably in this matter.

The $\frac{1}{250,000}$ -scale maps of Switzerland and of Germany, except Prussia, are contoured at 10-mètre intervals.

The $\frac{1}{250,000}$ -scale maps of France are contoured at 10-mètre intervals.

The $\frac{1}{250,000}$ -scale maps of Japan and Spain are contoured at 20-mètre intervals.

On the Swiss $\frac{1}{250,000}$ -scale contours are 30 mètres apart.

On the United States $\frac{1}{250,000}$ -scale the contour interval varies from 20 to 100 feet.

On the British 1-inch map there are contours at 50 feet, at every 100 feet up to 1000 feet, and thence at 250 feet intervals.

On the Canadian 1-inch and $\frac{1}{2}$ -inch maps the contour interval is only 25 feet, but the sheets published have been in ground with only moderate elevations.

On the German $\frac{1}{250,000}$ -foot the contour interval is 50 mètres.

I consider that if the contours are printed in colour the vertical interval may with advantage be such that on steep ground the contours are reasonably close together, every fourth or fifth contour being printed heavier so as to be more easily followed. If the contours are in black they cannot with advantage be so close.

It is, in my opinion, best if the contour interval is uniform all over a country. Failing this, it seems desirable that it should be uniform over considerable areas and at least throughout a sheet; but this view is not universally held. I do not like the varying interval adopted by the Ordnance Survey. The contours on the Ordnance Survey maps are surveyed with great accuracy and at great expense. For topographical maps much cheaper and more rapid methods will suffice.

Cartography.

I have, with a view to clearness, kept the question of the method of reproduction separate, but it has a bearing on some of the points already considered. Thus the fine engraving of the German 1:100,000-scale map enables an amount of small detail and ornament to be shown on that map which could not have been clearly shown if any other method of reproduction had been used.

The older maps were generally engraved on copper, or sometimes on stone, and printed in black and white. Subsequently photographic methods, such as the photogravure of the Austrian and the more recent 1:100,000-scale French maps, were used, and colour printing is now largely resorted to.

In some cases the colour-plates are prepared by engraving on copper, stone, or zinc. The maps of the United States and Switzerland are engraved on copper. In other cases, for instance, the 1-inch Ordnance Survey, colour-plates are prepared on stone by transfers and offsets from the engraved copper plate. In other cases—*e.g.* the 1:100,000-scale map of France—the colour-plates are prepared by photographic methods.

For clearness, delicacy of outline, and artistic effect nothing equals engraving on copper. It forms also the best basis for colour-printing. Unfortunately it is very slow and costly.

Engraving on stone is quicker and less expensive than copper engraving. It is inferior in delicacy to the latter, but some of the best stone engraving is very good.

Photographic methods are the most rapid and the cheapest, and with care give very fair results. As good examples I may quote the 1:100,000-scale maps of Austria, prepared by heliogravure, and the 6-inch maps of the Ordnance Survey, prepared by heliozincography, both black and white maps.

Of colour-printed maps I may instance the new 1:100,000-scale map of France prepared by heliogravure, and the 1/2-inch Ordnance Survey map hitherto prepared by photo-etching, although I understand that in future the outline will be engraved on copper.

When rapid reproduction and moderate cost are desired I do not hesitate to recommend photographic methods which, although not so good as engraving, give, when carefully executed, reasonably good results.

Opinions differ as to the extent to which colour should be used, the modern tendency being to use it very freely. I can hardly be accused of prejudice against colour, as during my tenure of office at the Ordnance Survey colour-printing was largely developed, but I think it is often overdone. I consider that a moderate amount of colour is a great improvement to a map. Ground forms, however indicated, can, in my opinion, be better shown by colour than in black; it is advantageous also to distinguish water

by colour, to give prominence to main roads by colouring them and to colour woods and forests, but I do not advocate going much beyond this. It is difficult to choose colours which are suitable, distinctive, and harmonious, and the more numerous the colours used the greater the difficulty of doing so.

Colour-printing introduces possible sources of error. Colour maps are based on a drawing on which all detail to appear on the map is shown. A plate is prepared for each colour on which there should be only such detail as shall be printed in its particular colour. In preparing this plate there is a risk that detail which should appear may be omitted, or that detail be inserted which should be on another plate, or that the detail may be slightly out of position. Again, owing to change of temperature and to the varying amount of moisture in the air, paper contracts or expands. Registration can rarely be mathematically correct, and with every care may sometimes be appreciably out. While with care errors such as I have indicated can be minimised so as not appreciably to affect the map, it is difficult to ensure that they should be altogether absent.

To recapitulate my views, I advocate for a topographical map a scale between $\frac{1}{2}$ inch and 1 inch to a mile, according to circumstances. The scale of survey to be double that of the finished map; ground forms to be shown by contours reasonably close together, the exact interval depending on the scale of the map and the nature of the country, also, if funds are available, by vertical hachures; both contours and hachures, if shown, to be in colour, the same colour being used for both. If considerations of time and cost do not admit of reproduction by engraving on copper, the map to be reproduced by some photographic method and printed in not more than five colours. I put forward these opinions rather as a basis for consideration than as having special weight in themselves. With the increasing recognition of the importance of geography an increasing demand for maps is sure to come, and good maps can only be satisfactorily designed after considering the points here discussed.

It is not yet, I think, generally recognised that a really good topographical map, based on triangulation, may be produced on a scale of about $\frac{1}{2}$ inch to the mile at very moderate expense if unimportant detail is left out and survey and reproduction carried out as economically as possible. Such a survey has recently been carried out in the Orange River Colony, a country mainly agricultural with generally poor land. There must be few parts, other than barren and mountainous regions, under settled government where such a survey would not be of value. I believe that in future still further economy in surveying and mapping will be attained, and this will stimulate the undertaking of fresh surveys.

Meeting, as we are privileged to do this year, in Canada, I should like to say a few words on the surveying and mapping of the Dominion. Until recently the only maps published have been on very small scales and have shown no ground forms. During the last few years, however, a regular topographical survey has been undertaken by the Militia Department. I am glad that for this topographical survey the scales of 1 inch and $\frac{1}{2}$ inch to the mile, both standard scales in Great Britain and Ireland, have been adopted. They are, in my opinion, suitable scales for Canada, and it is to be hoped that for any new mapping within the British Empire these or similar scales may be adopted as they have been in many parts. Uniformity in scales is very desirable.

Without committing myself to praise in every respect of the maps prepared by the Militia Department, I may say that they appear to me excellent, well-executed maps. Not many sheets have yet been issued, and they are probably not yet well known even in Canada; but I have little

doubt that when known their value will be appreciated, and that the area mapped will be rapidly extended. There are no doubt large areas in Canada for which a smaller scale than one inch will suffice, but there can be few, except waste and barren regions, for which maps on some scale will not be needed. To a country like Canada, which has made wonderful progress already, and which has a great future before it, adequate mapping must be of importance, specially so in view of the vast area of the country. I have misread the character of the Canadian people if they will be content with any except first-rate maps for the whole settled area of the Dominion.

I should like to have said a few words on the aid which good maps give to geographical education, but my address is already too long. I will only say that while good maps and geographical education are of use to all countries, they are of special value to the British Empire, whose different parts are geographically so scattered, but which are so closely bound together by common ties of kinship, interest, sentiment, and loyalty.

British Association for the Advancement of Science.

WINNIPEG, 1909.

ADDRESS

TO THE

ECONOMIC SCIENCE AND STATISTICS SECTION

BY

PROFESSOR S. J. CHAPMAN, M.A., M.Com.,

PRESIDENT OF THE SECTION.

AFTER searching for some time for a topic for this address suitable to Winnipeg, I finally made a choice which may not commend itself at first as a happy one. It is not a topic of immediate local interest, but at a distance of nearly 4,000 miles I was not in a position to discover the economic problems the treatment of which would immediately arrest the attention of the people of middle Canada at the present time, and had a wizard's wand disclosed to me such problems I should not have been able to solve them on paper from the other side of the Atlantic. And yet my subject has a direct reference to Canadian affairs, though the extent of this reference is not apparent till we look ahead and view things in perspective. It occurred to me after a cursory examination of some recent examples of that remarkable modern crop of Utopias and anticipations which apparently are appealing to an extensive public. If only these 'new worlds' represented what existed somewhere among human beings with passions and infirmities like our own, how much more instructive they would be! one was naturally led to reflect. You will see now the train of suggestion fired in my mind. Clearly, if the gaze of humanity is repeatedly drawn to its future, a visitor from a land of advanced industrialism who had made that industrialism his study, in speaking, in a country as yet thinly populated and young in industrial experience, of some of the most urgent problems which industrialism brings with it, might expect a hearing at least as patient as that which a very minor prophet would win. Now among the most insistent root problems to be found in our great industrial city civilisations are those which group themselves around wages, conditions of work and living, and the hours of labour. From this group I have chosen the problem of the hours of labour, as the one which has not, perhaps, received the same measure of practical consideration as the rest. Expressed in another way, our topic is the value of leisure, the bearing of industrial development upon it, and its effectiveness in shaping economic arrangements. The demands continually made for shorter hours and a normal day, the claim, now extensively supported among Western peoples, that the State should

intervene, and the fact that some Governments have intervened, even to the length of regulating the hours of adult male labour, are additional grounds for trusting that this topic will be at present of more than academic interest.

We naturally inquire at the outset why the question of leisure does not assume prominence until modern industrialism has supplanted a simpler economy, and why much less is heard of it among agricultural than among industrial communities? In the hand industries of the past the hours of labour were excessively long in comparison with modern industrial standards, and among the peasantry and pioneering farmers work never wholly ceases in waking hours throughout much of the year except for short breaks for meals; and yet little complaint would seem to have reached us from either source. The explanation may lie partially in the fact that new grievances emerge with the spread of the wages system—the problem of the working day does not present itself in quite the same light to wage-earners and to the self-employed—that these grievances are rendered more articulate by group production; and that the aggregation of people of one economic class in dense packs gives unanimity and volume to the demand for reform. The hardships suffered by a scattered population, occasioning discontents, which, however, stop short of provoking outbreak, seldom succeed in attracting public notice; and people acting in isolation are naturally timid. But this, I think, is not the sole explanation. The character of much of the world's work has changed and so have the demands made upon leisure.

Industrial work on the whole has certainly become more regular and continuous throughout the year, and analysis would seem to show that work per unit of time gets more severe, in a sense, as communities advance, though no doubt a strong case could be made out for the view that the trend of economic progress is towards an end in which the character of labour generally will be far more conducive both to satisfaction and to human development. I am not so optimistic as to suppose that mechanical improvements do not frequently bring with them a new monotony of work, though higher wages may prevent them from forcing greater monotony of life upon those who suffer from the new monotony of work. Mechanical improvement proceeds by 'specialising out' mechanical tasks, the performance of which by hand must be a dreary occupation, but each step in the march of invention seems to create, as a rule, by its incompleteness, tasks meaning a new and more concentrated monotony, though no doubt it must generally result in an appreciable reduction of the amount of dull employment involved in the attainment of a given output. Any work must be wearisome the pace of which is set by a machine and kept absolutely steady. We may usefully compare mechanical improvements with discoveries relating to the utilisation of by-products. The latter always recover from refuse something of value to the community, but they generally leave a refuse more concentrated than that with which they began.

The road of economic advance is by way of specialism, and, just as there has been specialism in tools and in division of labour, so there has been a specialism of labour in working hours and of leisure and social intercourse in non-working hours. Specialism on the one side implies the elimination of waste, whether of means or of time, and it has therefore meant to the labourer the partial or occasionally complete elimination of the leisure with which his working hours used to be plentifully interspersed. In a modern workshop, noise, the necessity of discipline, or a continuously absorbed state of the attention, have frequently reduced the possibilities of conversation to the barest limits. Humanity has no doubt been relieved of the heaviest burden of toil by inventions relating to the mechanism of production, but their application has been accompanied on the whole by the

closer concentration of some kind of effort in time. The intensification of labour in a more confined sphere of activity may, as Professor Münsterberg argues, exercise more fully the higher human faculties and thereby bring with it a deeper interest, but it will almost certainly prove more exhausting, even apart from the elimination of change, leisure, and social intercourse. And decade by decade, with the 'speeding up' of machinery, we should expect to find more nervous strain accompanying the process of production. That industrial functioning has become a severer tax on the energy of the workman is fully borne out by the evidence of numerous reports upon industrial conditions.

The increasing nervous strain of industrial work, whether it results from the progressive specialisation of labour or not, would account sufficiently for the curious circumstance that there is apparently no finality about any solution of the ever-recurring problem of the normal working day, though it is not the sole explanation. The workman whose day has been reduced is soon repeating again his demand for shorter hours, and there are pessimists who infer from this that the shorter hours attained hitherto have shifted the community on to a slippery inclined plane which leads from the economic 'struggle for existence'—by which is meant the competitive striving for place, reputation, and achievement, whereby progress is naturally stimulated—to economic stagnation. They think they discern in the present generation a growing disinclination to make an effort and a growing disposition to take the easy path; but that the truth cannot be mainly with the pessimists an examination of the effects of curtailments of the daily hours of labour upon output would at least suggest. A mass of material exists in official and other reports in more than one advanced industrial country for a study of this question. Beginning with the writings of Robert Owen and Daniel le Grand, both of whom laid especial stress on moral and social elements, an investigator would find an almost unbroken sequence of evidence. Mr. John Rae collected a volume of facts in 1894, and these may now be supplemented by the experiences of yet another half generation.¹ Limitations of space forbid that I should quote examples, but I may at least roughly generalise from the recorded facts. I have found no instance in which an abbreviation of hours has resulted in a proportionate curtailment of output. There is every reason to suppose that the production in the shorter hours has seldom fallen short by any very appreciable amount of the production in the longer hours. In some cases the product, or the value of the product, has actually been augmented after a short interval. In a few cases the reaction of the shorter hours on the output per week has been instantaneously noticeable, and the new product has surpassed the old product before mechanical methods could be improved. Further, for some industries—for instance, for the Lancashire cotton industry—we have preserved for us the results of a string of observations reaching back about three-quarters of a century, and it would appear from them that the beneficial effects wrought upon output by the shortening of hours were substantially repeated, though, of course, in different degrees, at each successive reduction of the working day.

So far I have directed your attention mainly to two incidents bearing upon the hours of labour: the one, the effect of industrial development in curtailing the hours which result in the largest daily output; the other, the subjective effect of the increasing strain associated with such advance. I have now to add another influence, which is the enhancement of the value of leisure which must accompany a rise in wages, improved education, and social progress generally. It

¹ Note in particular the report of the Industrial Commission of the United States.

must be insisted that the amount of the real wage yielded by a given money wage varies as the time left to spend it; and, further, that the value of leisure is a function of the goods which can be enjoyed in the period of leisure. The acute operative would aim at so distributing his time between work and recreation that the gain resulting from a little more leisure would equal the loss consequent upon the implied diminution of wages. Hence, when the volume of goods per head annually supplied to labour was augmented, an attempt would almost certainly be made by the operatives to buy more leisure, even if the satisfaction derived from leisure were unaffected, which it would not be, because the satisfaction derived from leisure must rise when each hour of leisure is enriched by greater possessions. As regards the effect of education, it is sufficient to point out that the value of leisure is a function of appreciative power and that this is developed by education, but it must be observed that the higher appreciative power might enhance the satisfaction got out of the work itself, and that this effect might conceivably counteract the effect on the value of leisure, or even more than counteract it. Ambitions would be further awakened; but the ambitious operative would probably demand, as a rule, more time for study. I think it unquestionable that, on the whole, educational advance causes a curtailment of hours. 'But unfortunately human nature improves slowly, and in nothing more slowly than in the hard task of learning to use leisure well. In every age, in every nation, and in every rank of society, those who have known how to work well have been far more numerous than those who have known how to use leisure well. But on the other hand it is only through freedom to use leisure as they will that people can learn to use leisure well: and no class of manual workers who are devoid of leisure can have much self-respect and become full citizens. Some time free from the fatigue of work that tires without educating is a necessary condition of a high standard of life.' Social progress, broadly regarded, by complicating life and rendering vague feelings of social obligation definite and more insistent, creates new claims on leisure. 'Generally it can be said that the more complex the social organism becomes, the more its constituent individuals must devote time, apart from work and business, to the family and recreation, to education and general affairs, the more necessary is a general social arrangement concerning the distribution of time between the several purposes which it has to serve.'²

The eight hours day has come to be regarded by some social reformers as the ideal of the future. The doctrine that the workman should normally work eight hours a day has been put forward as holding at least as generally and with as high a degree of certainty as, say, the doctrine that the workman should normally sleep some definite number of hours a day. But I should argue that the problem of the length of the working day is of an order different from that of the problem of the time which should be devoted to sleep, for whereas the hours which should be given to sleep depend mainly upon physiological conditions, though these physiological conditions are affected by economic and psychological conditions, the hours which it is wise to assign to labour depend upon the attitude of the workman to leisure and work, which results as much from non-physiological as from physiological influences. It is my purpose to demonstrate that the non-physiological value of leisure, as well as its physiological value, must rise with progress, and, therefore, that in all probability the hours which should normally be worked per day will become steadily less. The ideal working

¹ Marshall, *Principles of Economics*, 5th ed., pp. 719-20.

² Schmoller, *Grundriss der allgemeinen Volkswirtschaftslehre*, p. 741.

day of the future cannot be eight hours, for it must be essentially a progressive ideal. As a community advances agitation for shorter hours will be constantly breaking out anew. If this be a correct reading of progress, it is important that we should understand fully the forces at work at each re-settlement of the length of the working day, those on the employing side as well as those expressed in the claims of the operatives. I propose now, in consequence, to disentangle the impulses and their relations, into which the question of the determination of the working day at any one time may be resolved.

The problem being elaborate, it is essential that we should proceed by successive steps of abstraction. We need not be afraid in this age of understanding of having recourse to abstraction; it is a method without which every scientific study, whether philosophy, biology, physics, or what not, even history, would be impossible. In the first instance, therefore, I intend to indicate the length of working day which operatives and employers would respectively seek if they recognised their own interests and were endowed with complete foreknowledge of the effects of different hours of labour upon their interests. I shall assume—as I may legitimately for ordinary factory employment—that the workman tends to get as his wage his marginal worth, that is to say, the value which would be lost by his dismissal. We may assume, further, that the marginal worth of the workman for any given working day becomes in the long run a stationary amount. If the efficiency of labour rose continuously in consequence of a reduction of hours it would obviously approximate to some limit, and if it fell continuously in consequence of an extension of the hours of labour it would equally approximate to a limit. After some time the differences between these limits and the actual efficiency of labour could be taken as negligible. Merely for the sake of simplicity, I shall now suppose that one kind of labour only is employed. It is clear, then, that it is possible on these assumptions to indicate what in the long run (*i.e.*, when all the reactions, as regards, for instance, the efficiency of labour and provision and arrangement of other agents, have taken place) the marginal daily worth of labour will be for different lengths of working day, it being understood that the number of shifts worked remains the same. If the number of shifts were increased the value of the labour would rise, as will be fully explained later. Let us suppose that the following table represents, at a given time, the value of labour of a given kind per week in relation to the length of the working day:—

Hours per Day.	Value of Labour per week in Shillings.
6	34
	38
	40
9	41
10	40
11	39
12	37

The fall in the value of labour after the working day exceeds nine hours is due to the fact that diminished weekly productivity more than counteracts the direct effect of the extension of the daily time for work. The diminished weekly productivity may be due to impaired vitality—physical, mental, or moral— or to some extent to irregularity, where that is possible, as in the case of colliers. The damage to productivity may be inflicted directly by excessive work, or it may be indirectly consequent upon it, the prime cause being found in the use of stimulants or recourse to unhealthy excitement in periods of leisure, reactions which are only to be expected when the day's

work is very exhausting or very dull. The use of leisure affects, of course, mental vitality, culture, and character, and it will therefore be generally observable that labour which has had its hours reduced will be capable after a time—when the use of leisure has been improved and the improvement has produced its effects—of managing satisfactorily more complicated machinery, and will be generally more responsible and trustworthy, and therefore less in need of continuous watching and directing. Now, clearly, if employers are endowed with the foresight presupposed, and if their hours of work need not increase concurrently with a lengthening of the working day, it is in the case supposed to their interest collectively to come to an agreement not to employ labour more than nine hours a day, and to their interest individually not to employ labour for shorter hours than nine a day. The second conclusion follows from the fact that the weekly product would be augmented by a greater amount than 1s. multiplied by the number of operatives were the hours of labour increased, say from eight to nine, because labour, as every other agent employed in production, is paid not by its aggregate but its marginal worth to the business in which it is employed. This proposition may be made more self-evident by the following example. Were labour rendered 25 per cent. more productive all round, the product and real wages would each be raised approximately 25 per cent., other things being equal; but as the product must be greater than aggregate wages the addition made to the former by the longer hours must be greater than the addition made to aggregate wages.

Next, suppose that an agreement between employers, tacit or overt, is impossible, and that each employer will make what he can when he can. What hours, then, will competition among employers tend to bring about, when humanitarian considerations and any resistance from the operatives are ruled out? Suppose the efficiency of labour at the time is that associated with a customary working day of ten hours. The product of the last fraction of the tenth hour could not be zero, for, if it were, ten hours would not be worked. The ultimate effect of extending the working day beyond nine hours is loss, not because the product of the last fraction of the ninth hour is zero, but because the product of the last fraction of the ninth hour just equals the ultimate reduction of the product of the other hours occasioned by the lengthening of the working day. Hence, on the assumption that employers are perfectly far-sighted but that agreement between them as to working hours is lacking, the disposition on the part of each employer to reduce hours to nine would be weakened if each employer could not depend upon keeping operatives after he had brought them to the level of efficiency associated with the nine hours day. The reforming employer would run the risk of paying the whole cost of the labour value created by shorter hours and getting little in return; other employers might secure and exhaust the new labour value, and no permanent good would be effected. Nor would there be any more guarantee in the conditions supposed that the nine hours day would be retained, if instituted, for an employer could always snatch a temporary advantage by extending hours and paying slightly higher weekly wages. This is a general proof that, on the assumption made as regards the intelligence and foresight of employers and in the absence of agreement between them, the hours resulting in the maximum product would not necessarily establish themselves, no force on the side of the workpeople being supposed operative.

I now pass on to analyse the determinants of the operative's choice in the matter of the hours of labour, assuming that his wage equals his marginal worth and that he knows it, and supposing in the first place that he is endowed with perfect prevision. Two things affect him which do not appeal to the self-interest of the employer, namely, the direct value of his

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(the operative's) leisure and the balance of satisfaction or dissatisfaction which his work yields of itself. Here I must interpolate the remark that by 'satisfaction' or 'utility' in this address I merely intend a conventional objective representation of the subjective fact of preference, behind which the economist *quâ* economist cannot penetrate. I say this in order to evade the charge so frequently made against economics that it implies the acceptance of Utilitarianism, psychological or ethical. Picking up again the main thread of our discourse, we observe that, apart from the two considerations mentioned above, namely, the value of leisure and the satisfaction got directly from the activity of labour, the operative's real income is maximised when his money income is maximised. Hence apart from these two considerations the choice, as regards the length of the working day, of perfectly far-seeing operatives would be the choice of far-seeing employers were the latter combined. Now take the value of leisure into account. Any daily duration of production being premissed, if the utility derived from an incremental addition to leisure is greater than the utility of the increment of wage sacrificed by transferring an increment of time from production to consumption, the operative would gain from a contraction of the working day, other things being equal. Recurring to our earlier numerical example, we see that from the long-sighted point of view the productivity of the last fraction of the nine hours day is zero while its value as leisure must be greater than zero. Hence the operative would choose to work less than nine hours a day, it being understood, remember, that he is paid his marginal worth and knows what that will be for different daily periods of work. Leisure consists in rival satisfaction-yielding occupations, active or passive, which are rendered possible by wages. There is consequently a close connection between this and that other determinant of the operative's choice, namely, the positive or negative utility associated with labour itself. It may be granted that in the long run, after the working day has exceeded a certain length, any further addition to it diminishes the satisfaction directly derived from working or adds to the balance of dissatisfaction. If a balance of dissatisfaction were associated in the long run with the efforts of the last minute in the working day which the operative would otherwise choose, as would ordinarily be the case, he would elect, other things being equal, to work an even shorter day, the duration of which would be determined at the point at which the gains and losses came to equivalence when everything was taken into account, that is to say at the point at which his satisfaction was maximised. Did the last minute of working still yield satisfaction in the long run when the hours were nine (referring to the case supposed), which is so highly improbable as to be a negligible case, the operative would prefer to devote more than nine hours of his day to production were this satisfaction of working greater than the value associated in the long run with the last minute of leisure left when nine hours a day were given to business.

So far in considering the operatives' interests we have fixed our eyes on a remote perspective. We next focus our attention upon immediate tendencies and suppose them not to be counteracted by forces arising out of a regard for ultimate results. In these circumstances the operative would be inclined to select a longer working day than that which would be continuously the most advantageous to him, because he would be blind to the reaction of the longer hours on efficiency and so on earnings and the capacity to take pleasure in work. Many people lower the general level of their earnings in the future, and spoil their enjoyment of work and leisure in the future, by making as much as they can in the present. However, even in these circumstances operatives would not approve such long hours as employers who were short-sighted, because the latter would make no allow-

ance for the disutility of labour to the operative or the utility to him of leisure.

We are assuming throughout, it must be remembered, that the wage will always be the operative's marginal worth—that is, what would be lost if he were dismissed—and that he knows it. Actually, of course, there is frequently an appreciable discrepancy between the marginal worth of labour and its wage, and the usual connection between them has not been commonly understood by the wage-earning classes. It would seem from the records of labour movements as if the operative's fear—based as much on ignorance as on distrust—lest the longer day should mean no more pay, though the weekly product would be greater, has protected him against the injurious consequences of short-sightedness; but I am inclined to think that the dominant force in these labour movements has consisted in ideals of life, formed half instinctively, which are unconnected with views, fallacious or otherwise, concerning the mechanics of distribution. Bad arguments have been used to justify good ends. To these ideals of life I shall refer again.

In reality the actions of both employers and employed, in so far as they are governed by self-regarding impulses, will be compromise resultants of immediate impulses and long-sighted calculations. Long-period results which are not very remote will usually be appreciated, and employers as well as operatives may aim at them, because the former may think the length of time an operative usually stays with one firm sufficient to justify a slight present sacrifice made with the object of securing improvement in the operative's efficiency.

The above analysis explains not only disagreements between employers and operatives as regards the normal working day, but also the friction which is constantly generated in the matter of 'overtime.' Without the admission of overtime heavy losses might be experienced by an industry in view of the inelasticity of its production and fluctuations in the market in which it sold; but, on the other hand, overtime once admitted sometimes tends to be worked out of proportion to the special need for it, and operatives are apt to suspect that it is being used unfairly to extend the normal day.

I now desire to compare specifically the effect on wages with the effect on the working day of the mechanical action of pure competition. In the matter of wages, if operatives were too weak to have much influence in settling their pay, competition between employers, were it keen and unchecked by combination, would at least secure to the operatives as a wage, for a given working day, their marginal worth (within limits set by social friction) in view of their then state of efficiency. Thus in the circumstances supposed the operative would tend to get approximately the utmost possible—apart from the question of the reaction of wages on efficiency—in an active society reposing economically on a basis of freedom of enterprise, for we may take it that in such a society the bidding of individuals against one another for labour would continue at least up to the known marginal worth of labour. Observe, however, that the existence of such bidding may imply that new businesses are being established, or that old established employers are anxious to make considerable extensions, for old established employers, knowing that similar workmen must be paid the same, might avoid courses of action which resulted in a gain less than the loss involved in the elevation of wages. It is doubtful whether employers would as a rule assume that if they did not take steps leading to an advance in wages others would do so, for, not unnaturally, employers are commonly indisposed to disturb rates of wages except for strong reasons. And in the cases in which competition is effective in raising wages to the marginal worth of labour, it must

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be remembered that employers, even if endowed with a powerful telescopic faculty, would not necessarily be induced by self-interest to offer the wage in excess of the operative's worth at the time which would ultimately produce (by augmenting the bodily and mental vigour of the operative) efficiency value equal to it, for their precautionary instinct would attach weight to the apprehension lest some of their operatives should leave them and carry to rival employers the proceeds of the long-sighted investments thus made in them. Other things being equal, of course, the higher the efficiency of labour the greater is the gain not only of the workmen but also of the employer. Now, as regards the working day, we have already seen that uncombined employers might keep it longer than would be desirable from their point of view, for the same reasons for which they might keep wages lower than would be desirable from their point of view. These reasons are, I repeat again, short-sightedness, or fear of incurring an expense the fruits of which other employers might reap. In this respect competition between employers is equally defective in its bearing on wages and in its bearing on the length of the working day. But it has an additional defect, as regards the amenities of working class life, in its bearings on the length of the working day; for though competition between employers in an enterprising society would bring about the degree of devotion of time to production which the operatives would choose at the wages rendering it possible, the choice of the operatives is apt to be governed by a circumscribed vision which is partially blind to the responses of efficiency to abbreviated hours.

It would seem, therefore, that two reasons at least can be derived from economic theory for State intervention in the matter of the hours of labour, if it be assumed that the State can discover what is best for the country. The one is to correct the tendency of people engaged in industry to agree upon an amount of sacrifice to money-making, which means a large future loss, involving the next generation, for a small present gain; the other is to fortify, if needful, the resistance of operatives to the disposition of some employers to secure a greater product at the expense of the operatives' convenience. This conclusion would, however, be too hasty a deduction. Economic matters are settled, not merely by the self-regarding forces which we have hitherto emphasised, but also by social conceptions, embodied in public opinion and class notions of what is right and proper, which defy expert analysis and any accurate evaluation as influences. These social conceptions, which are not deliberately framed on a rationalistic basis, but proceed insensibly as it were from the needs of human life, are less intermixed with religious elements now than they used to be, but are none the less powerful. Resting on the seventh day is not at present a religious observance to the extent to which it has been in certain periods of past history, but it has not universally been found necessary to supplement the declining religious sanction with the legal sanction. How far progress which runs counter to tendencies determined solely by self-regarding forces may be left with confidence to the operation of these incalculable motives which sway every community, can be settled only by careful observation. It is sufficient now to recognise their existence, and to point to the reductions of the hours of labour in recent years. I do not propose to consider here, in the light of the existence of these incalculable motives, the merits and demerits of the method of legal enactment for attaining the ideal in the matter of the daily duration of toil, except to observe, first, that Government interference which aimed at securing reasonable hours for adult males in all the diversified industries of a country would entail elaborate, elastic, and frequent legislation, and would no doubt be accompanied by many grave errors; and

secondly, that a *prima facie* case can be made out for the regulation of the hours even of adult males by authoritative boards, Order of the Home Office, or by statute, when labour is weakly combined and hours are evidently sweated hours, and evidence is forthcoming that they are detrimental to health or vigour. Nor do I propose to consider whether it might not be better to suffer for a time present ills in the hopes that there would grow up in the community an adequate power of self-regulation, which would incidentally be accompanied by highly valuable social consequences, outside the sphere of our present inquiry, that otherwise might never have been elicited. I am hopeful that the intangible force of public opinion, directed by economic and ethical enlightenment over a field rendered yearly more co-extensive with contemporary facts in consequence of the growing demand for publicity and the response made to that demand by governmental authorities and the press, will become in the future an increasingly efficacious factor in progress, apart from its expression in law. Even to-day, in view of the dependence of producers on demand, neither employers nor trade unions can afford to brave for long public sentiment, though unmorganised, when it is deeply stirred; and public sentiment in the years before us may be expected to respond more sensitively to incidents in its surroundings which offend against social conceptions of what is right and proper. The cases of children, young persons, and women, which bring in special considerations, must be ruled off from the subject matter of this address.

There is no doubt but that all advanced industrialism to-day is feeling the strain of an accumulation of forces tending to bring about an abridgement of the working day, and that it will be subjected to the same strain in the future. Now, in relation to this experience, it is disturbing to notice that a close-set limit is imposed upon reduction of hours by the heavy interest and depreciation charges with which the product of a machine is burdened when it works only a fraction of the time for which interest must be paid. As regards depreciation it must be observed that buildings deteriorate in value at least as much when shut up as when they are occupied; that machinery continues to wear out, and sometimes rapidly, when it is idle; and that the reserve fund necessary because the market may contract at any time, and because machinery may at any time be rendered obsolete, is independent of the length of the working day. Many inventions involve an extended use of capital per head, though all do not, and interest and depreciation charges are on the one hand interdicting the application of some of those new ideas to industry which do necessitate heavier capital investment, and on the other hand preventing those applied from reducing hours so much as they otherwise would.

The weight of the discouragement indicated above to the shortening of the hours of labour depends, of course, upon the relation between wages and payments for capital in the expenses of a business, and this relation varies with the industry. A rough calculation, nevertheless, for a particular industry of the saving in hours which might be effected by the continuous running of plant will not be altogether irrelevant. In the industry for which I have obtained figures, interest and depreciation would be reckoned ordinarily at 10 per cent. on the capital, about half for each, while wages would be in the neighbourhood of 12½ per cent. Now, it is being assumed provisionally that the depreciation charge varies as the hours worked, that the rate of interest is a constant, that the equipment of the industry remains as before and labour tends neither to leave the industry nor to flood into it, and that other costs of production are not affected, we find that hours could be reduced from ten to eight

without any loss of wages, were the continuous running of plant substituted for the ten hours day.¹

Actually, of course, some of the gain would be taken in the form of higher wages. Further, it must be noticed that the assumptions made do not accurately correspond with fact, though they are satisfactory for the purposes of a first approximation. On the one hand they lead to an over-estimate of the advantages of continuous running, because twenty-four hours of work could not possibly be squeezed into a twenty-four hours day, and because the cost of artificial light during night work is disregarded, as are also the costs connected with awkward points in organisation, with the sharing of responsibility for the proper treatment of machinery, and with the fact, universally experienced, that night-shifts are not so productive as day-shifts. On the other hand, they lead to an under-estimate of the advantages of continuous running, because the cost of depreciation, as we have seen, is not proportional to the daily hours of work,² because the shorter hours would raise the efficiency of labour, and because the demand for capital would be reduced, as would also the demand for land for manufacturing purposes. The inevitable contraction of the demand for capital is a point to be emphasised. If working hours per day were raised from ten to twenty-four, then, the reaction on the efficiency of labour still being disregarded, the old output could be obtained with five-twelfths of the old capital: the consequence would be a fall in interest, an augmentation of the amount of the plant per head of the people working with it at one time, and therefore an increased output per head.

In view of its great economies, the shift system calls for very careful consideration. The magnitude of the advantages which the wage-earners might hope to derive from its more extensive application has been denied, on the ground both of theory and of experience of those businesses in which it has been tried. But theoretic objections of a fundamental nature will be found to reduce to false doctrine concerning the determination of wages; and it must be remembered that as the benefits accruing from the comparatively few cases in which

¹ The calculation is as follows:—

Interest	= 5 per cent. of capital.
Depreciation	= 5 " "
Wages	= $12\frac{1}{2}$ " "
∴ Wages + Interest	= $17\frac{1}{2}$ " "

Continuous running would mean increasing the annual duration of production in the ratio of $\frac{24}{10}$. Hence, with continuous running,

$$\text{Wages + Interest} = 17\frac{1}{2} \times \frac{24}{10} = 42 \text{ per cent. of capital.}$$

And, as the capital remains as before—

Interest	= 5 per cent. of capital.
Wages	= 37 " "

Writing x for the daily hours worked per head which would yield the same weekly wages as before, we have

$$\frac{37}{24} \times x = \frac{12\frac{1}{2}}{10} \times 10.$$

$$\therefore x = \frac{300}{37} = 8 \text{ (approximately).}$$

² Had the depreciation been taken as independent of the hours of work the calculation in the previous note would have pointed to a seven hours day instead of an eight hours day.

the shift system is practised are by competition spread over the whole community, the gain of any individual is cut down to a very small figure. It must not be supposed that the effect of its universal adoption would be equally inappreciable. Without general recourse to shift systems I cannot see any immediate prospect of much additional leisure for the mass of the population. Shifts could be designed so that no one shift would be particularly disagreeable to work in, and, if all shifts did not offer equal advantages, the operatives could be moved round, being assigned for so many weeks to each shift. The shifts for foremen, and the management generally, which would have to be strengthened, might be arranged to run over a portion of two operatives' shifts, so as to cement the new work on to the old; and the connecting of the work of each shift with that of the shift which it followed could also be secured by arranging that the unit of labour should be a group of partners, consisting of one man from each shift, it being the duty of each man before commencing work to see his partner in the displaced shift and receive instructions from him. Naturally, a shift arrangement could only be introduced gradually. Are the objections to shifts of such gravity as to counteract their immense economies? The fact that an affirmative answer was generally given to this question in the past is no proof that the affirmative is the right answer to-day in England, or even in industrial Canada. Conditions have been revolutionised in the last fifty years. Improvements in artificial lighting and in intra-urban transportation have alone swept away a mass of the conditions underlying the evils which used to be associated with night work. And two or three shifts of approximately seven hours each, or three or four shifts of approximately six hours each— I state a not immediately attainable ideal—are very different in their effects upon social life, exclusive of those associated with the shorter period of toil for each workman, from two shifts of some ten or eleven hours each. With the shorter shift in use, arrangements could be made without much difficulty for all operatives to get most of their sleep in the night, if they so wished, and to enjoy most of their leisure in daylight. But it is not my intention in this address to make a practical proposal, or argue points of detail. I merely present certain theoretic corollaries which have incidentally been derived from our analysis of conditions determining the length of the working day. In conclusion, I may quote Dr. Marshall's final judgment that were shift systems more extensively adopted 'the arts of production would progress more rapidly; the national dividend would increase; working men would be able to earn higher wages without checking the growth of capital, or tempting it to migrate to countries where wages are lower: and all classes of society would reap benefit from the change.'¹

Let me now summarise my main conclusions, and humanise them by restoring the moral and social elements from which our premisses were to some extent abstracted. I have hitherto spoken of progress in such terms that the critic would have some excuse for charging me with narrowness of vision. Progress is not summed up in improvements in productive methods which reduce the cost of things, nor in these improvements combined with the application to production of ideas which render work pleasanter and more educative. Nor is it wholly, or in bulk, summed up even if we add improvements in distribution (resulting in a more satisfying sharing of wealth) and a greater responsiveness of production to the needs of the community. The essentials of what most of us really understand by progress are to be found only in the world of consciousness in the spiritual constituents of the universe. I mean what we cannot exactly define if we are not philosophers—and hardly then—but something implying a full living,

¹ Marshall, *Principles of Economics*, 5th ed., p. 695

with understanding of life and its surroundings, including its ethics, and a living with volitional powers strong enough to enable us to follow our lights. As all this is actually, though vaguely, desired in some degree by humanity generally, it is no doubt covered by the satisfactions measured in demand, but the admission of its reflection on one plane cannot be regarded as its adequate inclusion in our social philosophy. The most important aspect of the question of the length of the working day consists in its relation to the most intimate constituents of progress. Let us call progress in this sense 'culture'—a term perhaps the best of the single terms available to convey my meaning. Now the world appears to be so designed that culture has on the whole a proportionately important place in the most primitive economic conditions. The hours of labour in such conditions may be long, but work is not so continuously absorbing that social intercourse during work is impossible, while variety of experience, contact with nature, and the calls made on initiative, afford that intimacy with life as a whole, and that evocation of moral forces, which must be obtained in later stages of civilisation largely through systematic education and books. I have argued above that each step in civilisation brings intensified specialism. Work is by no means rendered non-cultural ultimately, but its cultural aspects are specialised, as are its objective aspects. Interest may be deepened on the whole, but it is no longer diffused; the need for thought and purpose may be no less than before, but the thought and purpose are of a confined character. The intensification of economic life which is implied is in itself all to the good, but the community must lose something of culture unless corresponding with this intensification there is an expansion of leisure and a specialised use of leisure for the purposes of culture. Certain expressions which have come into common use would seem to be significant of the needs and dangers of an industrial society highly advanced on the technical side. Thus we speak of the 'cultured' classes and the 'leisured' classes. For the attainment of culture, leisure is essential to-day as it was not in the past in quite the same sense, 'culture' being broadly defined. I need not say that a 'progress' which meant the 'specialising out' of leisure for the sole enjoyment of one class would not commend itself to any reasonable person; and I do not discern any danger of 'progress' of this sort; but there is some danger lest the growing importance of leisure generally, and of a proper use of leisure, should not be fully realised. Tangible things force themselves upon our attention as the more intangible do not, and some of us who have an economic bent of mind get into the way, in consequence, of thinking too much of the quantity of external wealth produced and too little of the balance between internal and external wealth. In ultimate terms, to those who care to put it that way, all wealth is life, as Ruskin insisted. There hardly appears to be any risk of a general underrating of external goods, but there is some risk of an underrating of the new needs of the life lived outside the hours devoted to production—which should themselves be, not a sacrifice to real living, but a part of it—and of an underrating of the dependence even of productive advance upon the widespread enjoyment and proper use of adequate leisure and an adequate income.

NOTE.

The argument in the more technical parts of this address, concerned with the determination of the length of the working day, may be conveniently summarised with the aid of the following figure. In order to avoid the complexities arising from the redistribution of labour between the industries of a country, suppose that only one industry exists. Measure units of ti

The influences guiding the operatives are expressed in the dotted lines, the meaning of which must now be explained. Draw any vertical line dl to the left of b . Then dn is the addition made in the long run to the money income of the operative when the O nth increment of time is added to the working day. Let dm be the long-period value to the operative, when his income is $On da$, of the leisure destroyed by the addition of the O nth increment of time to the working day. The curve I is the locus of the point m . Evidently, starting at a , it will lie throughout its length below P , increasingly departing from P (because leisure is subject to the law of diminishing utility and the value of leisure rises with income), and cut OX to the left of b . Apart from the satisfaction or dissatisfaction of working, therefore, the far-sighted operative who took into account the value of leisure would choose a normal day Oi , which is less than Ob (the choice of far-sighted employers in combination). When the normal day is Oi the marginal value of leisure to an operative with a wage $Oih a$ would be ih , which equals the long-period marginal earnings attributable to the O ith increment of time in the working day. Now, let L indicate the long-period values to the operative of the effects of different lengths of working day on the absolute satisfaction or dissatisfaction involved in the labour itself, L being otherwise interpreted as I , when units of money are measured along OY' as well as along OY , and the parts of the curve below OX indicate the prices which would be paid to escape the dissatisfaction involved in working, and the parts above OX the money value of the satisfaction involved in working. As some of the time devoted to production will probably be pleasant to the operative when the length of working day is most favourable to his enjoyment of work, we may assume that L need not lie throughout its length below OX . Then the working day which perfectly wise operatives would choose would be On , the point n being such that $nm = nl$, the attainment of which equation is the condition under which the operative's satisfaction is maximised. If, as is theoretically conceivable but practically impossible, L lay further above OX for the abscissa Ob than I lay below it, the length of day most advantageous to the operative would be greater than Ob .

If normal hours are On , the operative who lives for the day, and is aware that more work, measured by results, means proportionally more pay, may be expected to desire hours longer than On for the following reasons. The product attributable to the O nth increment of working time is greater than dn , since dn represents the gain resulting from the O nth increment of working time, less the loss occasioned by the reduction which will *ultimately* take place in the productivity of the operative's earlier hours in consequence of the addition of the O nth increment of time to the working day. For similar reasons the short-period or immediate value of leisure may be less than dm . Again, the money measure of the disutility of the O nth increment of working time is less than nl , because nl measures the disutility of the last fraction of time worked, together with the disutility which results from the fact that the O nth increment of working time diminishes capacity in earlier hours to enjoy labour or sustain fatigue. It is, therefore, practically certain that the operative will experience a balance of gain from the working of the O nth unit of time, when wages, the value of leisure and the feeling involved in the work, are all taken into account, while effects on the gain or loss associated with the rest of the working day are ignored; and, further, it is practically certain that a balance of gain will continue to result directly from the work of the O nth unit of time if the working day be slightly increased, though this balance might be expected to contract. Hence we must conclude that operatives who are not alive to the reactions of long hours on efficiency and capacity to enjoy life and work will

tend to choose a longer working day than is wise from their point of view. However, to repeat, they will not approve such long hours as employers who are equally blind to future reactions, because the latter, if purely self-interested, make no allowance for the disutility of labour to the operative or the utility to him of leisure.

In the event of progress in methods of production the new position of P would be such that the area enclosed between it and the co-ordinate axes would be increased. P in its new position might cut OX at b , but in all probability the new intersection with OX would be to the left of b . It is not likely to fall to the right of b , since improvements in the mechanical aids of labour seldom mean that work is rendered less exhausting. Even if the new curve P passed through b , the new position of I would practically mean its intersection with OX to the left of i because of the enhanced value of leisure. Further, L , though it might rise higher than before would probably descend sooner and at least as steeply. It is to be observed in addition that but for interest, rent, and heavy depreciation charges, industrial progress would bring about movements of P involving more considerable augmentation of the area contained between P and the co-ordinate axes. Improved education, apart from its effect on efficiency, would bring about a subsidence of the curve I , so that in its new position it would cut OX to the left of i . The effect wrought by progress on short-period forces need not be worked out in detail. The general conclusion is manifest that progress may be expected to be accompanied by a progressive curtailment of the working day.

British Association for the Advancement of S

WINNIPEG, 1909.

ADDRESS TO THE ENGINEERING SECTION BY

SIR W. H. WHITE, K.C.B., Sc.D., LL.D., F.R.S.,

PRESIDENT OF THE SECTION.

ON the present occasion, when the meetings of the British Association for the Advancement of Science are held in the heart of this great Dominion, it is natural that the proceedings of Section G. (Engineering) should be largely concerned with the consideration of great engineering enterprises by means of which the resources of Canada have been and are being developed and the needs of its rapidly increasing population met. It will not be inappropriate, therefore, if the Presidential Address is mainly devoted to an illustration of the close connection which exists between the work of civil engineers and the foundation as well as the development of British Colonies and Dominions beyond the seas.

British colonies and possessions have started from the sea-front and have gradually pushed inland. Apart from maritime enterprise, therefore, and the possession of shipping, the British Empire could never have been created. An old English toast, once familiar but which has of late years unfortunately fallen into comparative desuetude, wished success to 'Ships, Colonies and Commerce.' A great truth lies behind the phrase: these three interests are interdependent, and their prosperity means much for both the Mother Country and its offspring. As colonies have been multiplied, their resources developed, and their populations increased, over-sea commerce between them and the Mother Country has been enlarged; greater demands have been made upon shipping for the over-sea transport of passengers, produce and manufactures; there has been a growing necessity for free and uninterrupted communication between widely-scattered portions of the Empire, the maintenance of which has depended primarily and still depends on the possession of a supreme war-fleet, under whose protection peaceful operations of the mercantile marine can proceed in safety, unchecked by foreign interference, but ever ready to meet foreign competition.

Now that our colonies have become the homes of new nations it is as true as ever that the maintenance of British supremacy at sea in both the mercantile marine and the war-fleet is essential to the continued existence and prosperity of the Empire. The trackless ocean supplies the cheapest and most convenient means of transport and intercommunication; continuous improvements in shipbuilding and marine engineering have abridged distances and given to sea-passages a regularity and certainty formerly unknown. It is a literal fact that in the British Empire the 'seas but join the nations they divide.' Every triumph of engineering draws

estimated by competent authorities that the railways under construction, and projects for extensions likely to be carried into effect in the immediate future reached a total of at least 10,000 miles, while probable further extensions of about 3,500 miles were under consideration. Further, it was estimated that the capital expenditure required to complete these schemes would be about 60 millions sterling. These figures may need amendment, but there are others representing ascertained facts which equally well illustrate the magnitude of the railway interests of the Dominion.¹ The total capital invested in Canadian railways in 1907 was officially reported to be about 234,390,000*l.*; the aid given to railways up to that date by Dominion and Provincial Governments, and by municipalities, considerably exceeded 36,000,000*l.* sterling in money; the land grants from the Dominion Government approached 32 million acres, while the Provincial Governments of Quebec, British Columbia, New Brunswick, and Nova Scotia had granted about 20½ million acres. The Governments have also guaranteed the bonds of railway companies to the extent of many millions of dollars. The capitalisation per mile of railway lines owned by the Governments (amounting to 1,890 miles) is reported as being 11,400*l.*; this is practically the same amount as that for Indian railways, that for the United States being 13,600*l.*, and for New South Wales and Victoria about 12,600*l.* For British railways the figure given is 54,700*l.* per mile. The freight carried by Canadian railways in 1907 amounted to nearly 63,900,000 tons (of 2,000 lbs.), which included about 14,000,000 tons of coal and coke, nearly 4,500,000 tons of ores and minerals, 10,250,000 tons of lumber and other forest products, nearly 7,900,000 tons of manufactures, and 2,309,000 tons of merchandise. In 1875, when 4,800 miles of railway were in operation, the corresponding freight-tonnage was 5,670,000 tons; so that while the length of railway increased nearly 4.7 times, the tonnage increased nearly 11.3 times. During the same period passengers increased from 5,190,000 to 32,137,000. For twenty-eight railways making returns the average revenue per passenger per mile was 2.232 cents, and for the four principal railways was 2.07 cents. For freight fifty-nine railways showed an average rate of 2.328 cents per ton-mile; and for the five principal railways it was .702 cent per ton-mile. The average distance travelled by a passenger was 64 miles, the corresponding figure for the United States being 30.3 miles. The average distance a ton of freight was hauled was 183 miles, as against 132 miles for the United States. In Canada, as the official reporter remarks, there is a small amount of suburban railway traffic and a low density of population. The following table is taken from the official Canadian Railway Statistics for 1907:

For each mile of Railway.

	Population,	Square miles of Territory.
United States	381	13.61
United Kingdom	1,821	5.29
France	1,590	8.46
New South Wales .	686	146.09
New Zealand . . .	358	43.42
Victoria	360	25.89
India	10,119	61.09
Canada	289	161.8

¹ Most of these statistics are taken from the valuable Report for 1907 presented to the Minister of Railways and Canals by Mr. Butler, Deputy Minister and Chief Engineer of the Department.

Canada has therefore the highest mileage measured against population, and the lowest against territory.

The earliest great railway system of Canada, the Grand Trunk, had its beginnings in 1845; in 1907 it was working about 3,600 miles within the Dominion. In association with the Government it is now engaged on the construction of the Grand Trunk Pacific Line, which will cross the Continent wholly in Canadian territory, and have a length of 3,600 miles, exclusive of branches.

The story of the Canadian Pacific Railway is well known, and need not be repeated; the influence which its existence and working have had upon the prosperity of the Dominion has been enormous and beneficial since its opening in 1885, and experience of its effect has led to the promotion of other Trans-Continental lines. In June 1907 the total length in operation was nearly 9,000 miles, and the company owned in addition great lines of steamships employed on Atlantic and Pacific services.

The Canadian Northern Railway system represents one of the most striking examples of recent railway development in the Dominion. In 1907 it was working nearly 2,600 miles in the North-Western provinces, about 150 miles in Ontario, 500 miles in the Province of Quebec, and 430 miles in Nova Scotia and Cape Breton, making a total of nearly 3,700 miles. In 1908 its mileage on the main system was reported to have increased to nearly 3,400 miles, and the total length in operation had become 4,800 miles. The North-Western Provinces have given substantial assistance to this great system, and its promoters are said to aim at a complete Trans-Continental route, as well as the development of railway communication to Hudson's Bay and the establishment of a line of steamships therefrom to Great Britain.

Besides these three great railway organisations, which in 1907 controlled about 75 per cent. of the mileage in operation, there are a large number of smaller companies, making up a total of about 80. Their total earnings in 1907 amounted to 29,350,000*l.*, the total working expenses being 20,750,000*l.* Earnings from freight service were (in round figures) 19,000,000*l.*; from passenger service 7,837,000*l.*; from express services 655,000*l.*; from mails 325,000*l.*, the balance coming from miscellaneous items. The total number of persons employed by the railways was 124,000; their salaries and wages amounted to 11,750,000*l.* It was officially estimated that if to the railway employes were added persons employed in factories for rolling stock and railway materials, as well as those engaged in the casual service and shipping, with an allowance for their families, 'quite 25 per cent. of the population win their daily bread from the carrying trade' of the Dominion.

The equipment of the Canadian railways in 1907 included 3,504 locomotives, 3,642 passenger cars, and 113,514 freight cars. In the opinion of the official reporter on railway statistics, based chiefly on a comparison of the proportion of rolling stock to mileage in Canada and the United States, a considerable increase of rolling stock is required, and there is a possibility of greater efficiency being obtained in the utilisation of existing freight cars. The manufacturing resources of the Dominion are declared to be fully capable of meeting all requirements, as in 1907 they produced 227 locomotives, 397 passenger cars, and 13,350 freight cars. A reduction of grades and curvatures has been carried out on the principal railways in recent years, and this has permitted the hauling of heavier loads. It is estimated that in 1907 the average earnings per ton of freight hauled were \$1.472, and the average earnings per passenger carried were \$1.219. The earnings per train mile were \$1.953, and the working expenses \$1.381. The total earnings per mile of railway were \$6,535.64, and the working

expenses were \$4,620.9. The working expenses were divided as follows in the official report:—

Maintenance of way and structures	20.13 per cent.
" equipment	20.88 "
Conducting transportation	55.25 "
General expenses	3.74 "

Allowing two cords of wood fuel to be equal to one ton, 5,609,000 tons of fuel—of which 5,578,000 tons were coal—were consumed by Canadian railway locomotives in 1907 in running 100,155,000 miles. The total cost was about 3,027,500*l.*, equal to 14.59 per cent. of the working expenses.

From this brief summary of facts some idea may be gained of the rapid development of Canadian railways, their immense capital value and traffic, and the remarkable influence they have had upon the progress and population of the Dominion. It is a matter for satisfaction that British capital and engineering skill have contributed in no small measure to produce this development, and it may be hoped that in the future they may render even greater service.

Inland Navigation.

The most important system of inland navigation which Canada possesses is primarily due to the existence of the Great Lakes and the St. Lawrence River; but the utilisation of these natural advantages and the construction of a continuous navigable channel from the sea to the head of Lake Superior is due to the work of engineers. The importance of such a navigable waterway leading to the heart of the Dominion was recognised long ago by the Government. The first canal is said to have been opened in 1821, and from that time onwards the canal system has been developed, but the greatest progress has been made during the last forty years under successive Administrations. Up to March 31, 1907, the capital expenditure on Canadian canals, exclusive of outlay by the Imperial Government, has approached 18,350,000*l.* sterling, of which more than ten millions have been spent on enlargements. Besides minor canal systems, many of which are important, a great 'trunk system' of water-transit has been created from Montreal to Port Arthur, at the head of Lake Superior, this all-water route being nearly 1,300 miles in length, having a minimum depth of water of 14 feet and effecting a total vertical rise of about 600 feet from tidal water in the St. Lawrence to Lake Superior. In order to effect this rise forty-nine locks are provided, most of which are 270 feet long and 45 feet wide, enabling vessels 255 feet long to be accommodated. Out of the total length of more than 1,200 miles only 73½ miles consist of artificial channels. The Welland Canal, connecting Lakes Erie and Ontario—with a total rise from lake to lake of 327 feet, effected in twenty-five locks—is 26½ miles long. This canal dates from 1824; its enlargement to present dimensions was begun in 1872, and occupied fifteen years; the total expenditure on the canal has been nearly five and a-half millions sterling. Another important section of the waterway is the Sault Ste. Marie Canal—about 6,000 feet in length and from 142 to 150 feet wide between the pier-ends, with a lock 900 feet long, 60 feet wide, having 20½ feet of water over the sills. The difference of level between Lakes Superior and Huron is 18 feet. Commenced in 1888, the Sault Ste. Marie Canal was opened for traffic in 1895, the cost being about 930,000*l.* Like its predecessor on the United States side of St. Mary's River—the so-called 'Soo' Canal affords *free passage* for the ships of both countries. In 1898 about two and three-quarter millions represented the tonnage of vessels passing through the Canadian Canal, and of this total about 403,000 tons was in Canadian vessels. In 1907 the total tonnage had risen to 12,176,000

tons, of which 2,288,000 was in Canadian vessels. The Soulanges Canal is fourteen miles long, with a rise of 84 feet effected in four locks. Commenced in 1892, it was opened for traffic in 1899, and cost nearly 1,400,000*l*. The Lachine Canal was commenced in 1821, enlarged in 1843 and 1873, and, as completed in 1901, is 8½ miles long, has 45 feet rise, effected in five locks, and has cost from first to last about 2,300,000*l*.

In the construction of this great waterway many difficult engineering problems have been solved, and every modern improvement has been introduced; electricity has been utilised in its equipment, both for power and lighting, so that navigation can proceed by night as well as by day. For the years 1903-7 the canals were declared *free of tolls*; but it is estimated officially that if tolls on the ordinary scale had been collected the revenue for 1907 would have exceeded 91,000*l*. In these five years the water-borne traffic of the Dominion increased from 9,204,000 tons in 1903 to 20,544,000 tons in 1907; in the same period the increase in Canadian railway traffic was from 47,373,000 tons to 63,866,000 tons. The official reporter justly remarks that 'these results are exceedingly encouraging.'

It was recognised long ago that the utilisation of the waterways of Canada from the Great Lakes to the sea would yield considerable advantages by facilitating cheap transport of agricultural products of the fertile regions from the great North-West, but the Canadian portions of that territory were then regarded as 'a great lone land.' Subsequent developments of the corn-growing regions of Canada have emphasised the value of the water route and its great potentialities. In his 'History of Merchant Shipping' (published 1876) Lindsay dwelt upon this point, and foresaw that if the waterways of Canada were made continuously navigable a struggle for supremacy in over-sea trade must arise between New York and the Canadian ports of Montreal and Quebec. This struggle is now in full force, so far as the grain trade is concerned, and it is likely to grow keener. The quantity of grain passed down the whole length of the St. Lawrence navigation to Montreal increased from about 450,000 tons in 1906 to 685,000 tons in 1907, while the quantity carried to Montreal by the Canadian Pacific Railway was about 387,000 tons for 1906 and 384,000 tons for 1907. On the other hand, the quantity carried by canals in the United States to New York fell from 294,500 tons in 1906 to 230,800 tons in 1907.

An important addition to the Canadian canal system has been proposed, and its execution will probably be undertaken when great works now in progress have been completed. This route extends from Georgian Bay on Lake Huron to the St. Lawrence, and would utilise Lake Nipissing as well as the French and Ottawa rivers. The distance to be traversed would be 450 miles, less than that of the present all-water route. On the basis of careful surveys it has been estimated that a canal having 20 feet depth of water could be constructed at a cost of twelve millions sterling, upon which capital a reasonable dividend could be paid, even if the charges made for transport were one-third less than the lowest rates of freight possible on United States routes to New York. It would, of course, be most advantageous to have the available depth of water increased from 14 to 20 feet, thus making possible the employment of larger and deeper draught vessels between the Lakes and Montreal. Considerable economies in the ratio of working expenses to freight earnings would be effected, break of bulk in transit to the sea would be avoided, and the cost of transport greatly reduced.

The magnitude of the grain trade and its growth may be illustrated by the following figures for recent years:—In 1897 the grain cargoes passed down the Welland Canal to the ports of Kingston and Prescott numbered 377 and represented 515,000 tons; for 1907 the corresponding figures were

518 cargoes, weighing 841,000 tons. As to the elevators and mechanical appliances for handling economically these huge quantities of grain, nothing can be said here, although they involve the solution of many difficult engineering problems and have been greatly simplified and improved as experience has been gained.

The bulk of the canal traffic, of course, moves eastwards and outwards from the interior provinces. For example, of the total quantity of freight (1,604,321 tons) passed through the whole length of the Welland Canal in 1907 about 75 per cent. moved eastwards, and more than 62 per cent. of the 2,100,000 tons which passed through the St. Lawrence canals moved in the same direction.

Shipping on the Great Lakes.

Canadian shipping and shipbuilding on the Lakes have made considerable progress in recent years, although they do not rival those of the United States. According to authoritative statements there were not twenty Canadian steamers engaged in the transport of grain fifteen years ago; only three of these were steel-built, and the largest carried only 90,000 bushels. The total carrying capacity of Canadian grain-carriers at the present time has been estimated at ten million bushels, and the capital invested in the fleet is said to be about three millions sterling. Between the harvest and the close of navigation in winter it is estimated that no less than sixty million bushels of grain can be moved from port to port in Canadian steamers.

Many special engineering features have been introduced into the structures and equipment of these Lake grain-carriers. They are really huge steel barges of full form, of uniform cross-section for a considerable portion of their length; and they possess enormous cargo capacity, moderate engine power and speed, with structures of a simple nature which can be largely standardised and made to resemble bridge-construction rather than ordinary shipbuilding. They can be built in a short time, the largest vessels occupying about four months in construction. In this way the cost of construction is cheapened, but the rates for labour and materials prevailing in the Lake shipyards are so high relatively to British costs that at present these grain-carriers are said to cost about 40 per cent. more (per ton dead weight carried) than the cost of ordinary 'tramp' steamers built in Great Britain. Their holds and hatchways are arranged so as to facilitate the rapid shipment and discharge of cargoes. At their ports of call special mechanical appliances are provided for dealing with cargoes, most of which consist of grain, ore, or coal.

In the design and construction of these cargo-handling appliances the mechanical engineer has displayed great ingenuity, and the results obtained in rate of shipment and discharge of cargoes of grain, ore and coal are remarkable. Cases are on record where vessels carrying 7,000 tons dead weight have been loaded in four hours and discharged in ten hours; more than 5,000 tons of ore have been discharged in about four hours. The draught of water of the steamers must be kept within moderate limits and the breadths of the locks are moderate, so that increase in carrying power must be chiefly obtained by increase in length; consequently, as individual cargoes are increased, a greater number of lifting appliances can be brought to bear simultaneously, and the rate of loading or discharge can be maintained or accelerated.

The season of navigation extends over only seven or eight months in the year; consequently, 'quick despatch' is essential to success. A large vessel of this class has the following approximate dimensions:—Length about 600 feet; breadth, 58 to 60 feet; depth, 32 feet; draught of water, 19 to 19½ feet

when carrying 10,000 to 11,000 tons of cargo; corresponding displacement, 16,000 tons. The engines of such a ship develop about 2,000 horse-power, and drive her at eleven to twelve statute miles per hour in fair weather. The large size and moderate speed result in very economical conditions of working, and the freight rates are exceedingly low. From official returns it appears that for these dead-weight cargoes the freight per ton mile across the Lakes is from .04 to .05 of a penny per ton mile, the corresponding railway rate being about ten times that amount. The multiplication of this type of vessel on the great Lakes is a proof that it satisfactorily fulfils the conditions of service. Similar vessels would not be well-adapted for ocean work, which demands greater structural strength, different proportions, and a more liberal equipment; but shipbuilders generally may benefit from a study of the Lake steamers.

The greater portion of the traffic on the Lakes passes through the 'Soo' canals. The voyages are comparatively short, the average length of the trip being about 840 miles. Consequently, individual vessels make several passages during the season when navigation is open, and the total number of passages as well as the total aggregate tonnage of the ships reaches very high figures. In the season of 1907, for example, when the canals were open less than 240 days 20,440 vessels (counting as a vessel each passage), with an aggregate registered tonnage exceeding 44 million tons, passed through the United States and Canadian canals at the Soo. The aggregate freight tonnage carried exceeded 58 million tons; the weight of coal approached 11½ million tons; the iron ore carried weighed 39,600,000 tons; and the grain transported amounted to 136 million bushels. The conditions of the Suez Canal are, of course, entirely different, as vessels passing through are engaged on long voyages, and individual ships make few passages in the year. On the other hand, Suez Canal traffic proceeds uninterruptedly throughout the year, while the Soo canals are closed during the winter months. Subject to these differences in working conditions, it may be of interest to state that in 1907 4,267 vessels of 14,728,000 tons passed through the Suez Canal, and paid transit dues which amounted to 4,460,000l.; whereas the passage of the 'Soo' canals was free.

The St. Lawrence Ship Channel

Closely allied with the waterway from Montreal to Lake Superior is the improvement of the channel of the St. Lawrence from Montreal to Quebec and beyond towards the sea. From the Straits of Belleisle to Montreal the distance is 986 miles; from Quebec to Montreal it is 160 miles. Formerly the minimum depth of water between Quebec and Montreal prevented the passage of vessels drawing more than 10 to 12 feet during the greater part of the season of navigation. In 1826 the question of deepening the river channel was raised; in 1844 the work was begun, but was abandoned three years later; in 1851 it was resumed, and has since been continued. In 1869 the minimum depth of the channel at low water was increased to 20 feet, in 1882 it was 25 feet; in 1888 27½ feet for 108 miles from Montreal to a point within tidal influence. A channel having a minimum width of 450 feet, and 550 to 750 feet wide at the bends, with a minimum depth of 30 feet was completed in 1906 from Montreal to tide water at Batiscan. Certain work remains to be done between this point and Quebec in order to complete the project adopted in 1889 and amended in 1906, but it is anticipated this will be finished in about four years. Below Quebec the channel is 1,000 feet wide. When once dredged it is stated that the channel remains permanent. Accidents in the channel are few. The Superintendent in his Report of July 1908 indicates the magnitude of the

work done by comparisons with the Suez and Panama Canals, the figures standing as follow:—

	Length. Miles.	Minimum depth. Feet.	Minimum breadth. Feet.	Estimated excavation. Cubic yards.
Suez Canal . .	100		100 (bottom)	
Panama Canal .	49	41	{ 200 (minimum) 500 (maximum) }	80,000,000
St. Lawrence Channel	220 ¹	39	{ 450 (minimum) 1,000 (maximum) }	70,000,000

¹ Length of channel requiring improvement demands dredging and excavation over a length of about 70 miles.

In 1844 the largest vessels navigating the St. Lawrence to Montreal were of 500 tons; now the *Virginian* and *Victorian* of the Allan Line (12,000 tons), and the *Laurentic* and *Megantic* of the White Star Line (15,000 tons), proceed to that port, and have made the passage from Quebec in less than ten hours. Ordinarily this passage occupies eleven to twelve hours, the return passage being made in nine to ten hours.

In the execution of these great works a specially designed dredging plant, including several types, has been employed, and works about seven months in the year; and the rock dredging and blasting in the section below Quebec has involved great difficulty. The total amount of rock to be removed amounted to 1,700,000 cubic yards, extending over nearly three miles, and the whole bottom was covered with huge boulders, some of which were 30 to 40 tons in weight. These great masses had to be lifted before blasting and dredging was done. During the fiscal year 1907-8 the expenditure on dredging plant and dredging was nearly 132,000*l.*, and 4,832,000 cubic yards of material were removed. At the close of that year 56 millions of cubic yards out of the estimated total of 70 millions had been dredged; the length completed to 30 feet minimum depth was 59 miles out of 70 miles. These facts indicate the advanced condition of the undertaking and the prospect of its completion at an early date.

In order to secure the safe and continuous navigation of this channel by night as well as by day, under all conditions of weather, during the season when the river is open every precaution and aid which engineering skill and invention can provide has been laid under contribution. A marine signal service, with telephonic equipment has been provided; submarine bells have been established for use in foggy weather; a complete system of buoys and lighting has been installed; the channel is periodically examined and swept to ensure that there are no obstructions; the question of prolongation of the season for navigation by the use of ice-breakers is being studied. The harbour of Montreal has been greatly improved in accommodation and equipment; and the aggregate tonnage as well as average size of sea-going vessels using the port have been much increased. In 1898, 868 such vessels aggregating 1,584,000 tons arrived at Montreal; in 1907, 742 vessels aggregating 1,926,000 tons arrived. Of the latter, 522 vessels aggregating 1,525,000 tons were British. At the St. Charles Docks and Wharves, Quebec, in the season of 1907, 235 vessels of 1,000,000 tons were entered inwards, and 67 vessels of 249,000 tons outwards, the first outward steamer leaving on April 7, and the first ocean steamer arriving on April 26. The last arrival from the sea was on December 9, and the ice formed in the tidal basin on December 12.

Still further improvements of the St. Lawrence navigation are now

proposed, and the work was commenced in 1907. It is intended to increase the depth of the channel to a minimum of 35 feet from the sea to Montreal, and the Superintending Engineer reported in 1908 that with certain moderate additions to the dredging and steam plant this work could be completed in six seasons. The widths and curves of the existing channel will not require any important changes as they were designed from the first for the largest classes of steamships. When this increased depth has been obtained Montreal as a port will have an approach channel comparing favourably with that of other ports available for Transatlantic traffic. At Southampton the existing depth at low water in the approach channel is about 32 feet, and it is proposed to obtain 34 feet. At Liverpool the minimum depth at low water over the bar and in the approach channel in the Mersey is about 28 feet. The Ambrose Channel leading to New York is to have 40 feet depth at low water when the works are completed. Ample depth of water is of the first importance in the economical working of the largest and swiftest ships, and the Canadian Government has been well-advised in deciding to carry out the great scheme above described.

Water-power.

Canada has unrivalled resources in water-power, and its extent and possible utilisation have been made the subject of investigation by engineers for many years past. One of the most important memoirs on the subject was presented to the Royal Society of Canada in his Presidential Address of 1899 by Mr. Keefer, C.M.G. In recent times many other engineers have studied the subject and carried out important works. Exact knowledge of the total power represented by the water-falls and rapids of the Dominion is not available, nor can any close estimate be made of the power which may be employed hereafter in factories, mills, or industrial processes, because profitable employment obviously depends upon commercial considerations, which must be governed largely by the localities in which water-power may be found, and the cost of works and of transmission of energy to places where it can be utilised. It has been estimated that on the line from Lake Superior through the chain of lakes and rivers leading to Niagara and thence through the St. Lawrence to the sea eleven millions horse-power may be developed.¹ Mr. Langelier has estimated that in the Province of Quebec the water-power aggregates more than eighteen millions horse-power; other provinces all possess large resources of the same kind as yet untouched. The most striking example of the utilisation of water-power is that on the Niagara River, which the writer had the good fortune to visit in 1904, during his Presidency of the Institution of Civil Engineers; the works on the Canadian side were then in full progress, and at a stage which enabled one to realise completely their great difficulty and immense scale. The three companies whose works are near the Falls on the Canadian side have provided for a total ultimate development of over 400,000 horse-power, and a fourth establishment lower down the river, intended chiefly for the use of Hamilton, is to develop 40,000 horse-power. In the construction of the works, in the electric generating plant, the arrangements for transmitting power over long distances, and other features of importance remarkable engineering skill and daring have been displayed. American capital and enterprise have had much to do with these undertakings, as they have with many other important Canadian enterprises; but it may be hoped that British capital will keep its lead and be freely employed in the development and utilisation of all the resources of the Dominion, including that magnificent asset its water-power. The applica-

¹ The *Times Financial Supplement*, April 2, 1906, contains a valuable article on this subject, from which many of the above figures are taken.

TRANSACTIONS OF SECTION G.

tions of water-power are already very numerous, including not merely the creation of electrical energy and its use for lighting and power in towns and factories situated at considerable distances from the Falls, but for manufactures and industrial processes carried on near the Falls. Amongst these manufactures, that of aluminium and carbide of calcium may be mentioned, while paper- and pulp-mills and saw-mills constitute important industries. Great advances have been made in the transmission of electrical power over long distances, and very high pressures are being used. Electric traction on railways and tramways also derives its power from the same sources, and is being rapidly developed. In 1901 there were 553 miles of electric railways, and in 1907 815 miles.

Over-sea Trade and Transport.

It was remarked at the outset that a great truth is embodied in the old toast of 'Ships, Colonies, and Commerce,' and the efficient and economical transport of passengers, produce, and manufactured goods between the Dominions beyond the Seas and the Mother Country is essential both for the development of Colonial resources and for the continued prosperity of the United Kingdom. The British mercantile marine commands the larger portion of the carrying trade of the world; its earnings constitute a valuable item in the national income; it forms one of the strongest bonds of union between the various parts of the Empire. This general statement may be illustrated by reference to the over-sea trade of Canada and to the shipping engaged therein.

The total value of the Imports and Exports of the Dominion in 1898 was close upon 61 millions sterling; in 1908 it exceeded 130 millions sterling, having more than doubled within ten years. During the year ending March 31, 1908, the vessels which were entered at Canadian ports (*inwards from the sea*) carrying cargoes were classified as follows in the official returns:—

Ships.	Tons register.	Freight carried.		Crews.
		Tons weight.	Tons measurement.	
British . 2,603	4,530,256	1,306,822	254,373	165,078
Canadian . 2,803	718,490	202,939	1,449,054	44,594
Foreign . 2,878	1,758,549	887,154	36,618	86,293
Totals . 8,284	7,016,295	2,396,915	1,740,045	295,965

The corresponding figures for ships entered outwards for sea carrying cargoes were:—

Ships.	Tons register.	Freight ca		
		Tons weight.	Tons measurement.	
British . 2,533	4,258,960	2,706,334	714,085	136,614
Canadian . 3,557	1,041,053	616,248	291,480	45,658
Foreign . 4,132	2,211,605	1,454,787	538,499	88,093
Totals . 10,222	7,511,618	4,777,369	1,544,064	

Taking the combined over-sea traffic inwards and outwards, it employed 18,506 ships of 14,528,000 tons, whose cargoes aggregated 7,174,000 tons

dead-weight and 3,284,000 measurement tons, the crews exceeding 576,000 officers and men.

Of the 2,603 British ships entered inwards there came from Great Britain 852 ships of 3,392,000 tons, carrying as cargoes over 860,000 tons dead-weight and 153,600 tons measurement; while there came from British Colonies 399 ships of nearly 381,000 tons, carrying cargoes of 236,000 tons dead-weight and 44,000 tons measurement. Of the 2,533 British ships entered outwards there proceeded to Great Britain 732 ships of 2,529,000 tons, carrying cargoes of 1,635,000 tons dead-weight and 509,000 tons measurement; while there sailed for British Colonies 648 ships of nearly 400,000 tons, carrying cargoes of 259,000 tons dead-weight and 76,500 tons measurement.

It will be seen, therefore, that the British ships entered inwards carried more than 54 per cent. of the total dead-weight cargoes and $14\frac{1}{2}$ per cent. of the measurement goods, while foreign ships carried about 37 per cent. of the dead-weight and rather more than 2 per cent. of the measurement goods. British ships entered outwards carried more than 56 per cent. of the total dead-weight, and more than 46 per cent. of the measurement; whereas foreign ships carried only about 30 per cent. of the dead-weight, and not quite 35 per cent. of the measurement.

The trade from and to ports in the British Empire amounted to 45 per cent. of the grand total dead-weight freight; and ships carrying the British flag—excluding Canadian vessels—carried about 56 per cent. of the grand total dead-weight, and nearly 30 per cent. of the measurement goods. Including Canadian vessels, the British Empire can claim possession of $67\frac{1}{2}$ per cent. of the total dead-weight trade, and $82\frac{1}{2}$ per cent. of the measurement goods. The average tonnage per ship for the British was about 1,700 tons; for the Canadian vessels less than 300 tons; for the foreign ships a little more than 900 tons.

It may be interesting to add a few figures showing the magnitude of the *coasting trade* of the Dominion. In 1908 there arrived and departed 104,527 steamers aggregating nearly 42,857,000 tons, and 50,710 sailing ships aggregating 7,673,000 tons. The sailing ships included nearly 50,200 small schooners, sloops, barges, canal boats, &c., averaging about 150 tons each. The grand totals for the coasting trade were 155,237 ships of 50,530,000 tons, and of these 151,873 ships of 47,356,000 tons were classed as British in the official returns. It will be obvious that great importance must attach to every detail of the business involved in carrying on a shipping trade of the magnitude indicated by the foregoing figures, and still more is this the case in regard to the immensely greater transactions of British shipping considered as a whole. No pains must be spared in promoting economy or improving procedure, and even minute savings on particular items must be secured, since their aggregate effect may be of vast amount.

Since the introduction of iron for the structures of ships and of steam as the propelling power marvellous economies have been effected in the cost of over-sea transport. The chief causes contributing to this result have been (1) improvements in steam machinery, leading to great reductions in coal consumption, (2) considerable enlargement in the dimensions of ships, and (3) the supersession of iron by steel for structures and machinery. It is unnecessary, and would be impossible on this occasion, to deal in any detail with these matters, which have been illustrated repeatedly by many writers, including the speaker. On the other hand, it would be improper to leave altogether without illustration the remarkably low cost of sea transport under existing conditions, since it has great influence on the commerce of the British Empire and of the world.

Rates of freight, of course, vary greatly as the conditions of trade and the stress of competition change. At the present time these conditions remain unfavourable, although it may be hoped that there are signs of improvement, after long and severe depression. It will be preferable, therefore, to give facts for more normal circumstances, such as prevailed five or six years ago. Coal was then carried from the Tyne to London (315 miles) for 3s. 3d. a ton; to Genoa (2,388 miles) for 5s. a ton; to Bombay (6,358 miles) for 8s. 6d. a ton, including Suez Canal dues. The corresponding rates of freight were .111, .025, and .016 of a penny per ton-mile.

Grain was brought across the Atlantic for 9d. per quarter in large cargo steamers, whereas in former times, when it was carried in small vessels, the charge was 9s. 6d. Goods were carried 6,400 miles eastward *via* the Suez Canal in tramp steamers at an inclusive charge of 25s. to 30s. a ton, the freight rate averaging about .05 of a penny per ton-mile. It was estimated at that time that the average railway rate per ton-mile in Great Britain for cost of transport and delivery of goods was about thirty times as great; but the moderate distances travelled, local and national taxation, high terminal charges, and the immense outlay involved in the construction, equipment, and maintenance of railways account for much of the great difference in cost of transport. The ocean furnishes a free highway for the commerce of the world.

Economy of fuel-consumption has played a great part in the reduction of working expenses in steamships. Fifty years ago from 4 to 5 lbs. of coal per indicated horse-power represented good practice in marine engineering for screw steamships. At present, with quadruple expansion engines, high-steam pressures, and more efficient reciprocating engines from 1½ to 1½ lb. is common practice, and better results are claimed in some cases. A cargo steamer of the tramp type, carrying 6,500 tons dead-weight, can cover about 265 knots in twenty-four hours in fair weather for a coal consumption of 27 tons per day, representing an expenditure on fuel of 20% to 25%. A larger vessel carrying about 12,000 tons dead-weight, driven by engines of similar type, would consume about 45 tons in covering the same distance at the same speed. This increased economy in fuel per ton-mile is the result of an increase in dimensions from 365 feet length, 47 feet breadth, and 24½ feet draught of water to a length of 470 feet, a breadth of 56 feet, and a draught of 27½ feet. The first cost of cargo steamers is small in relation to their carrying capacity and possible earnings; varying, of course, with the current demand for new steamships. In the present depressed condition of shipping, about 5% 10s. per ton dead-weight is named as a current rate; in busy times the price may be 40 to 45 per cent. higher; even then it is small in proportion to earning power. Working expenses are kept down also by the use of efficient appliances for rapidly shipping or discharging cargoes, and so shortening the stay of ships in port. As an example a case may be mentioned when a ship of 12,000 tons dead-weight and 800,000 cubic feet measurement capacity had her full cargo discharged at an average rate of 300 tons an hour, a fresh cargo put on board at the rate of 250 tons an hour, and 1,600 tons of coal shipped between 7 A.M. on Monday and noon on the following Friday—that is, in 101 hours. In another case a cargo weighing 11,000 tons was discharged in 66 hours. ‘Quick dispatch’ in dealing with cargo is now universally recognised as essential, and it has been asserted that a saving of one day in discharging or loading a tramp steamer when she finds full employment may involve an expense equal to 1 per cent. on her first cost.

The ‘intermediate’ type of steamer—in which large carrying capacity is combined with provision for a considerable number of passengers and moderate speed—is of comparatively recent date, but it has been developed rapidly

and is subject to the universal laws to which all classes of shipping conform. Increase of size is adopted in order to favour economy in working and greater earning power, while increase in speed is made in some cases. Vessels like the *Adriatic* or *Baltic* of the White Star Line, the *Carmania* and *Caronia* of the Cunard Line, and the *George Washington* of the Hamburg-American Line illustrate this statement; while its latest and greatest examples are found in the two steamers now building for the White Star Line by Messrs. Harland and Wolff, which are said to be of 45,000 tons, to be intended to steam twenty to twenty-one knots, to provide accommodation for a great number of passengers, and to have large capacity for cargoes. In mail and passenger steamers of the highest speed increase in dimensions is devoted chiefly to provision for more powerful propelling apparatus and for a correspondingly large quantity of fuel, and the cargo-carrying capacity is relatively small; but the law of increase in size and cost is obeyed, and will be followed up to the limit which may be fixed by the vast outlay necessary in order to provide suitable harbours and dock accommodation with an adequate depth of water, or by commercial considerations and the possibility of securing a suitable return on the large capital expenditure. Growth in dimensions of ships will not be determined by the naval architect and marine engineer finding it impossible to go further, for there are even now in view possibilities of further progress if the shipowner so desires. Invention and improvement have not reached their ultimate limits.

The wonderful progress made during the last seventy years is well illustrated by the history of shipping trading between Canada and Great Britain, and it may be of interest to recall a few of the principal facts. For a long period trade and communications were carried on by wood-built sailing ships, many of the finest being Canadian built; but at a very early period Canadians had under consideration the use of steamships. One of the first steamers to cross the Atlantic was the *Royal William* paddle-steamer, built near Quebec in 1831. She was 160 feet long, 44 feet broad, of 363 tons burden, sailed from Quebec on August 5, 1833, and reached Gravesend on September 16, a passage of more than forty days, in the course of which sail-power was largely used. Cabot, in 1497, crossed in the good ship *Matthew*, of 200 tons burden, which was probably from 90 to 100 feet in length; so that three centuries of progress had not made very great changes in size of the ships employed. Wood was still the material of construction, and sails were still used as a motive power, although the steam-engine was installed. In 1839 it was a Canadian, Samuel Cunard, who secured—in association with two British shipowners, Burns and McIver—the contract for a monthly Trans-atlantic service from Liverpool to Halifax and Boston. The four steamers built were wood-hulled, driven by paddle-wheels, had good sail-power, and were of the following dimensions: 207 feet long, 34½ feet broad, 1,150 tons burden, and about eight knots speed. A rapid passage to Boston then occupied about fourteen days.

Another Canadian enterprise, the Allan Line, started about fifty-six years ago. The first steamer built for the company was appropriately named the *Canadian*. At the time of her construction she ranked among the most important mercantile steamers in existence, and was quite up to date. Her dimensions were: Length, 278 feet; breadth, 34 feet; burden, 1,873 tons. She had inverted direct-acting engines, driving a screw propeller, and a full sail equipment.

The Trans-atlantic service to New York, as was natural, rapidly surpassed that to Canadian ports, but the latter has been continuously improved, and its development has been marked by many notable events. For example, the Allan Line was amongst the first to use steel instead of iron for hulls, and in their two largest steamers now on service, dating from

1903, they were the first to adopt steam turbines for ocean-going ships, although their lead of the Cunard Company was not long. The *Virginian* and *Victorian* are 520 feet long, 60 feet broad, of 10,750 tons, and their maximum speed is 18 knots. The Canadian Pacific Railway authorities added shipowning to their great land enterprises at an early period in their career by building for the Pacific service in 1891 three important steamers, each 456 feet long, 51 feet broad, of 5,950 tons, and 17 knots speed. These vessels continue on service, and have done splendid work as a link in the 'all red' route. Since this step was taken the Canadian Pacific Railway has become possessed of a large fleet of Atlantic steamships, and quite recently has placed on the service from Liverpool to Quebec passenger steamships nearly 550 feet in length, 66 feet in breadth, of 14,200 tons, with a maximum speed of 20 knots.

The latest addition to the Canadian service has been made by the White Star Line in the form of two steamers, the *Laurentic* and *Megantic*, of 15,000 tons, 550 feet long, about 67 feet broad, and 17 knots speed. In the *Laurentic* an interesting experiment has been made. Messrs. Harland & Wolff having introduced a combination of reciprocating engines and a low-pressure turbine. This system was patented as long ago as 1894 by Mr. Charles Parsons, to whom the invention of the modern steam turbine and its application to marine propulsion are due. Mr. Parsons foresaw that while the turbine system would prove superior to reciprocating engines in ships of high speed and with a high rate of revolution, there would be a possibility of getting better results by combining reciprocating engines with low-pressure turbines in ships of comparatively slow speed, where a low rate of revolution for the screw-propellers was necessary to efficient propulsion. His main object, as set forth fifteen years ago, was 'to increase the power obtainable by the expansion of the steam beyond the limits possible with reciprocating engines,' and subsequent investigations led Mr. Parsons to the conclusion that it would be possible to secure an economy of 15 to 20 per cent. by using the combination system as compared with that obtainable with efficient types of reciprocating engines. Many alternative arrangements have been designed for combining reciprocating engines with low-pressure turbines; that now under trial associates twin-screw reciprocating engines, in which the expansion of the steam is carried down to a pressure of 9 to 10 lbs. per square inch when working at maximum power, and then completed to the condenser pressure in a turbine. Triple screws are employed, the central screw—driven by the turbine running at a higher rate of revolution than the side screws, which are driven by the reciprocating engines. The *Laurentic* has been but a short time on service, and few particulars are available of her performances as compared with those of her sister ship, fitted with reciprocating engines. It has, however, been reported that the results have proved so satisfactory that the combination system will probably be adopted in the two large White Star steamers of 45,000 tons now building at Belfast. This favourable view is fully confirmed by the performances of the *Otaki*, built by Messrs. Denny, of Dumbarton, for the New Zealand Shipping Company, and completed last year. That firm, as is well known, have taken a leading part in the application of the Parsons type of steam turbine to the propulsion of mercantile and passenger steamers, and they possess exceptional experience as well as special facilities for the analysis of the results of trials of steamships, having been the first private firm to establish an experimental tank for testing models of ships and propellers on the model of that designed by Mr. W. Froude and adopted by the Admiralty. Messrs. Denny have generously placed at the disposal of their fellow-shipbuilders the principal results obtained on the official trials and earliest voyages of the *Otaki*, and have compared them with

similar results obtained in sister ships fitted with reciprocating engines.¹ The *Otaki* is the first completed ship fitted with the combination system and subjected to trial on service, and as the successful application of that system to cargo steamers and steamers of the intermediate type would result in a considerable economy in the cost of oversea transport, it may be of interest to give some details of her recorded performance. She is 465 feet long, about 60 feet broad, and of 7,420 tons (gross). Her dead-weight capability is about 9,900 tons on a draught of 27 feet 6 inches, and the corresponding displacement (total weight) is 16,500 tons. The vessel was designed for a continuous sea-speed of 12 knots when fully laden, and the contract provided for a trial speed of 14 knots with 5,000 tons of dead-weight on board. The trials were accordingly made at a displacement of about 11,700 tons. Her installation of boilers is identical with that of her sister ship, the reciprocating-engined twin-screw steamer *Orari*, which is 4 feet 6 inches shorter than the *Otaki*, but generally of the same form. On the measured mile the *Otaki* obtained a speed of 15 knots, while the *Orari* reached 14·6 knots. In order to drive the *Orari* at 15 knots about 12 per cent. more horse-power would have been required, and this is a practical measure of the superiority of the combination system over the reciprocating twin-screw arrangement in the *Orari*. The total water consumption per hour of the *Otaki* at 15 knots was 6 per cent. less than that of the *Orari* at 14·6 knots. If the *Otaki* also ran at 14·6 knots, the water consumption would have been 17 per cent. less than that of the *Orari* at the same speed. On the voyage from Liverpool to New Zealand the *Otaki* averaged about 11 knots, which would have required on the measured mile only about 40 per cent. of the power developed when running 14·6 knots. With the ship laden more deeply, the average development of power on the voyage was about one-half the maximum developed on the measured mile, and this was disadvantageous to economy in the combination. Even in these unfavourable conditions the *Otaki* realised an economy in coal consumption of 8 per cent. on the voyage from Liverpool to New Zealand and back as compared with her reciprocating-engined sister ship; this represents a saving of about 500 tons of coal. Ordinarily the ship would leave England with sufficient coal on board for the outward passage, so that 250 tons less coal need be carried and a corresponding addition could be made to cargo and freight-earning. Probably as experience is gained the actual economy will prove greater than that realised on the maiden voyage; but even as matters stand there is a substantial gain, and a prospect of the extended application of the steam turbine to vessels of moderate and low speed. In view of results already obtained, the New Zealand Shipping Company have decided to apply the combination system to another vessel just ordered from Messrs. Denny.

In designing turbine machinery for vessels of moderate or low speed there must necessarily be conflicting claims. For maximum efficiency in steam turbines a high rate of revolution is necessary; whereas at moderate or low speeds it is antagonistic to propeller efficiency to run at this high rate of revolution. Engineers are at present much occupied with the study of arrangements by means of which these conflicting claims may be harmonised and greater total efficiency of propulsion obtained. Having regard to the enormous capital invested in cargo steamers of moderate speed, and the importance attaching to their economic working as influencing the cost of oversea transport, it will be obvious that it is most desirable to find an arrangement in which the high speed of the rotor may be reduced by means of some form of gearing or its equivalent, so as to enable the screw shaft and

¹ See a Paper by Engineer Commander Wisnom, R.N., in the *Proceedings of the Institution of Engineers and Shipbuilders in Scotland* for 1909.

its propeller to be run at a speed which will secure maximum propeller efficiency. Many proposals have been made, including mechanical gearing and hydraulic or electric apparatus for transforming the rate of motion. Some of these are actually undergoing experimental trials, and are said to have given very promising results. One of the most important trials is that undertaken by the Parsons Marine Steam Turbine Company, which has purchased a typical tramp steamer, and is carrying out on her a series of trials in order first to ascertain accurately what are the actual conditions of steam and coal consumption with the present reciprocating engines, and then to ascertain the corresponding facts when those engines have been removed and a steam turbine with its associated gearing has been fitted. It is interesting to note in passing that in the earliest days of screw propulsion with slow-running engines it was found necessary to adopt gearing in order to increase the rate of revolution of the propellers, whereas at present interest is centred in the converse operation. Furthermore, if any system of gearing-down proves successful it may be anticipated that its application will be extended to swift turbine-driven steamships, since it would enable good propulsive efficiency to be secured in association with rapidly running turbines of smaller size and less weight than have been employed hitherto.

The Marine Steam Turbine.

The rapid development of the marine steam turbine during the last seven years constitutes one of the romances of engineering, and the magnitude of the work done and the revolution initiated by Mr. Charles Parsons will be more justly appreciated hereafter than it can be at present. In some quarters there is a tendency to deal critically with details and to disregard broader views of the situation as it stands to-day. In May 1909 there were 273 vessels built and under construction in which steam turbines of the Parsons type are employed, the total horse-power being more than three and a half millions. In the Royal Navy every new warship, from the torpedo-boat up to the largest battleships and armoured cruisers, is fitted with turbine engines; and the performances of vessels which have been tested on service have been completely satisfactory, in many instances surpassing all records for powers developed and speeds attained. In the war-fleets of the world this example is being imitated, although in some cases it was at first criticised or condemned. In the mercantile marine as a whole, while the new system has not made equal advance, many notable examples can be found of what can be accomplished by its adoption. It is now admitted that steam turbines enable higher speeds to be attained in vessels of given dimensions; and in steamers built for cross-channel and special services, where high speed is essential and coal consumption relatively unimportant, turbines have already ousted reciprocating engines. For oversea service and long voyages an impression has existed that the coal consumption of turbine-engined ships would considerably exceed that of ships driven by triple or quadruple expansion reciprocating engines. Critics have dwelt on the reticence in regard to actual rates of coal consumption practised by owners of turbine steamships. Naturally there are other reasons for reticence than those which would arise if the coal consumption were excessive; but pioneers in the use of turbine machinery may reasonably claim the right of non-publication of results of trials in the making of which they have incurred large expenditure and taken considerable risks if they think that silence is beneficial to their business interests. Even if it were true that in the earliest applications of the new system economic results had not been obtained equal to those realised in reciprocating engines which have been gradually improved during half a century, that circumstance should

not be regarded as a bar to acceptance of a type of engine that admittedly possesses very great advantages in other ways, but should be regarded as an incentive to improvements that would secure greater economy of coal. The evidence available, however, does not confirm the adverse view, and those familiar with the facts do not admit its truth. One example may be cited as it affects the Canadian service. In June 1907 it was authoritatively stated that in the Allan liner *Virginian* the reports which had been circulated respecting the excessive coal consumption were unfounded, that the vessel was making passages at speeds of $17\frac{1}{2}$ to $17\frac{3}{4}$ knots, as against the 17 knots estimated, and the rate of coal consumption was really about 1.4 lbs. per indicated horse-power which would have been required to attain this speed if the vessel had been fitted with reciprocating engines. This result compares well with the consumption in ordinary passenger steamers running at high speeds in proportion to their dimensions, although in large *argo* steamers and vessels of the intermediate type, working under much easier conditions and at very low speeds in proportion to dimensions, lower rates of consumption may be obtained. With these latter vessels the fair comparison is the combination system and not the pure turbine type which is adapted for high speeds.

The crowning triumph of the marine steam turbine up to the present time is to be found in the great Cunard steamships *Lusitania* and *Mauretania*. The passages made this year by the latter ship since she was refitted have been marvellously regular, and the 25 knots average across the Atlantic, which was the maximum contemplated in the agreement between the Government and the Cunard Company, has been continuously exceeded. As one intimately concerned with the design of the *Mauretania*, who has had large experience in ship design, has made a life-long study of the laws of steamship performance, and had the honour of serving on the committee which recommended the employment of turbines in these great ships, the writer ventures to assert that equal results could not possibly have been obtained with reciprocating engines in vessels of the same form and dimensions. Contrary opinions have been expressed, but they have been either based upon incorrect data or have omitted consideration of the fact that in vessels of such great engine-power it was necessary to have time to perfect the organisation of the staff in order to secure uniform conditions of stoking and steam production, and to bring the 'human element' into a condition which would ensure the highest degree of efficiency in working the propelling apparatus. This necessity for time and training has been illustrated again and again in the case of new types of Transatlantic steamers, including some which held the record for speed prior to the appearance of the Cunarders. In the *Lusitania* and *Mauretania* the engine-power is fully 60 per cent. greater than that of their swiftest predecessors, yet no similar allowance appears to have been thought necessary by some critics, who assumed that performances on the earlier voyages represented the maximum capabilities of the vessels. Subsequent events have shown this view to be fallacious and have justified the recommendation of the Turbine Committee and the action of the Cunard directors. Allegations made in regard to excessive coal consumption have also been disproved by experience; and in this respect the anticipations of the committee and of Mr. Parsons have been fully realised.

The marvellous regularity maintained by the *Mauretania* on a long sequence of consecutive Transatlantic passages—made under varying and in many cases very adverse conditions of wind, weather, and sea—illustrates once more, and on an unprecedented scale, the influence which large dimensions have upon the power of maintaining speed at sea. Starting from the eastward passage, beginning on February 3rd last, and taking twelve passages (westward and eastward) which followed, the average

speed for the thirteen passages, approaching 40,000 sea miles in length, has been $25\frac{1}{2}$ knots; the lowest average speed in the series has been 25.2 knots, the highest average speed 25.88 knots. Many of the winter passages in this series were made in winter weather against strong winds and high seas, which would have considerably reduced the speed of her predecessors, but had small influence on the *Mauretania*. In many instances delays have been caused by fogs.

On seven consecutive passages made since the beginning of last May the average speed of the *Mauretania* in covering about 20,000 sea-miles has been 25.68 knots, the minimum speed for the passage having been 25.62 knots and the maximum 25.88 knots. On her contract trials, the *Mauretania* maintained an average speed of 26.04 knots for a distance somewhat exceeding 1,200 knots, the steaming time being rather less than forty-eight hours. On the passage when she averaged 25.88 knots, she ran 1,215 knots from noon on June 17th to noon on June 19th (about forty-six hours), at an average speed of 26.23 knots, and by noon on the 20th had covered 1,817 knots at an average speed of 26.18 knots for 69 hours. The ship has, therefore, surpassed on service her performance on the contract trial.

In view of the foregoing facts and of others of a similar nature, it is reasonable to assume that as experience is enlarged and information is accumulated in regard to forms of propellers likely to prove most efficient in association with quick-running turbines, sensibly improved performances will be obtained. At present, in comparisons made between the efficiency of reciprocating-engined ships and turbine-engined ships, the former have the great advantage attaching to long use and extended experiment; but this is not a permanent advantage, and it may be expected that good as the position is to which the marine steam turbine has attained in the brief period it has been in practical use, that position will be gradually improved. Whether or not other forms of propelling apparatus in their turn will surpass the steam turbine it would be unwise to predict. Internal combustion engines are regarded in some quarters as dangerous and probably successful rivals to steam turbines in the near future. Within certain limits of size, internal combustion engines no doubt answer admirably; but as dimensions and individual power of the engines are increased, the difficulties to be overcome also rapidly increase, and the fact is fully recognised by those having the best knowledge of those types of prime movers. On the whole, therefore, it seems probable that the turbine will not soon be displaced, whatever may happen eventually.

An Imperial Navy.

Three centuries ago a great English seaman and coloniser wrote these words:—

‘Whomsoever commands the sea commands the trade;

Whomsoever commands the trade of the world commands the riches of the world, and consequently the world itself.’

In these words Sir Walter Raleigh clearly expressed the doctrine of ‘sea-power,’ which in recent times has been emphasised by Admiral Mahan of the United States Navy and other writers. Twenty years ago when the movement began which has been followed by an unprecedented series of ship-building programmes, great additions to the *personnel* of the Royal Navy and large expenditure on improvements of existing naval bases and the creation of others at important strategical points, the same truth was expressed in a report made by three distinguished Admirals, one of whom, Admiral of the Fleet, Sir Frederick Richards, subsequently became First Naval Lord of the Admiralty, and did much to give effect to the policy he

had joined in recommending. One passage in this report may be quoted :— ' No other nation has any such interest in the maintenance of an undoubted superiority at sea as has England, whose seaboard is her frontier.' ' England ranks amongst the great powers of the world by virtue of the naval position she has acquired in the past, and which has never been seriously challenged since the close of the last great war. The defeat of her Navy means to her the loss of India and her Colonies, and of her place amongst the nations.'

The ' maintenance of an undoubted superiority at sea ' in existing circumstances and in face of foreign competition is no easy task, and it is good to know that the Dominions beyond the Seas are ready to take a share of the heavy burden of Empire. In what way effect can best be given to this fundamental idea it is not easy to decide. It is necessarily a matter in which the views of all concerned must be considered, and a policy determined on which shall command hearty support from all portions of the Empire. It may be presumed that the arrangement of such a policy has been the chief object of this year's Defence Conference. The decision which may be reached and the action taken must exercise momentous influence upon the destiny of the Empire. Universal approval has been given to the arrangement for that Conference, and this is a happy augury of its ultimate success in framing a satisfactory scheme for the construction and maintenance of an Imperial Navy. Many valuable suggestions have been made by British and Colonial authorities as to the great lines on which such a scheme should be drawn, but this is not the place to enter upon a discussion of the subject. It may be permitted, however, as a sequence to the preceding remarks on overseas transport, to remark that the protection of trade routes between the Mother Country and the Dominions beyond the Seas constitutes an essential duty ; in the performance of which duty, especially in portions of trade routes adjacent to the Colonies, naval forces maintained by the Colonies may render valuable service. Such a policy in no way infringes the fundamental condition that supremacy at sea ultimately depends upon battle-fleets ; while it recognises the fact, which past struggles have demonstrated, that behind and beyond the work of battle fleets lies the need for adequate protection of commerce and communications. Moreover, it leaves Colonial Governments unfettered in making arrangements for the execution of that portion of the general scheme of defence which they may undertake ; and there can be no inconvenience or loss from such independent action provided the scheme of Imperial defence has been considered as a whole, and an understanding reached in regard to the distribution of the work. At present the Mother Country alone possesses experience and means of manufacturing warships and armaments ; so that gradual developments, requiring time and experience, will be necessary before the Colonies can become self-supporting in these respects should they desire to do so. On the side of *personnel* and its training also the Royal Navy must be the great school for all parts of the Empire. Finally, the full utilisation of Imperial defensive forces demands the existence of a complete understanding and the pre-arrangement of a common plan of campaign. In order to meet this essential condition there must be an Imperial staff.

The burden of naval defence has hitherto been borne almost entirely by the Mother Country. What the weight has been is hardly realised until the figures for expenditure are examined. As indications of what is involved in creating and maintaining a modern navy of the first class, it may be mentioned that in the ten financial years of the present century (including the current year 1909-10) the total expenditure on the Royal Navy amounts to 328 millions sterling. From 1885 to 1902, during the period the writer occupied the position of Director of Naval Construction and Assistant

TRANSACTIONS OF SECTION G.

Controller of the Navy, the total outlay on the 245 ships for the designs of which he was responsible, amounted to about 100 millions sterling. The stress of foreign competition and the growth in dimensions and cost of warships is leading to still greater expenditure on the Navy, and it is good to know that Canada, Australia, New Zealand, and South Africa are ready and willing to bear their share of the inevitable burden.

All branches of engineering have been and will be drawn upon freely in the execution of this great task. Mining and metallurgy assist by the production of materials of construction; mechanical and electrical engineers contribute machines and appliances required in shipyards and engine factories, as well as guns, gun-mountings, and mechanical apparatus of all kinds required in modern warships in order to supplement and economise manual power; marine engineers design and construct the propelling apparatus, and constantly endeavour to reduce the proportion of weight and space to power developed; naval architects design and build the ships; constructional engineers are occupied in the provision of docks, harbours, and bases adapted to the requirements of the fleet; and other branches of engineering play important, if less prominent parts. The progress of invention and discovery is increasing, rapid changes occur unceasingly, the outlay is enormous, the task is never ending, but its performance is essential to the continued well-being of the Empire, and it must and will be performed.

British Association for the Advancement of Science.

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ADDRESS TO THE ANTHROPOLOGICAL SECTION

BY

PROFESSOR JOHN L. MYRES, M.A., F.S.A.,

PRESIDENT OF THE SECTION.

The Influence of Anthropology on the Course of Political Science.

ANTHROPOLOGY is the Science of Man. Its full task is nothing less than this, to observe and record, to classify and interpret, all the activities of all the varieties of this species of living being. In the general scheme of knowledge, therefore, anthropology holds a double place, according to our own point of view. From one standpoint it falls into the position of a department of zoology, or geography; of zoology, since man, considered as a natural species, forms only one small part of the animal population of this planet; of geography, because his reason, considered simply as one of the forces which change the face of nature, has, as we shall see directly, a range which is almost worldwide. From another point of view anthropology itself, in the strictest sense of the word, is seen to embrace and include whole sciences such as psychology, sociology, and the rational study of art and literature; since each of these vast departments of knowledge is concerned solely with a single group of the manifold activities of man. In practice, however, a pardonable pride, no less than the weighty fact that man, alone among the animals, truly possesses reason, has kept the study of man a little aloof from the rest of zoology. Dogmatic scruples have intervened to prevent man from ever ranking merely as one of the 'forces of nature,' and have set a hard problem of delimitation between historians and geographers. And the pardonable modesty of a very young science—for modern anthropology is barely as old as chemistry—has restrained it from insisting on encyclopædic claims in face of reverend institutions like the sciences of the mind, of statecraft, and of taste.

Yet when I say that anthropology is a young science I mean no more than this, that in the unfolding of that full bloom of rational culture, which sprang from the seeds of the Renaissance, and of which we are the heirs and trustees, anthropology found its place in the sunlight later than most; and almost alone among the sciences can reckon any of its founders among the living. This was of course partly an accident of birth and circumstance; for in the House of Wisdom there are many mansions; a Virchow, a Bastian, or a Tylor might easily

have strayed through the gate of knowledge into other fields of work ; just as Locke and Montesquieu only narrowly missed the trail into anthropology.

But this late adolescence was also mainly the result of causes which we can now see clearly. Man is, most nearly of all living species, the 'ubiquitous animal.' Anthropology, like meteorology, and like geography itself, gathers its date from all longitudes, and almost all latitudes, on this earth. It was necessary therefore that the study of man should lag behind the rest of the sciences, as long as any large masses of mankind remained withdrawn from its view ; and we have only to remember that Australia and Africa were not even crossed at all—much less explored—by white men, till within living memory, to realise what this limitation means. In addition to this, modern Western civilisation, when it did at last come into contact with aboriginal peoples in new continents, too often came, like the religion which it professed, bringing 'not peace but a sword.' The customs and institutions of alien people have been viewed too often, even by reasonable and good men, simply as 'ye beastlie devices of ye heathen,' and the pioneers of our culture, perversely mindful only of the narrower creed, that 'he that is not with us is against us,' have set out to civilise savages by wrecking the civilisation which they had.

Before an audience of anthropologists, I need not labour the point that it is precisely these two causes, ignorance of many remoter peoples, and reckless destruction or disfigurement of some that are near at hand, which are still the two great obstacles to the progress of our science. But it is no use crying over spilt milk, and I turn rather to the positive and cheering thought that the progress of anthropology has been rapid and sure, in close proportion to the spread of European intercourse with the natives of distant lands ; and that its further advance is essentially linked with similar enterprises.

Anthropology and Politics in Ancient Greece.

Instances of what I mean are scattered over the whole history of anthropology. Philosophy, as we all know, begins in wonder ; it is the surest way to jostle people out of an intellectual groove into new lines of thought, if they can be confronted personally and directly with some object of that numerous class which seems uncouth only because it is unfamiliar. The sudden expansion of the geographical horizon of the early Greeks, in the seventh and sixth centuries, B.C., brought these earliest and keenest of anthropologists face to face with peoples who lived for example in a rainless country, or in trees, or who ate monkeys, or grandfathers, or called themselves by their mothers' names, or did other disconcerting things ; and this set them thinking, and comparing, and collecting more and more data, from trader and traveller, for an answer to perennial problems, alike of their anthropology and of ours. Can climate alter character or change physique, and if so, how ? Does the mode of life or the diet of a people affect that people's real self, or its value for us ? Is the father, as the Greeks believed, or the mother who bore them, the natural owner and guardian of children ? Is the Heracles whom they worship in Thasos the same god as he whose temple is in Tyre ? Because the Colehians wear linen, and practise circumcision, are they to be regarded as colonists of the Egyptians ? or can similar customs spring up independently on the Nile and on the Phasis ? Here, in fact, are all the great problems of modern anthropology, flung out for good and all, as soon as ever human reflective reason found itself face to face with the facts of other human societies, even within so limited a region as the old Mediterranean world.

And I would have you note that these old Greek problems, like all the supreme problems of science old and new, were not theoretical problems merely. Each of them stood in direct relation to life. To take only cases such as I quoted just now from the Father of History—is there, for example, among all the various regions and aspects of the world, any real earthly paradise, any delectable country, where without let or hindrance the good man may lead the good life ? Is there an ideal diet, an ideal social structure, or in general, an ideal way of life for men ; or are all the good things of this world wholly relative to the

persons, the places, and the seasons where they occur? I do not mean that the ancient Greeks ever found out any of these things, for all their searching; or even that all ancient seekers after marvels and travellers' tales were engaged consciously in anthropological research at all. I mean only this: that the experiences, and the problems, and the practical *end* of it all, were as certainly present to the minds of men like Herodotus and Hippocrates, as they have been in all great scientific work that the world had seen.

In the same way it has for some while been clear to me that neither Plato nor Aristotle, the great outstanding figures of fourth-century Greece, was constructing theories of human nature entirely in the air. Their conceptions both of the ideal state of society, and of the elements which were fundamental and essential in actual societies as they knew them, were determined to a very large extent by their observation of real men in Sparta, Persia, or Scythia. But it is also clear that much that had been familiar to the historians of the fifth century, and particularly to Herodotus, had fallen out of vogue with the philosophers of the fourth. Systematic clearness had been attained only by the sacrifice of historic accuracy. Thucydides, in fact, standing right in the parting of the ways between history and rhetoric, might fairly have extended his warnings to a disassociation of history from political philosophy, which was just as imminent.

Anthropology and the Renaissance.

At the Revival of Learning it was the same as in the great days of Greece: New vistas of the world were being opened up by the voyagers; new types of men, of modes of life, of societies and states, were discovered and described; new comparisons were forced upon men by new knowledge crowding thick into their minds; and new questions, which were nevertheless old as the hills, made eddies and rapids in the swift current of thought, and cried out for an answer. Take the central political problems for example: What constitutes the right to govern, and what is the origin of law? In mediæval Europe this was simple enough. The duke, or the king, or the bishop governed by authority of the emperor, or the pope; and pope and emperor ruled (like Edward VII.) 'by the Grace of God.' Yet here, in Guinea, in Monomotapa, in Cathay, and in Peru, were great absolute monarchies which knew nothing of the pope or the emperor, and were mighty hazy about God. Yet their subjects obeyed them, and gave good reasons for their obedience, and chiefest of their reasons (as in all times and places) was this: 'We should be much worse off if we didn't.'

Unsocial Man and the Pre-Social State.

It would take me very far afield if I were to try to show how this universal answer came to change its ground from politics to anthropology, so that to the question—how men knew that they would be much worse off if they didn't—the answer came, that 'once upon a time they *had been* much worse off, *because* they didn't.' For my present purpose it is enough to note that, in all ages, philosophers who set out to define the *nature* of the State, have become involved in speculations about its *origin*; that historians in their researches into its origin, have been forced into conclusions as to its nature; and that in both cases every belief about the Nature of the State has been found to involve a belief about a State of Nature; an answer of some kind, that is, to the question whether man was originally and naturally a social animal, or whether at some early period of his history he became social and domestic. In the latter event, how was domestication effected, and what sort of thing was undomesticated man? In the ancient world, after long controversy, Aristotle's definition of man as the 'social animal' had carried the day, and ruled that question out of court. But at the Revival of Learning, the unnatural behaviour of certain actual societies towards their individual members had revived irresistibly the whole question whether society was part of the natural order at all, and not a 'device of the heathen,' a mistake or a *pis aller*; and whether, if society was not thus 'natural,' men would not really be better off if they returned to their natural, pre-social, *unsocial* state,

and began again at the beginning, to work out their own salvation. This belief in a pre-social state played a large part in the political philosophy of the seventeenth and eighteenth centuries; and conversely it was the very fact that the pre-social state as a philosophical conception fell out of vogue at the beginning of the nineteenth, which has distinguished modern political philosophy so markedly from its predecessors.

Now it is impossible to compare the successive presentations of the pre-social state, without being struck by the widely different content of them. But how was it that the conception of a pre-social state of man, whether conceived as a period of prehistoric development or as the result of a psychological analysis of mankind in society, assumed in different writers such widely different forms, and led—as was only natural—to such widely different proposals for the remedy of actual grievances? Why should Hobbes, for example, describe the life of the natural man as little better than a hell upon earth, ‘no arts, no letters, no society; and (which is worst of all) continual feare, and danger of violent death; and the life of man solitary, poore, nasty, brutish and short’; ‘no property, no dominion, no Mine and Thine distinct, but only that to be every man’s, that he can get; and for so long as he can keep it.’ How comes it that Locke, whatever else he may deny his natural man, at all events reserves to every man, even in his first ‘Treatise on Government,’ property in his own person, and (as a corollary to this) property in the products of his labour, while in his second ‘Treatise’ he contemplates also a natural property in agricultural land? How comes it, again, that Montesquieu bases the whole fabric of civilisation upon the timidity of pre-social man; while for Rousseau it is the utter fearlessness of the savage which most distinguishes him from the craven members of societies? Flat contradictions of this sort, between thinkers who were almost contemporaries, and who agree so closely in the form and system of their reasoning, clearly result not so much from any defect of method as from some discrepancy in the data which the method was employed to explain. The question, therefore, begins to assume another shape: Whence did those political philosophers, whose theories involved a state of nature, get their respective data as to the character of natural man?

It is common knowledge, of course, as I have hinted already, that each thinker’s own view of the nature of society went far to determine his imagination of its origin; and that his view of its nature was itself suggested by the political stresses of his own time. Hobbes, for example, writing in the middle of the Great Rebellion, was searching for a Sovereign, whose mandate should be beyond dispute; Locke, standing in even closer relation to the Revolution of 1688, was explicitly replying to the advocates of a Divine Right of Kings, and insisting that the Contract is revocable; Rousseau, confronted with iniquities which resulted from an antiquated distribution of privilege, is all for equality and fraternity as the necessary guarantees of liberty.

But it is possible also to put the sequence in the reverse order, and to make the inquiry, how far each thinker’s conclusions as to practical politics resulted from his view of the nature of the State; how far his view of its nature is deducible from his beliefs as to its origin; and how far his beliefs as to the origin of society were themselves rendered almost inevitable for him, by the state of contemporary knowledge of the more primitive specimens of mankind and of the State itself.

The ‘Geographic Control’ of the Renaissance.

That such a line of reasoning was not foreign to the political thinkers of the seventeenth and eighteenth centuries is clear from a variety of considerations. In the first place, the whole movement in political philosophy, which is in question, stands, like the political events with which its turning points are so closely connected in point of time and personality, in the closest relation with a larger contemporary movement of scientific inquiry, of which the inquiry into the antecedents of society and of man is only one special, departmental, and relatively late application. And in the larger sphere, also, a general advance of physiographic theory had gone hand-in-hand with active physiographic discovery. Bacon’s enlargement of current ideas of scientific method stands, as

we all know, in the closest historical connection with the discovery of a new world by Columbus, and with the new prospects of exploration within the old world which were opened by Vasco de Gama. It would therefore be natural to expect that Hobbes, for example, should reflect in his 'Leviathan' the current conceptions of what *pre-social* man would be like, as inferred from the behaviour and circumstances of *unsocial* man as reported by contemporary voyagers.

Two great events of this time, in particular, set the study of mankind, no less than all the physical sciences, on a new pinnacle of outlook, and challenged all the theories of the Greeks and Arabians which had done duty at second-hand, to explain the universe, since the great days of Alexandria. First, the discovery of the Cape route to the East threw open to European observation vast tracts of country and an immense number of societies of men whose fame indeed had come down through Pliny and Ptolemy, but whom no one but a few traders and missionaries had visited in person, since the Arab and the Turk tore East and West asunder and began to keep them so. Then, within the same generation, the discovery of America opened up, literally, a new world, wherein (among many marvels) one of the things which impressed its explorers most vividly and constantly was the presence of varieties of men whose mere existence shook Adamite theories of mankind to their foundation; who utterly failed to conform to the traditional requirements of the Flood, and professed inveterate ignorance on that subject; and whose manners and customs—when indeed they seemed to have any—betrayed a culture, or a lack of culture, totally unlike anything which the old world yielded, even taking into account the barbarous *Terra Nigritarum* which lay between the Canaries and India.

Thus almost at one gift three new sets of human documents were presented to the philosophers of Europe: (1) first-hand knowledge of the famous empires and kingdoms of the civilised East, of India, China, and the parts of 'India beyond the Ganges,' as the saying was, beyond the desert belt of Asia; (2) fresh access to the black men, south of the desert belt of Africa; (3) the discovery, beyond the no less desert ocean, of new and Western 'Indies,' peopled by wholly un-Indian tribes, whose aspect was Tartar rather than Indian or Malay, and whose behaviour seemed all the more inexplicable because it differed totally from what was expected so surely by the geographers.

Bodin, 1577.

It was long before this mass of new material could be compared and applied by the philosophers at home; but it was collected and recorded with avidity, and the insatiable demand for books of travel spread it broadcast, and made it sink deep into popular imagination. Still, with all his learning, even Bodin, writing in 1577, 'Of the Lawes and Customes of a Common Wealth,'¹ hardly shows by an allusion that he appreciates the new age that has dawned. There is a wonderful chapter, indeed, at the beginning of his fifth book, which is thus entitled: 'What order and course is to be taken to apply the form of a Common Wealth to the diversitie of men's humors, and the meanes how to discover the nature and disposition of a people.' Its contents show clearly what contribution he hoped to make to the art of statecraft, and also what was to be his method of research, to extract the truth from the mass of conflicting instances. It contains the whole pith and kernel of modern anthropo-geography, and completely anticipates the ethnological work of Montesquieu; but the data upon which it is based are with a single exception such as would have been available before the fall of Constantinople. His climatic contrasts are based on the Ptolemaic geography; he betrays no knowledge of a habitable south-temperate zone, and argues as if the world broke off short at the Sahara. It is only by a curious afterthought, which superposes on his classification of environments from arctic North to tropic South, a cross-division by grades of culture from civil East to barbaric West, that he betrays any hint that his cosmography has been disturbed by the new age of exploration. 'The Spaniards have observed,' he

¹ I quote from the English edition of 1605, 'out of the French and Latin copies done into English by Richard Knowlles, Author of the Turkish History.'

says, 'that the people of Sina (China), the which are farthest Eastward, are the most ingenious and courteous people in the world; and those of Brezill, which are farre Westward, the most cruell and barbarous;' ¹ so that East goes with South, and West with North, and Bodin's cultural equator begins to lie askew between them; and we should note that the crucial instance here supplied by 'those of Brezill' is his single glimpse of Columbian man.

He has, indeed, full grip of the doctrine of a pre-social state, and of the application of inductive proof to support it; but his instances are exclusively derived from classical authors. 'He that would see,' he says, ² 'what force education, lawes, and customes have to change nature, let him look into the people of Germanie, who in the time of Tacitus the Proconsull had neither lawes, religion, knowledge, nor any forme of a Commonweale; whereas now they seeme to exceed other nations in goodlie cities and well peopled; in arms, varieties of arts, and civil discipline.' A curious exception goes far to establish this rule. The only instance which I can recall, in which Bodin refers to an event in Negro-land, is where he illustrates the revolt of the Mombottu Negroes against the Moors in 1526 (p. 555); but this was an event, the news of which certainly reached Europe by way of the Morocco ports, not by way of the southern route, or westward down the Gambia; it was also one which made a great sensation in Europe, and was still a commonplace of cosmographers and moralists a generation later. In illustration of this I quote as follows from Peter Heylin's 'Microcosmus': ³ 'The last Moroccan governor, Soui Halin, was slaine by Ischia, Anno 1526, and the negroes againe recovered their long lost liberty: instituting divers kings, and among others, Ischia was worthily made king of Tombutum. After this advancement, he quickly united many of the weaker kingdoms to his owne, which at this day is the greatest of the foure in whose hands kingly authority remaineth.' This actual example of a 'Leviathan' in process of construction was thus in text-book use in 1577, a generation before the time of Hobbes.

Shakespeare's Caliban.

The trend of popular opinion at the end of the sixteenth century, as to the characteristics of the state of nature, could hardly be better illustrated than by the Shakespearean conception of Caliban, 'solitary, nasty, and brutish'; barely human, in fact, but for his vices; living 'like a bear' (as Montesquieu so often puts it), grubbing roots, and plundering bees' nests; a prey to panic, haunted by the spirit of the power of the air, and instinctively appeasing him, as savages do, by abstinence, abasement, and offerings. Mr. Hartland has only lately called attention again to the truth of detail with which Caliban is portrayed, and Mr. Sidney Lee has gone at some length into the question of his probable originals. No doubt there is in Caliban a touch of the gorilla pure and simple: and a touch of the gorilla's own brother, the 'Salvage Man' of heraldry and mediæval legend; Linneüs and Blumenbach, in fact, quote several examples of such 'wild men of the woods' who had been captured in various parts of Europe, and described in books before Shakespeare's time. But apart from his make-up—which, in the Globe Theatre (as at Her Majesty's), was mainly to tickle the gallery—Caliban is certainly neither ape nor idiot. He has his own code of conduct (when he can bring himself to conform to it); he knows when he has done wrong; and in his treatment of his invaders, of his small belongings, and in particular of his island property, he corresponds too closely with the current sixteenth century descriptions of the feckless, passionate 'child of nature' to be set down as anything else but an experiment in the portrayal of natural man. And if we once view Caliban from this standpoint, it becomes almost incredible that he should have preceded Hobbes' sketch of the state of nature by nearly half a century, unless Hobbes' portrait itself was based upon a type already widely current, and generally accepted in popular belief.

¹ *Loc. cit.* English Ed., 1605, p. 562.

² *Ibid.* p. 565.

³ I quote the Oxford edition of 1636, p. 722.

Edward Grimstone, 1615.

I come now to a work of which I would gladly have further information. It is entitled 'The Estates, Empires, and Principallities of the World'; it was published in London in 1615, and it is described as having been 'translated out of the French by Edward Grimstone,' doubtless the translator of Joseph Acosta (1604) and Jean François Le Petit (1608). I introduce this work here for three reasons. It contains a fuller application of what I shall best summarise as Baconian methods to political science, than is easily to be found elsewhere. It shows very clearly that by this time the new discoveries were already being applied systematically to philosophical ends. And it illustrates a remarkable series of coincidences of discovery which in less than a generation were to have a profound effect on European thought.

The treatise consists of a collection of studies of human societies—*συννηγμένα πολιτεία*, as Aristotle used to call them—which professes to be complete. Its title-page, engraved by 'Ren. Elstracke,' is of a cosmographic type which descends, for example, into the title-page of Heylin's 'Microcosmus' a generation later; but which is seen here in its pristine glory. Four female figures, emblematic of Europe, Asia, Africa, and America, advance to do homage to James I., who sits enthroned, as he sits on Bodley's Tower in Oxford; and below are four posed warriors, in the weapons of their countries. America is represented by an obvious Aztec warrior in a peaked cap and coat of mail; but of the four women, America alone is nude: even Africa is partially draped in a mantle. The distinction is significant, for though Europe, Asia, and Africa all contribute to the contents of the book, America provides no example of a constitution at all: if it had any human inhabitants, they were, for Edward Grimstone, in a 'pre-social state.'

A few examples will illustrate sufficiently Grimstone's style and method, his attitude towards the new and the older learning, and his obvious debt to Bodin and to contemporary geographers. His preface censures alike the mere complacent patriots 'so farre in love with themselves as they esteeme nothing else, and think that whatsoever fortune hath set without the compasse of their power and government, should also be banished from their knowledge'; and the mere politicians who 'remain so tied to the consideration of their owne Commonweale as they affect nothing else, carrying themselves as parties of that imperfect bodie, whereas in their curiositie they should behave themselves as members of this world.' 'But there are others,' he goes on—and here his lash falls on the rigidly classical humanists of his own day—'which lie grovelling in the dust of their studies, searching out with the sciences the actions and manners of the Ancient, not respecting the Moderne, and they seeme so to admire the dead, as they have no care for the living.' What the classicists lack, he goes on to explain, is the 'Science or knowledge of the World,' a good part of which knowledge 'is comprehended in the discourse of this book.' And so 'although my chief desseigne was to deal onely with politicke and civile matters, yet to the end they might find all together, and not be forced to seeke for the description of countries whose custome I represent, I have made the corographie,' which in the next generation Peter Heylin defines as the 'exact description of some Kingdom, Countrie, or particular Province of the same.' But after describing thus 'all that the countrie yeields and the beasts that naturally live there and have their breeding,' he adds 'yet all this were little . . . if I should not show you the man which dwells in evere countrie, and for whom all those things seem to have been made, first in his ancient posture, and with his old customes, either altogether or for the most part abolished, then in his modern habit . . . to the end that every man may judge which is the better of the two Estates, and make use of part of the one and part of the other, having carefully ballanced the most considerable particularities of both.' He then explains that he must take account of their economics, their means of self-defence, and their religion, 'whereof I have discoursed, to show that it is the feare of some divinitie which maintaines people in their duties, makes them obedient to their princes, and diverts them much more from all bad desseignes than armes and souldiers which environ and

threaten them. I do it also to show that whereas religion wants, of what sort soever it be, policie and order faile in like manner, and barbarisme, confusion, and rebellion, reign there in a manner continually, whereas they that seise on them should presently settle in their rude minds the apprehension of some power over all to dispose of things at pleasure.'

Here there is certainly a remarkable anticipation of a well-known passage of the 'Leviathan'; only the point of view is different, and the cynicism of Hobbes is well away.

Grimstone was well aware that he stood at the opening of a new period of discovery. 'I protest with trueth that if I have given any ranke or commendation to this worke, I will give much more to those that shall labour to make it perfect, and that any man may adde something dayly unto it, for that from time to time they have more certaine advice from all parts, especially from those countries which have not been much frequented, either by reason of the distance, or for their barbarousnesse.' For his own part, however, he had clearly done his best with the materials which he had. The 'Order of all the Estates contained within this booke' includes (besides all European states) 'the kingdomes of Tartary, China, Japan, Pegu, the Great Mogul, Calicut, Narsinge, and Persia; the Turkes Estate in Europe, Africke and Asia (including the ancient kingdomes of Egypt, Judaea, Arabia, &c.), the empire of Presbiter John, the Estate of the King of Monomotapa, the realme of Congo, and the Empire of Moroccco'; and consequently was very fairly abreast of the travels and compilations of the day. His frank confession, therefore, that he knows only this, and wishes to know more, coupled with his total neglect of America, suggests that there may be real significance in the nude American on his title page; and that America was not regarded as offering any regular constitutions.

Now it is certainly remarkable that, with the exception of a few European republics, all the 'Estates, Empires, and Principallities of the World,' which the author thinks worth describing, and in particular all the non-European states, are personal monarchies of more or less absolute type: and this from a man who is expressly throwing classical and mediæval experience to the winds, and setting out to describe men as he finds them.

Peter Heylin and the Cosmographers.

Nor is this peculiarity confined to Grimstone's treatise. The standard English cosmography of the early seventeenth century is that of Peter Heylin, the learned, witty, and pugnacious chaplain of Archbishop Laud. Its method of treatment is closely modelled upon that of Grimstone; the sequence of topics is the same, and there is a good deal of matter common to the two, though Heylin of course is far more encyclopædic in his treatment, and includes many regions and 'estates' which do not occur in Grimstone. Here, too, with hardly an exception, the constitutions which are described are despotic: and as in Grimstone, particular attention is given to the brutal kingships of Western and Southern Africa. Almost the only exceptions are the cases where the royal power is not yet fully established, and others in which, to the best of Heylin's knowledge, there is no settled form of government.

In fact, if an unprejudiced inquirer were to attempt, with only the materials available in Heylin's time, to generalise as to the political evolution of the Old World outside Europe, I do not see how he could fail to arrive at the conclusion, *first*, that the natural and primitive state of man was, in the words of Hobbes, 'poor, nasty, and brutish; in continual feare, and danger of violent death'; and *secondly*, that wherever man had emerged from this primitive condition it had been by submission, more or less voluntary, and more or less by way of a *pis aller*, to an absolute despotism, usually exercised by a single imperial master who, like Ischia of Tombutum, had superseded by common consent a number of smaller despots.

Thomas Hobbes.

Hobbes himself does not often make mention of ethnographic matters. His outlook is, of course, primarily political, and his analysis, so far as it is not political,

is psychological. Moreover, he is reticent throughout as to his sources. Now and then, however, he does lift the veil, and betrays an interest in the reports of travellers, and even a certain dependence on them.

On the vexed question of the 'naturalness' of patriarchal rule, on which Hobbes differs as violently as usual from the current Aristotelianism, his general attitude, though not positively that of an anthropologist, is at all events in agreement with the contemporary trend of observation. 'When the parents are in the State of Nature,' he says, 'the dominion there over the child should belong equally to both; and he be equally subject to both; which is impossible, for no man can obey two Masters.' In civilised states, he goes on, the law decides whether the father's claim or the mother's shall prevail; 'but the question lyeth now in the state of mere nature; where there are supposed no lawes of matrimony; no lawes for the education of children; but the Law of Nature, and the natural inclination of the Sexes one to another, and to their children.' 'If there be no contract,' he adds, 'the dominion is in the mother,' and this for the same obvious reason as Heylin had given already for female sovereignty in Borneo.¹

It may be admitted at once that Hobbes' normal attitude of opposition to the Aristotelian tradition is such that the mere fact that Aristotle had laid down that 'the father is naturally in authority over the sons' may be held sufficient reason why Hobbes should decide for the Matriarchate. But it is certainly an instructive coincidence—and for my own part I am inclined to regard it as more—that the first great groups of patriarchal folk to be studied in any detail were precisely in areas now being thrown open by the discoverers: Southern India, Negro Africa, and North America; so that, at this period, patriarchal institutions, which had so long been treated as evidence of human depravity, or, at best, as curiosities and antiquities, were being rehabilitated for the first time in European thought as a practical scheme of society. Heylin had even generalised already that female kingships were correlated with tropical climate.² Once more the circumstances of the age and the general progress of knowledge were forcing on the notice of the philosophers fresh phenomena of a kind which precisely fitted the demands of the philosophic situation.

Most important of all, however, is the direct appeal of Hobbes to the evidence of discovery, when he is dealing with the State of Nature itself. 'It may peradventure be thought,' he says,³ 'there are never such a time nor condition of warre as this, and I beliebe it was never generally so, over all the world; but *there are many places where they live so now*. For the savage people in many places of America, except the government of small families, the concord whereof dependeth on natural lust, have no government at all, and live at this day in that brutish manner, as I said before. However, it may be perceived what manner of life there would be, if there were no common Power to fear, by the manner of life which men that have formerly lived under a peaceful government use to degenerate in a civil War.' Here, clearly, we have Hobbes the psychologist and politician supplementing his psychological and political evidence from a totally different quarter, and in particular quoting America as the last citadel of pre-social man.

To refer all Governments, as he explicitly does refer them, to the standard of Peru or Monomotapa; to imagine the State as a 'Leviathan,' a nightmare, a Frankenstein's monster, tolerable only because without it the life of man had been, and would be again, 'solitary, poore, nasty, brutish, and short,' was indeed but a partial inference from the life of 'natural man,' as it might have been constructed from evidence which was available even then. But it accords so closely with the accidents of contemporary discoveries, and with an actual tone of pitiful contempt which had come in fashion among the voyagers themselves, as to force the conclusion that Hobbes was really doing his best to state what nowadays we should call the 'most recent conclusions of anthropologists' on a

¹ Heylin, *Micocosmus*, Oxford, 1636, p. 830.

² *Ibid.*

³ Hobbes, *Leviathan*, ch. 13.

matter of practical concern, and that political science owes more than is commonly supposed to this attempt to define and interpret large new facts of human nature as the Age of Discoveries revealed them.

John Locke.

In the next generation the connection between physics and politics is even more strongly marked. Closely as Locke was allied, in his political aspect, to the leaders of the English Revolution, he is still more closely associated with the first administrators of the Royal Society, and that in more than one department. His 'Elements of Natural Philosophy' remain to show how near he stands to Newton and the physicists; his medical studies kept him in close touch with the chemists and anatomists, and gave him a rational psychology; and we shall see how intimately his psychological analysis is concerned with his general anthropology. On the other hand, his interest in exploration and travel was keen and continuous. It peeps out in his 'Two Treatises on Government'; it is evident in his 'Essay on the Conduct of the Human Understanding'; it is confessed in a striking passage of his 'Thoughts concerning Reading and Study for a Gentleman'; and it bears remarkable fruit in his Introduction to Churchill's 'Collection of Voyages,' published in 1704, which shows him thoroughly acquainted with a wide range of the writers best qualified to inform him of the recent discoveries in regard to unsophisticated man.

Thus the case of John Locke is rather clearer than that of Hobbes. Here, too, though what impresses at the outset is the dependence of his political theory upon the political needs of his time, yet side by side with this we have the same intimate connection between his politics and his psychology as is obvious in the case of Hobbes, and it is naturally therefore to his psychology that I turn first for indications of his method of work. And we have not to go far into the 'Essay concerning Human Understanding' before we have a good example of what I mean. In the third chapter he is following up his contention that there are no 'innate principles' in the mind by an argument to the same effect as regards moral, or, as he calls them, 'practical,' principles. Virtue is generally approved, he says, not because it is innate, but because it is profitable; nor do men's actions betray any such 'internal veneration of these rules.' Even conscience, which is usually represented as checking us for our breaches of them, cannot be distinguished, in the mode of its origin, from any other kind of human knowledge, and that in many cases it is 'from their education, company, and customs of their country' that men are persuaded that morals are binding on them; 'which persuasion, however got, will serve to set conscience at work.' Then comes the passage which concerns us now. 'But I cannot see how any men should ever transgress these moral rules, with confidence and serenity, were they innate and stamped upon their minds. Have there not been whole nations, and those of the most civilised people, amongst whom the exposing of their children, and leaving them in the fields to perish by want or wild beasts, has been the practice, as little condemned or scrupled as the begetting them?' Then follows a list, a couple of pages long, of barbarities practised by the Mingrelians of the Caucasus; the natives of the interior of Africa; the Caribbees of the Orinoco; a people in Peru (who fattened and ate the children of their female captives); and many others. Among the Tououpinambos, another American tribe, 'the virtues whereby they believed they merited Paradise were revenge and eating abundance of enemies; they have not so much as a name for God, and have no religion, no worship.' Among the Turks 'the saints who are canonised lead lives which one cannot with modesty relate.' 'He that will carefully peruse the history of mankind,' he concludes, 'and look abroad into the several tribes of men, and with indifference survey their actions, will be able to satisfy himself that there is scarce that principle of morality to be named, or rule of virtue to be thought on (those only excepted that are absolutely necessary to hold society together, which commonly, too, are neglected betwixt distinct societies), which is not, somewhere or other, slighted and condemned by the

general fashion of whole societies of men, governed by practical opinions and rules of living quite opposite to others.'

Here, clearly, Locke claims to support, if not to found, his generalisation as to the nature of the human mind on a comparison of specific varieties of human behaviour. At the same time he makes definite exception of those principles which, as he says, 'are absolutely necessary to hold society together,' and these he is apparently inclined to regard either as actually innate or at all events as a higher order of universality than the ordinary principles of morals. It is the beginning of a deep distinction in anthropological theory, which bears fruit, long after, in Bastian's distinction between Universal and Racial Ideas.¹

There are other passages in the 'Essay' in which the same argument is used, drawn from observation of actual savages. In Chapter IV., for example, he gives a long list of tribes whose members are devoid of the idea of God. 'Besides the atheists taken notice of among the ancients, and left branded upon the records of history, hath not navigation discovered, in these later ages, whole nations at the Bay of Soldania (in South Africa), in Brazil, in Boranday, and in the Caribbee Islands, &c., amongst whom there was to be found no mention of a God, no religion?' He goes on to quote further evidence as to the Caiaquas of Paraguay, the 'Siamites' (which 'will I doubt not be a surprise to others, as it was to me'), and the Chinese. His authorities in this passage are ample: Sir Thomas Roe, the hard-headed English ambassador to the Great Mogul, and his French editor, Thévenot; de Choisy, for Siam; La Loubère, for Siam and China; Navarette and the Jesuit Relations, for China; Ovington, for Surat; Martinière, de Léry, and Nicholas del Techo. For South Africa, of course, he quotes Terry, and through Terry, the educated Hottentot *Coore* or *Courwee*, who came to England for a time, and of whom Heylin, too, has a quaint story to tell. And these are no mere gleanings from other people's fields. Few of Locke's contemporaries had a better right to an opinion in the department of knowledge which now we should call anthropology, and which formed already a principal department of geography. And he had the highest opinion of its importance, for in his 'Thoughts concerning Reading and Study for a Gentleman' he recommends a list of original books of travel which occupies more than a page. His own reading was enormous, and set him wholly free of compendia like those of Heylin and Moll, which indeed he could compare and criticise as an expert. By a comparison of the libraries of Christ Church, of the Bodleian, and of the Royal Society, it is easy to verify the general conclusion that if the English gentleman, as Locke feared, did not think it worth while to bestow much pains on geography, it was not for want of available books or of examples of distinguished publicists who were also good geographers. And this is of some importance to my general thesis, for it shows that in Locke's time still, as in the days of Hobbes and before, inductive anthropology and inductive politics were greatly in the air and were being studied together; and consequently that political philosopher, no less than a psychologist, was addressing a public which knew about savages and expected a thinker to take account of them.

It is time now to turn to the 'Two Treatises on Government.' Their form was, of course, mainly dictated by that of Sir Robert Filmer's 'Patriarcha, or the Natural Power of Kings,' in which the patriarchal theory of society, maintained with a thoroughness which would have delighted Aristotle, anticipates almost verbally the orthodox criticism which was levelled two centuries later at MacLennan and Lewis Morgan. Filmer's attitude in fact is exactly that of the Aristotelian and classicist thinkers castigated by Edward Grimstone. He can quote Athens, Sparta, Rome, and the Jewish patriarchs; he is learned about Nimrod and Codrus; but from beginning to end he writes as if America and the Cape route to India were still unknown. Locke has arguments enough, of a more relevant kind, to bring against Filmer, and makes no direct comment upon the narrowness of his experience of mankind; but implicitly his reply is precisely in that form. It is an appeal to experience against authority; to modern discovery in the new worlds beyond the oceans, against traditional

accounts of ancient societies in the Mediterranean and the Semitic East. To refute Filmer's claim that Patriarchal rule is natural, he recalls the systematic fattening and eating of children by the Peruvians,¹ and quotes a long passage from de la Vega's 'History of the Yncas.' On the question of the authority of the law over an alien, the 'Indian' is his typical example. 'The legislative authority by which they are in force over the subjects of the commonwealth hath no power over him. Those who have the supreme power of making laws in England, France, or Holland, are, to an Indian, but like the rest of the world—men without authority.'²

Locke himself, indeed, was before long to be confronted with this question in a very practical shape; for it was he who was deputed to draw up a constitution for the new settlement of Carolina, the first British settlement which came into direct contact with communities of agricultural Redskins of the Muscogean stock, and consequently one of the first to be confronted with any worse problems of expropriation than those which had been described by Heylin.³

In the very next section⁴ he is confronted with another question of natural law on which the experience of the colonists was modifying opinion profoundly. 'It is not every compact that puts an end to the state of Nature between men, but only this one of agreeing together mutually to enter into one community and make one body politic: other promises and compacts men may make with one another, and yet still be in the state of Nature. The promises and bargains for truck, &c., between the two men in Soldania, or between a Suris and an Indian in the woods of America are binding to them though they are perfectly in a state of Nature in reference to one another; for truth and keeping of faith belongs to men as men, and not as members of society.' Here we have a clear anticipation of Montesquieu's position:⁵ 'The law of Nature is naturally founded upon this principle, that the various nations ought to do one another as much good as possible in peace, and as little harm as possible in war, without damage to their true interests. . . . All nations have a law of nations. Even the Iroquois, who eat their prisoners, have one. They send and receive embassies; they recognise laws of war and laws of peace. The only trouble is that this law of nations is not founded on the right principles.' Montesquieu, it will be observed, recurs here, like Locke, to the 'Indian in the woods of America'; and we shall see presently that there is a historical reason for this prominence of the Redskin in such a context.

One of Locke's main advances upon the position taken up by Hobbes is in his treatment of the Right of Property." 'Though the earth and all inferior

¹ Ch. I. 57.

² Ch. II. 13.

³ Heylin, *Microcosmus*, Oxford, 1636, *An advertisement to the reader concerning America in general*. 'He that travelleth in any Part of America not inhabited by the Europeans shall find a world very like to that we lived in, in or near the times of Abraham the Patriarch about three hundred years after the flood. The lands lie in common to the Natives and all Comers, though some few small parcels are sown, yet the Tiller claims no right in them when he has reaped his crop once. Their Petty Kings do indeed frequently sell their kingdoms, but that in effect is only the taking Money for withdrawing and going further up the Country, for he is sure never to want land for his subjects because the country is vastly bigger than the Inhabitants, who are very few in proportion to its greatness and fertility. . . . Sometimes whole Nations change their Seats, and go at once to very distant places, Hunting as they go for a Substance, and they that have come after the first discoverers have found those places desolate which the other found full of inhabitants. This will show that we have done them no Injury by settling amongst them; we rather than they being the prime Occupants, and they only Sojourners in the land: we have bought however of them the most part of the lands we have, and have purchased little with our Swords, but when they have made war upon us.'

⁴ II. 14.

⁵ *Esprit des Loix*, l. iii.

⁶ Ch. V. 27. Though the 'Two Treatises on Government' were published simultaneously in 1690, it must be remembered that the first of them was written in reply to Filmer's tract of 1680, and bears evident marks of earlier composition. It was indeed already out of date in 1690; but for our present purpose it is this very

creatures be common to all men, yet every man has a property in his own person. This nobody has any right to but himself. The labour of his body and the work of his hands we may say are properly his. . . . The fruit or venison which nourishes the wild Indian, who knows no enclosure, and is still a tenant in common, must be his ; and so his—i.e. a part of him—that another can no longer have any right to it before it can do him any good for the support of his life.’ Here Locke’s ethnological position becomes clearer still. He is familiar with the hunting and berry-eating Redskin of the New England forests ; but he is not yet brought into contact with the agricultural communities of the south-east ; and still less is he aware of the paradoxical behaviour of the later-discovered Indians of the Chaco, where precisely that observance holds of which he denies the existence—namely, that the actual hunter has no recognised right to his game, and sits out, hungry and patient, until the whole of the clan has had its fill. Locke proceeds accordingly : ‘ Thus this law of reason makes the deer that Indian’s who hath killed it. It is allowed to be his goods who hath bestowed his labour upon it, though before it was the common right of everyone.’

His estimate of the agricultural skill of his ‘ Indians ’ was a low one.³ ‘ An acre of land that bears here twenty bushels of wheat, and another in America, which with the same husbandry would do the like, are without doubt of the same natural intrinsic value. But yet the benefit mankind receives from one in a year is worth 5*l.*, and the other possibly not worth a penny : if all the profit an Indian received from it were to be valued and sold here, at least, I may say truly, not one thousandth.’ Here again his experience does not extend yet to the agricultural communities of Carolina and Georgia ; it is the rude husbandry of the Iroquois and Algonquins that is typical, for him, of the natural state of man. More generally still, when he speaks of the function and use of money,⁴ he asserts : ‘ Thus in the beginning, all the world was America, and more so than that is now ; for no such thing as money was anywhere known.’

His views on the natural estate of matrimony are coloured again from the same source. ‘ All the ends of marriage being to be obtained under politic government, as well as in the state of Nature, the civil magistrate doth not abridge the right or power of either [parent] naturally necessary to those ends ’ ; a reflection once more of the many curious compromises between patriarchal and matriarchal government in American societies, and particularly among the peoples who had partially adopted agriculture—namely, the Southern Iroquois and the Eastern Sioux of Virginia. America, as we see from the extract on money, though it is still near the state of Nature, has in some parts advanced beyond it ; but it is still to America that he turns for examples of more purely natural conditions : ‘ If Josephus Acosta’s word may be taken, he tells us that in many parts of America there was no government at all.’⁵ ‘ There are great and apparent conjectures,’ says he, ‘ that these men [in Peru] for a long time had neither kings nor commonwealths, but lived in troops, as they do this day in Florida—the Cheriquanas, those of Brazil, and many other nations, which have no certain kings, but as occasion is offered in peace or war, they choose their captains as they please.’⁶ ‘ I will not deny,’ he goes on,⁷ ‘ that if we look back, as far as history will direct us’—he might well have added, as far as ethnology is any guide—‘ towards the original of commonwealths, we shall generally find them under the government and administration of one man. . . . Conformable hereunto, we find the people of America, who (living out of the reach of the conquering swords and spreading domination of the two great empires of Peru and Mexico) enjoyed their own natural freedom [to elect a monarch], though *ceteris paribus* they commonly prefer the heir of their deceased king ; yet, if they find him any way weak and incapable, they pass

circumstance which gives it value as evidence for the growth of Locke’s knowledge and thought.

¹ § 30.

² § 43.

³ § 49.

⁴ § 102.

⁵ § 102.

⁶ Again he is quoting Acosta, *Natural and Moral History of the East and West Indies*, 1604, I. 25.

⁷ § 105.

him by and set up the stoutest and bravest man for their ruler.' Once more America supplies the typical instance, and (once more) that part of America which best satisfies Locke's description is among the hunting tribes of the Southern Algonquins, with their elective war-path chiefs, and regular deposition of the war-lord as soon as his physical force abates. And once more the comparative argument is pressed home, with a hypothesis of the graduation of culture from East to West, almost in the manner of Bodin or Thucydides: 'Thus we see that the kings of the Indians, in America, *which is still a pattern of the first ages in Asia and Europe*, whilst the inhabitants were too few for the country, and want of people and money gave no temptation to enlarge their possession of land, or contest for wider extent of ground, are little more than generals of their armies; and though they command absolutely in war, yet at home, and in time of peace, they exercise very little dominion, and have but a very moderate sovereignty; the resolutions of peace and war being ordinarily either in the people or in a council, though the war itself, which admits not of pluralities of governors, naturally devolves the command into the king's sole authority.'¹ Here, at all events, is a quite unmistakable sketch of the characteristic diarchies of the warlike tribes on the Appalachian chain and its Atlantic slope—Creeks, Cherokees, and the like: a type of constitution quite limited in geographical range, and exactly representing in its distribution the outskirts of European knowledge in Locke's day.

Robinson Crusoe

I made use of Caliban as a popular anticipation of Hobbes: as a sequel to Locke I cannot do better than refer to the savages in 'Robinson Crusoe,' and particularly to Man Friday. This again is a composite portrait, the predominant features of which come from the piratical Caribs of the Brazilian coast, with their dug-out canoes, their simple weapons, their inveterate cannibalism. This Carib type represents quite a different line of observation from Locke's mainly Redskin evidence, and the novelty is the more important, since as at the next turn of the wheel Rousseau makes just as free with this very word 'Carib,' as Locke had done with his 'Indian in the forest,' or as Montesquieu was about to do with his 'Iroquois.'

So far as any other element besides Carib is recognisable in the savages of Defoe—and the portrait, as I have said, is clearly a composite one—it is another eighteenth-century type, the 'South Sea Islanders,' first popularised in England immediately before the appearance of 'Robinson Crusoe,' by the discoveries of William Dampier,² which were at the same time of great geographical importance, admirably described, and very widely read. They figure repeatedly, for example, in the footnotes of Montesquieu.

But the point in which Defoe's savages date his book and affect our present subject most clearly is in the psychology of Man Friday. In particular the dialogues between Crusoe and his man on such subjects as the existence of God, and other test questions of the day, are full of learning, and of ingenious, if partly humorous, parody of current psychology and of the state of Nature. But to develop this subject in detail would require a whole essay to itself.

French Canada: Sagard and Lufitau.

On French thought, meanwhile, as on English, the natives of North America had a very definite influence in the seventeenth century, though not quite in the same way as in England; for the natives whom the French encountered on the St. Lawrence were of a different stock, lived in a different latitude and climate, and enjoyed a very different culture. The French colonists also had come with different predispositions, and were struck by different characters in the order of

¹ § 108.

² Capt. William Dampier, *A New Voyage round the World, describing particularly the Isthmus of America*, 1697. It will be remembered that *Robinson Crusoe* appeared in 1719.

things which they invaded. Here, as elsewhere, a foremost place must be given to the Jesuit reports; full and graphic records of native life and custom, which were widely read in France, as elsewhere, and have hardly been superseded even now. Another book which became classical was that of Gabriel Sagard,¹ which was well known to Locke, and is recommended by him, and was certainly a remarkable study of a barbarous people.

The full tide, however, of what I may call the Huron and Iroquois mythology does not come till the beginning of the next century. Another Jesuit missionary, Joseph Lafitau, produced, in 1724, a large work entitled 'The Manners of the American Savages, compared with the manners of the First Ages.'² Lafitau had only been five years in Canada himself; but he had the acquaintance of Julien Garnier, who had been in the mission field for sixty years, and spoke Algonquin, Huron, and all the five dialects of Iroquois. Lafitau's personal experience was mainly among the Iroquois; he did not, however, confine himself to the Redskins of French Canada; he ranged as far as the Eskimo and the Peruvians, and put together an immense amount of information. For all his protestations to the contrary, Lafitau starts with a theory. 'I have not been satisfied to understand the character of the savages, and to make myself acquainted with their customs and practices. I have searched among these customs and these practices for traces of the most distant antiquity; I have read with care those of the most ancient writers who have treated of the manners, laws, and usages of the peoples with whom they had some acquaintance; I have compared these manners with one another, and I confess that while the ancient writers have given me lights on which to base some lucky guesses concerning the savages, the customs of the savages have given me light to understand more easily, and to explain many things which are in the ancient authors.' He regards the Odyssey, for example, as a collection of sketches of primitive peoples, strung together on the thread of an interrupted voyage from Troy, but having as their object to recommend the study of ethnology. Manners, moreover, are to be studied to form—perhaps even to reform—manners, and also to reform people's ideas. 'I have seen,' for example, he says, 'with extreme pain, in the majority of the *Relations*, that those who have written of the manners of barbarous nations have depicted them as people who have no religious feelings, no knowledge of God, no object of worship; as people who have neither laws nor administration nor forms of government; in a word, as men who have little human about them except their faces. . . . I know' (he goes on) 'that in these latter days people have wanted to shake the proof of the unanimous agreement of the nations to recognise a Deity, as if this unanimous agreement could possibly be a mistake. But the sophisms and subtleties of some individual who has no religion, or whose religion is highly suspect, cannot shatter a truth which has been recognised by the Pagans themselves, which has been received from all time without contradiction, and which we can assume as an axiom.'

Having said that it is an axiom, Lafitau proceeds rather inconsistently to declare it his task to *prove* this unanimity of opinion among all nations, by showing that there is in fact no one so barbarous as not to have a religion and not to have morals. 'And I flatter myself that I make the matter so obvious that no one can doubt it, unless he wishes to be blind in the midst of light.'³ He has a long chapter, also, on their form of government, again with one eye upon Locke. 'Of all the forms of government, that which has seemed to me most curious is that of the Hurons and the Iroquois, because it is most like that of the ancient Cretans and Lacedemonians, who had themselves preserved the longest the laws and usages which they received from the first ages of the world. Though this oligarchic form of government is peculiar to them, the manner of dealing with business is pretty general in all the states of barbarous nations; the nature of the business almost the same, as well as their public assemblies, their feasts and their

¹ Gabriel Sagard, *Grand Voyage au pays des Hurons*. Paris, 1632.

² Joseph Lafitau, *Mœurs des Sauvages Américains comparées aux Mœurs des premiers Temps*. 2 vols. Paris, 1724.

³ Lafitau, i. p. 20.

dances.' His conviction that human nature is the same all the world over comes out again later on.' 'The time which I spent among the Iroquois has tempted me to describe their manners in greater detail, because I know them better and am more confident of what I assert. Nevertheless one may say that the manners of the natives in general are pretty much alike.'

We are here already in the middle of a reaction, on the one hand, against Locke's disproof of innate ideas, and, on the other, against the belief that the savages of the New World represent, in any essential, a lower stage of culture than is to be traced in survivals in classical antiquity. In fact, we are on the straight road to the noble savage as we get him in Pope's 'Essay on Man' (1733), which uses Laftau freely. But we are also very much further still on the road to a synthetic ethnology. Locke had pointed the way, in his Thucydidean comparison of the modern Indian kings to the 'most ancient kings of Europe,' by which, presumably, he meant the Homeric Monarchy. When, therefore, the first curiosity and wonder began to subside, and the real similarity in the performances of human reason under similar circumstances began to be perceived, the foundations began to be laid for a fresh statement of the characteristics of non-social man. Whether the synthesis was to have a psychological or historical content was still a matter of uncertainty; but, in spite of all his eccentricities, I think we may count Laftau as a pioneer of a new line of work. This at least he had of the pioneer; his book succeeded and was much talked of; he certainly influenced Pope and his English contemporaries, and in France he prepared the way for the decisive intervention of Montesquieu.

Montesquieu.

It is easy to examine in similar detail the sources for the ethnology of Montesquieu, who had of course a very wide range of reading, and evidently made good use of his English acquaintances, and his connection with the Royal Society, to keep himself well posted in current English exploration. He quotes Dampier and the 'Lettres Edifiantes' repeatedly; together with Hyde's 'Persia,' Chardin's 'Persia,' Pyrard's 'Turkey,' Bernier's 'Kashmir,' Perry's 'Russia,' Smith's 'Guinea,' Kaempfer's 'Japan,' and a number of other explorers; and he has the immense merit that he rises altogether superior to the current cant about Caribs and Hurons. I doubt whether either name occurs more than once or twice throughout the 'Esprit des Lois.' Montesquieu also goes far more nearly back to the geographical standpoint of Bodin than any of his predecessors or contemporaries.² If he does not, in fact, take rank as one of the founders of synthetic ethnology, it is because, like his great predecessor, he was inclined to overrate the influence of physical environment, and to neglect the human factor of racial momentum. But it is still for the future to show whether it is Montesquieu or the ethnologists who are in the right.

'Man, as a physical being, is governed' for Montesquieu 'like other material bodies, by invariable laws. As a rational being he is constantly breaking the laws which God has established, and changing those which he establishes himself.' He is made, that is, for a life in society. 'But before all these laws are those of nature, so called because they are derived solely from the constitution of our being. To understand them rightly we must consider what man was before the establishment of societies. The laws of nature will be those which he would obey in such a condition. Such a man would at first only be sensible of his weakness. His timidity would be extreme, and if we need experience of that, there have actually been found 'wild men' in the forests: they are afraid of, and run away from, everything. In this condition, each one feels his own inferiority; at best, if at all, he feels himself an equal. He would never therefore attempt to attack, and peace would be the first law of nature.' At this point Montesquieu quotes 'Wild Peter,' to whom we must return before long, as a recent and notorious example of this kind of natural man. From this standpoint, he goes on to attack Hobbes' idea of a natural man, aggressive and domineering, and

¹ Laftau, i. p. 25.

² See particularly Book XIV. *Of Laws in their relation with the nature of the Climate*, where his geographical learning is most displayed.

concludes that, just as fear drives men to fly, so signs of *mutual* fear would soon tempt them to draw nearer; not to mention the natural pleasure which any animal takes in the society of its kind. His four 'laws of nature,' therefore, are (1) the sense of weakness; (2) the sense of hunger and desire to satisfy it; (3) the sense of mutual support; (4) the natural need of society in the sense of mere acquaintance. This last alone is purely human.

It will be seen at once that three of these are concerned merely with the maintenance of an animal life, and that so far, Montesquieu is arguing on the lines of a purely zoological psychology. It will also be clear that in the fourth 'law of nature' he is either begging the question that man is a social animal, or else he is appealing to experience of actual human societies.

Montesquieu does not leave us long in doubt which is to be his line of argument. In the very next chapter he argues that, 'as soon as men are in association they lose the feeling of weakness; the equality which existed between them ceases, and the state of war begins. Each separate society comes to feel its strength, and this produces a state of war of nation against nation.' For there must be different peoples. This last point, however, he does not attempt to prove.

Therefore there arise laws, in the relations in which these nations stand to one another; and these are the 'Law of Nations'—the *Jus Gentium*. 'All peoples have a law of nations. *Even the Iroquois*, who eat their prisoners, have one. They send and accept embassies, they recognise laws of war and laws of peace. The only trouble is that *this* law of nations is not founded on the right principles.'

Here then, as was by this time inevitable for a Frenchman, Montesquieu is once more face to face with the Iroquois. Their 'law of nations,' it is true, 'is not founded on the right principles'; but a law of nature they have got; and this is his proof that there is a law of nature. But clearly he only proves this if we are to assume that the Iroquois are in the state of nature; or at any rate so near to it as to be a fair sample of what human behaviour would be, untrammelled by any positive or non-natural law.

Montesquieu, therefore, like his predecessors, not only takes full account of recorded observations of barbarous peoples, but is directly and specifically guided in his argument by the 'last new thing' in current anthropology, the Iroquois of French Canada, as revealed by Lafitau in 1724.

Rousseau.

Rousseau, I need hardly say, remains something of a puzzle. Like his predecessors, he comes at the subject of the State of Nature, in the first instance, as a reformer and a political philosopher; and I am bound to say that it is only in proportion as he feels the need of illustration, and realises that his whole case is hypothetical, that he is driven back upon ethnology as an ornament of style and as a makeshift for proof. Unlike his predecessors, however, he cannot be given credit for great learning on the point at issue, and he frankly admits as much:— 'As we know so little of Nature and agree so ill as to the meaning of the word Law, it would be difficult to settle on a good definition of the Law of Nature.' There was, however, a good deal known about 'Nature' in 1753 which was not in Rousseau's philosophy. Yet he had clearly read travels, as everyone did in those days, and he reproduces a few details as to the qualities and customs of savages.

He quotes Peron's 'Voyages aux Terres Australes' for the comparative strength of Europeans and Tasmanians, and illustrates sensory acuity from Hottentots and Redskins; but his favourite type is the Carib, whom we have already met in discussing Defoe. It is the 'Carib of Venezuela' who shows such surprising skill in tackling wild animals; it is, too, 'the inhabitant of the banks of the Orinoco,' who learned the use of 'those boards which he applies to the temples of his children, and which assure to them at least part of their natural idiocy and happiness.' It is the 'Carib' again who 'sells his cotton mattress in the morning and comes with tears in the evening to buy it back, for lack of foresight that he

was going to want it for the coming night,' and whose happiness is, nevertheless, so quaintly compared with that of a European Minister of State. There is a curiously Amazonian flavour, meanwhile, about Rousseau's sketch of the primitive family. 'The most ancient of all societies, and the most nearly natural, is that of the family. But even here the children do not stay bound to the parent any longer than they need him for their own maintenance. As soon as this need ceases, the natural tie dissolves. The children, released from the obedience which they owed to the father, the father released from the care which he owed to the children, all return equally to independence. This common liberty is a consequence of human nature.' Such an analysis is, of course, only true in fact under the conditions of a tropical forest. Nowhere else does the family tie break down in the way Rousseau describes; and nowhere was this type of social anarchy more open to study than in the equatorial forests of South America.

Whence did Rousseau acquire his conception of the Carib? The most obvious source would be the 17th volume of the Abbé Prévost's '*Histoire Générale des Voyages*,' which contains a full summary of the 'Origin, Character, and Customs' of the Caribs, and a narrative of European colonisation of the Antilles; but this volume does not seem to have been published till 1761. Raynal's '*Histoire Philosophique et Politique des Établissements et du Commerce des Européens dans les deux Indes*,' published in Geneva in 1781, is also too late; but Raynal in particular had a wide acquaintance, and his ideas were current in French society long before his book came out; so we are probably safe in crediting Rousseau with at all events a gossiping acquaintance with a type of savagery which was enjoying a considerable vogue in his time.

'Wild Peter.'

Both Rousseau and Montesquieu were, of course, also in a position to enjoy the perplexities of the advocates and assailants of the doctrine of innate ideas when a real live specimen of *Homo sapiens ferox* turned up in the Hanoverian forests in the year 1724. The story of 'Wild Peter' is probably familiar reading, but though the literature which this poor creature provoked is in parts diverting both to the anthropologist and to the philosopher, I should encumber my story unduly if I digressed. Montesquieu, having been in England and having his friends in London, has not very much to say; but Rousseau gives 'Wild Peter' a long note, and was evidently considerably impressed.

The South Sea Islanders.

Rousseau wrote just too early to be able to make use of what must have appeared to his contemporaries a remarkable confirmation of his view of the State of Nature—namely, the discovery by Cook, Bougainville, and La Perouse of the Polynesian Islanders. But this discovery, coming as it did so closely after Rousseau's manifesto, and so markedly confirming certain phases of his sketch, seems to have attracted some attention and to have been given more than its due weight. For it came, at all events to the public mind, as the revelation of a new type of Man and Society, still more remote from contact with the modern world even than the Carib and the Iroquois, still more likely therefore to have withstood the attacks of reason, if not of time, and consequently to have preserved some traces of the original State. The South Seas had, of course, been traversed cursorily since the days of Magellan; Dampier had done much to make their natives known; and I have indicated the share which his work may have had in forming the portrait of Man Friday. But it was not till after the publication of Rousseau's 'Discourse' that the significance of these data was appreciated; and ethnology owes much in this instance to philosophy for the impulse which was given in the generation which follows to the study of 'Pacific Man', in more senses than one; though I think the debt is in part repaid when we see what Herder owes to ethnology.

The Pacific Islanders, of course, with their Garden of Eden existence, challenged all preconceived notions of the defective mentality of races remote from

Europe, and effected an almost Copernican revolution in the self-centred ethnology of the discoverers. If a South Sea Islander like Omai could pick up English, play chess, and behave like a gentleman after a few months' consort with Europeans, there could not be much amiss with his 'mind'; and it was clearly time to amend current conceptions as to the identity of the primitive with the remote.

George Forster, for example, who wrote the first really philosophical account of the voyages of Captain Cook, with whom his father had sailed as one of the chief naturalists of the Expedition, was completely convinced by his experiences that the Biblical record was true after all, and that the primitive state of man was a state of innocence and happiness. It was a reaction against the ideas of Hobbes, Locke, and Montesquieu, which went far beyond what was contemplated even by Rousseau, and it did more to retard the progress both of anthropology and of general biology than anything else in that century.

So long as the sentimental enthusiasm aroused by Rousseau persisted, there was little hope of advance in the direction of a solid ethnology. But in England the contagion was slighter, the contact with the facts of exploration closer, and the reaction earlier; and Germany too was already well awake, with Herder, almost before the Revolution was ablaze.

'I take this opportunity,' writes Chamisso, who had himself been in the Pacific in 1815-18,¹ 'to protest most vigorously against the term savage in its application to the South Sea Islanders. I prefer, so far as I can, to connect definite ideas to the words which I use. A savage for me is the man who in the absence of fixed abode, agriculture, and domestic animals, knows no form of property but his weapons, with which he maintains himself by the chase. Wherever the South Sea Islanders can be accused of corruption of morals, this seems to me to bear indication not of savagery but of over-civilisation. The various inventions, coinage, writing, and the like, which are appropriate to mark off the different degrees of civilisation which the peoples of our continent have attained, cease to afford under conditions so different any standard for this insular and isolated stock which lives under this happy sky, without yesterday or to-morrow, living for the moment, and for pleasure.'

Voltaire.

I must leave out of consideration here the results of these successive pictures of the Pre-Social State on the course of Political Philosophy. All I am concerned to do here is to give reasons why these different conceptions took the particular shape that they did, under the several circumstances of the age which gave birth to them; and I hope that I have been able to show that one of the principal factors which determined their form was the actual state of anthropological knowledge in the years which immediately preceded the publication of each.

A good example—if this were the time to develop it fully—is the very entertaining controversy between Rousseau and Voltaire over the psychical unity and uniformity of Man. What led Voltaire to so totally opposite a conception of the state of Nature to that entertained by Rousseau? Partly, of course, his own political and philosophic standpoint, with which we are not concerned directly here; but partly also the circumstances that in the years which immediately preceded his attack upon Rousseau, the learned world of Europe—and learned France in particular—had come under the influence of a fashion—I might almost call it a craze—of enthusiastic admiration of China and things Chinese. The Jesuit Missions to China, in particular, had been sending home wonderful accounts of the civilisation of the Chinese, and fabulous versions of its antiquity; and it was, of course, common knowledge in Europe in the eighteenth century that any civilisation which went back into the second and third thousand years B.C. must be in respectably close contact with the Origin of Man, and therefore might be expected to reflect at close quarters the outlines of the original State. To find, therefore, that this immemorial

¹ Chamisso, *Works* i. 119.

civilisation of China had existed apparently unchanged since its first ages, was to discover fresh light on the nature of Man and a new glimpse of primitive society. By this revelation of China, it is true, the Pharaoh's heart of the *ancien régime* was hardened in pursuit of what has come down into our vocabulary as *chinoiserie*; and, by a strange irony, one of the acutest critics of that *régime* was furnished from the same source with a fresh instrument of proof of the essentially social nature of Man in reply to the Nihilism of Rousseau. 'Do you mean by "primitive man" (*sauvages*) a two-footed animal, walking on its hands too if occasion calls, isolated, wandering in the forests, pairing at hazard, forgetting the woman with which he has mated, knowing neither her offspring nor his parents, living like a beast, only without the instinct and the resources of the beasts. You will find it in books that this state is the true estate of man, and that we have merely degenerated pitifully since we left it. But I do not think that this solitary life ascribed to our forefathers is in human nature at all. If I am not mistaken, we are in the first rank of the gregarious animals, much as bees, wasps, and the like. If you come across a strayed bee, ought you to infer that this bee is in the state of mere nature, and that those which work in association in the hive have degenerated? All men do live in Society: can you infer from that, that there was a time when they did not?' 'Man in general has always been what he is. That does not mean that he has always had fine cities and so on: but he has always had the same instinct which leads him to feel affection for himself, for the companion of his toils, for his children, and so forth. That is what never changes, from one end of the world to the other. As the basis of society is always in existence, there always is some society. We were not made to live after the manner of bears'—a clear hit at the favourite simile of Montesquieu. 'It is therefore demonstrated that Nature alone inspires us with the useful conceptions which precede all our thoughts. In morals it is the same. We all have two instincts which are the basis of society, pity and justice.'¹

From this fundamental uniformity of the human mind, which Voltaire assumes and defends, it follows that certain fundamental ideas recur everywhere, under suitable circumstances, more especially such religious dogmas as the conception of the immortality of the soul. In this conception it will be seen that Voltaire at the same time reverts almost completely to the anthropological standpoint of Aristotle, and anticipates by a century the philosophic position of Bastian. But it is also clear that Voltaire's mode of arriving at the Natural State of Man does not differ in its method from that of his predecessors. Both alike discover it by the process of subtracting from human nature, as we know it, all that can be traced to the operation of any positive prescription or observance. What each side finds lying behind this customary stratum of human nature, whether sheer passivity, or positive qualities of a selfish tendency, or otherwise, depends, as before, partly on the prejudices of the observer, but mainly on the current phase of emphasis on this or that section of what was known.

Christopher Meiners.

The new attitude towards Rousseau is well illustrated by the criticism of Christopher Meiners, whose 'Historical Comparison of the Customs and Constitutions, the Laws and Industries, the Trade and Religion, the Sciences and Educational Institutions of the Middle Ages' was published at Hanover in 1793. 'Experience, history, and sound reason,' he says, 'are mishandled (by Rousseau) with unprecedented audacity. On all sides false or distorted facts are treated as fundamental, and the best known and best attested observations are misinterpreted or left on one side.'² 'Among the poets of enlightened peoples there is hardly to be found any fiction so utterly in conflict with experience and history as Rousseau's picture of the State of Nature, and of Natural Man.' But Meiners'

¹ Voltaire, *Œuvres*, xi. 19, 21; see also Rousseau's reply to this position, *Discours sur l'origine et les fondemens de l'inégalité parmi les hommes*, p. 170.

² Vol. i. pp. 7, 16, 18.

criticism is directed wholly against Rousseau's ignorance of anthropological fact, and most particularly of facts about 'modern savages'; not against the principles of his method. For, as Meiners himself contends, 'the most important conditions in which considerable sections of the human race have been or are now to be found, are the conditions of savagery and barbarism, of incipient, or half-completed, or entire enlightenment.' 'Human history devotes its particular attention to the savages and barbarians of all parts of the world, who have never produced the smallest perceptible change in the fortunes of humanity as a whole; because often a single small horde of savages and barbarians can make greater contributions to the knowledge of human nature than the most magnificent peoples who ever conquered and devastated a continent.' And Meiners goes on to hit also Montesquieu for his failure to appreciate the contribution of savages to political philosophy. Here we have clearly the beginnings of the modern comparative method, with its search of uncontaminated survivals of primitive, though not strictly 'pre-social' states.

Herder.

But it is mainly to Herder that the expression of the new movement is due; and it is his 'Thoughts on the History of Mankind'¹ that makes the first systematic attempt to solve the problem of the development of man and his culture, and to create, in the modern sense, a Science of Man.

'Already in comparatively early years,' he says, 'when the field of knowledge lay before me in all that morning glory from which life's midday sun detracts so much, the idea often besets me, since everything in the world has its philosophy and science, ought not human history, which after all lies nearest to ourselves, to have in a general sense its philosophy and science also?' He argues, thereupon, that we must discard speculation and follow experience simply. 'When, therefore, we set about philosophising upon the history of our species, let us forswear, as far as possible, all narrow forms of thought which are derived from the culture of a single region, or even of a single school. It is not what man is among ourselves, nor what he ought to be in the conception of any dreamer whatever—this is clearly aimed at Rousseau—but what he is, on the earth in general, and at the same time in every single region in particular; or rather, what it is to which the rich multiplicity of accidents in the hands of Nature has had the power to train him. This is what we are to regard as the purpose of Nature for him.'

Herder, that is, conceives it as possible, at the same time to determine inductively what Man is in himself, and to determine by simple description what he actually is (or rather what *men* actually *are*) under the various different conditions in which we find him. But he insists on the distinction between these two modes of regarding Man, or Men; and rightly, for it is the confusion between the description of this or that kind of uncivilised Man—Iroquois, Hottentot, or South Sea Islander—and the guess that uncivilised Man everywhere *must* have such and such qualities or defects of qualities—which had in fact produced all the discrepancies between the previous theories of a Pre-Social state.

Writing when he did, Herder of course was but little more capable than his predecessors of delineating human nature in detail on inductive lines. His merit lies in the clearness with which he gripped and stated the conditions of the problem; in an advance of method, which came just in time to guide the theoretical treatment of a vast mass of new data. At the same time he did accomplish a good deal, even as regards the filling in of the picture. In particular he marks the turn of the tide from the philosophy of the Pre-Social State towards the old Aristotelian conception of Man as a social animal. Both Hobbes and Locke, though not I think anywhere named, come in for effective criticism. 'There have been philosophers,' he says, 'who on account of this instinct of self-preservation have classified our species among the carnivora, and made out its natural state to be a state of war. Of course when Man plucks the fruit of a tree he is

¹ Herder, *Ideen zur Geschichte der Menschheit*.

a robber; when he kills an animal he is a murderer; and when—with a footstep, with a breath, perhaps—he takes the life of myriads of invisible creatures, he is the most brutal oppressor on earth . . . But put Man among his brethren, and ask the question, Is he naturally a beast of prey of his own kind, is he an “unsocial” being? In his physical shape he is clearly not the former, by his birth still less the latter.’ Herder is thus returning afresh to the Aristotelian conception of the parental bond as the complement and remedy of the long helpless infancy. Herder’s ideal Man has, in fact, a Humanity which is in itself an end, an ideal, not a pre-social attribute, and just for this reason Humanity exists potentially in all members of the species, however small their progress towards realising it, or however eccentric the results of their social activity. ‘Look at the godlike laws and regulations of Humanity, which emerge, if only in the merest traces, among the most savage peoples. Can they really have been invented by the exercise of reason only after the lapse of thousands of years? Can they really owe their origin to this changeful sketch, this man-made abstraction? I cannot believe it, even from the standpoint of history. If men had been distributed like animals on the earth’s surface, to invent for themselves the inner form of Humanity, we should still find mere human stocks, without language, without reason, without religion or morals; for as Man was created such is he still upon the earth.’

The Patriarchal Theory.

All these theories of a Social Contract as the starting-point of human societies presupposed, as we have seen, that mankind had actually passed through a Pre-Social State; and the proof which had been offered of this supposition, though partly theoretical and *a priori*, had partly also been inductive and based on experience. Further, the experience of ‘primitive Man’ which was actually open to the philosophers of the seventeenth and early eighteenth centuries, had been, in fact, such as to force the conclusion not merely that a Pre-Social State had once existed, but that some barbarous peoples had not yet emerged from it. It was a sad error of observation, as we now know, which led to that conclusion; but given the travellers’ tales, in the form in which we can read them in the ‘Cosmographies’ and ‘Voyages’ of the time, I do not see how that conclusion could have been avoided without culpable neglect of such evidence as there was. If blame is to be assigned in this phase of inquiry at all, it is to be assigned to the travellers and traders, for making such poor use of their eyes and ears. All, however, that I am concerned to establish at present is this, that one of the most important and far-reaching speculations of modern political philosophy, the speculation as to a Pre-Social Condition of Mankind, and a Social Contract which ended it and brought in Society and the State, arose directly and inevitably from the new information as to what primitive man *was* and *did*, when he was studied in the seventeenth century at Tombutum, or Saldanha Bay, or the ‘backwoods of America,’ or the ‘bank of the Orinoco river.’

But the Social Contract Theory has long since passed out of vogue. Its political consequences are with us to-day, like the political consequences of the belief in the Divine Right of Kings; but the theories themselves are dead, and likely to remain so. Plato and Aristotle, with their belief in Man as a Naturally Social Animal, have come by their own again, for most of us, if not for all; and the search for an ideal State, which shall realise and fulfil Man’s social instincts, is again in full cry.

What part, if any, has the direct study of barbarous people played at this fresh turn of the wheel? Let us look once again at the state of geographical knowledge, and more particularly, as before, at the regions in which by transitory chance of circumstances, there was most to be learned at the moment. First, the British occupation of India was the occasion, on the one hand, of the discovery of Sanskrit, the creation of this science of comparative philology, and the demonstration of a new link of cultural affinity over the whole realm of Aryan speech. The same political event led no less directly to the discovery of the patriarchal structure of Hindoo society, and so through the comparative study of

Indian, Roman, and ancient Celtic and Teutonic law to an inductive verification of Aristotle's doctrine of the 'naturalness' of patriarchal society. This doctrine dominated political science for nearly fifty years. 'The effect of the evidence derived from comparative jurisprudence,' Sir Henry Maine could write in 1861,¹ 'is to establish that view of the primæval conditions of the human race which is known as the Patriarchal Theory. There is no doubt, of course, that this theory was originally based on the Scriptural theory of the Hebrew patriarchs in Lower Asia. . . . It is to be noted, however, that the legal evidence comes nearly exclusively from the institutions of societies belonging to the Indo-European stock, the Romans, Hindoos, and Slavonians supplying the greater part of it; and indeed the difficulty, at the present stage of the inquiry, is to know where to stop; to say of what races of men it is *not* allowable to lay down that the society in which they are united was originally organised on the patriarchal model.' And he refers explicitly to the former controversy between Filmer and Locke, to point out how the tables had now been turned upon the latter.

Thus in the half-century which intervenes between Herder and Maine, the political philosophy of Europe seemed to have turned almost wholly from exploration to introspection; from the Pacific to early Rome and the German forests; and from the study of survivals in the modern practice of savages, to that of primæval custom betrayed by the speech and customs of the civilised world. It was Aristotle over again, with his appeal to custom, ancestral belief, and canonical literature, following hard upon the heels of the visionary revolutionary Plato. Maine's own words, indeed, about Rousseau² would be applicable almost without change to the course of Greek thought in the fourth century B.C. 'We have never seen in our own generation,' he says, 'indeed the world has not seen more than once or twice in all the course of history, a literature which has exercised such prodigious influence over the minds of men, over every cast and shade of intellect, as that which emanated from Rousseau between 1749 and 1762. It was the first attempt to re-erect the edifice of human belief after the purely iconoclastic efforts commenced by Bayle, and in part by our own Locke, and consummated by Voltaire; and besides the superiority which every constructive effort will always enjoy over one that is merely destructive, it possessed the immense advantage of appearing amid an all but universal scepticism as to the soundness of all foregone knowledge in matters speculative. . . . The great difference between the views is that one bitterly and broadly condemns the present for its unlikeness to the ideal past, while the other, assuming the present to be as necessary as the past, does not affect to disregard or censure it.'

I have devoted some space to these first steps of Linguistic Palæontology and Comparative Jurisprudence because the method of inquiry which they announced promised at first sight to make good a very serious defect in the instruments of anthropological research. Human history, outside of Europe and of one or two great oriental states like China, hardly went back beyond living memory; even Mexico had no chronicles beyond the first few hundred years, and the records of old-world states like China, which at first sight offered something, turned out on examination to have least to give. They had lived long, it is true, but their lives had been 'childlike and bland,' devoid of change, and almost empty of experience. Consequently there was no proof that the 'wild men' of the world's margins and byways were really primitive at all. The Churches held them children of wrath, degenerate offspring of Cain; the learned fell back upon pre-Adamite fictions, to palliate, rather than to explain their invincible ignorance of Europe and its ways. Here, however, in the new light thrown by the history of speech, there seemed to be a prospect of deep insight into the history of human societies. Disillusionment came in due course, when doctors disagreed; but illusion need never have taken the form it did, had either the philologists or the philosophers realised that all the really valuable work was being done within the limits of a single highly special group of tongues; that the very circumstance that this group of tongues had spread so widely, pointed to some strong impulse driving the men who spoke them into far-reaching migrations; that one

¹ Maine, *Ancient Law*, pp. 122-3.

Ibid. pp. 86-9.

of the few points upon which linguistic palaeontologists were really unanimous was that both the Indo-European and the Semitic peoples, in their primitive condition, were purely pastoral; and that this pastoral habit was itself an almost coercive cause for their uniformly patriarchal organisation. The last point, however, belongs so completely to another phase of our story that it is almost an anachronism to introduce it here. It serves however to indicate, once again, if that be necessary, how completely the philosopher, and even the man of science, is at the mercy of events in the ordering of his search after knowledge. It is, indeed, almost true to say that if the primitive Aryan had not had the good fortune not merely to live on a grass-land, but also to find domesticable quadrupeds there, there could no more have been a science of comparative philology in modern Europe, than there could be among the natives of your own Great Plains or of the Pacific Coast: for in no other event would there have been any such 'family of languages' to compare.

In the absence of warning thoughts like these, however, the comparative philology and the comparative law of the patriarchal peoples of the North-West Quadrant and of India went gaily on. What Maine had done for India, Maine himself, with Solm and von Maurer, in Germany; Le Play, de Laveleye, and d'Arbois de Joubainville in France; W. F. Skene in far-off Scotland; Whitley Stokes and others in Ireland; Rhys in Wales; and Mackenzie Wallace and Kovalevsky in Russia, had done for the early institutions of their respective countries: all emphasising alike the wide prevalence of the same common type of social structure, based upon the same central institution, the Patriarchal Family, with the *Patria Potestas* of its eldest male member as its overpowering bond of union; and Maine's own words do not the least exaggerate the beliefs and expectations which were evoked by this new aspect of the Study of Man.

The Matriarchate in Southern India, Africa, and North America.

The Patriarchal Theory lasted barely fifty years. It had owed its revival, as we have seen, to two fresh branches of research, comparative jurisprudence and comparative philology, both stimulated directly by the results of European administration in Northern India. It owed its decline to the results of similar inquiries in other parts of the world, stimulated no less directly by other phases of the great colonising movement, which marks, above all other things, the century from 1760 to 1860. Here again a small number of examples stand out as the crucial instances. British administration in India had, of course, been extended over the non-Aryan south, as well as over the north; and in Travancore, and other parts of the Madras Presidency, British commissioners found themselves confronted with types of society which showed the profoundest disregard of the Patriarchal Theory. Like the Lycians of Herodotus, these perverse people 'called themselves after their mothers' names': they honoured their mother and neglected their father, in society, and government, as well as in their homes; their administration, their law, and their whole mode of life rested on the assumption that it was the women, not the men, in whom reposed the continuity of the family and the authority to govern the State. Here was a *parechasis*, a 'perverted type' of society, worthy of Aristotle himself. It is a type which, as a matter of fact, is widely distributed in Southern and South-eastern Asia, and had been repeatedly described by travellers from the days of Tavernier (in Borneo) and Laval (in the Maldivé Islands), if not earlier still. It existed also in the New World, and Lafitau had already compared the Iroquois with the ancient Lycians. But it was Buchanan's account of the Nairs of the Malabar Coast, published in 1807, which came at the 'psychological moment,' and first attracted serious attention. At the other extremity of India, also, analogous customs were being recorded, about the same time, by Samuel Turner in Tibet, which might have given pause at the outset to the speculators who hoped to base general conclusions on anything so special and peculiar as the customs of Aryan India.

Similar evidence came pouring in during the generation which followed; partly, it is true, as the result of systematic search among older travellers, but mainly through the intense exploitation of large parts of the world by European

traders and colonists. Conspicuous instances are the Negro societies of Western and Equatorial Africa, first popularised by the republication of William Bosman's 'Guinea' (1700), in Pinkerton's 'General Collection of Voyages and Travels' (London, 1808, &c.), and by Proyard's 'Histoire de Loango' (1776), which also reached the English public in the same invaluable collection. But it was from the south that the new African material came most copiously, in proportion as the activity of explorers, missionaries, and colonists was greater. Thunberg's account of the Bechuanas¹ takes the lead here; but for English thought the principal authorities are, of course, John Mackenzie² and David Livingstone.³

It was not to be expected that America, which had made such remarkable contributions to the study of Man in the seventeenth and eighteenth centuries, should fall behind in the nineteenth, when its vast resources of mankind, as of Nature's gifts, were being realised at last. From Hunter,⁴ Gallatin,⁵ and Schoolcraft,⁶ in the twenties, to Lewis Morgan⁷ in 1865, there was hardly a traveller 'out West' who did not bring back some fresh example of society destructive of the Patriarchal Theory.

As often happens in such cases, more than one survey of the evidence was in progress simultaneously. Bachofen was the first to publish,⁸ and it is curious that his great book on 'Mother-right' appeared in the very same year as Maine's 'Ancient Law.' Lubbock's 'Prehistoric Times,' in the next year, represents the same movement of thought in England in a popular shape, but almost independently. In America, Lewis Morgan, whom I have noted already as an able interpreter of Iroquois custom, followed up his detailed studies of Redskin law by a Smithsonian monograph in 1871 on 'Systems of Consanguinity and Affinity of the Human Family,' and, in 1877, by his book on 'Ancient Society.' Meanwhile Post had published his great work on the 'Evolution of Marriage'⁹ in 1875, and J. F. McLennan his first 'Studies in Ancient History' in 1876. It was the generation of Darwin and of the great philologists, as we have seen, and 'survivals' were in the air: Dargan¹⁰ pointed out traces of the Matriarchate in the law and custom of Germany, and Wilken¹¹ in those of early Arabia. The period of exploration, if I may so term it, closed on this aspect of the subject with Westernmarck's 'History of Human Marriage,' which was published in London in 1891.

Australian Evidence: Totemism and Classificatory Kinship.

I have now mentioned India, South Africa, and North America, three principal fields of English-speaking enterprise during the nineteenth century, and have indicated the contribution of each to modern anthropology in its bearing on political science. Only Australia remains; and, though Australia's task has been shared more particularly with North America, I shall be doing no injustice to Lewis Morgan or to McLennan if I couple with their names those of Fison and Howitt,¹² as the discoverers of classical instances of societies which observe neither paternal nor maternal obligations of kinship as we understand them, but

¹ Pinkerton, vol. xvi.

² John Mackenzie, *Ten Years North of the Orange River* (1859-69). Edinburgh, 1871.

³ David Livingstone, *Narrative of an Expedition to the Zambesi and its Tributaries* (1858-64). London, 1865.

⁴ Hunter, *Manners and Customs of several Indian Tribes located West of the Mississippi*. Philadelphia, 1823.

⁵ Gallatin, *Archæologia Americana*. Philadelphia (from 1820 onwards).

⁶ Schoolcraft, *Travels in the Central Portions of the Mississippi Valley* (New York, 1825; *Notes on the Iroquois* (1846)).

⁷ Lewis H. Morgan, *Proc. Am. Acad. Arts and Sciences*, vii. 1865-8.

⁸ Bachofen, *Das Mutter-recht*. Stuttgart, 1861.

⁹ Hermann Post, *Die Geschlechts-genossenschaft der Urzeit und die Entstehung der Ehe*. Oldenburg, 1875.

¹⁰ Dargan, *Mutter-recht und Raub-ehe und ihre Reste im Germanischen Recht und Leben*. Breslau, 1883.

¹¹ Wilken, *Das Matriarchat bei den alten Arabern*. Leipzig, 1884.

¹² Fison and Howitt, *Kamilaroi and Kurnai*. Melbourne and Sydney, 1880.

have adopted those purely artificial systems of relationships which in moments of elation we explain as 'Totemic,' or, in despair, describe as 'classificatory.'

Hermann Post: Comparative Jurisprudence.

Our retrospect, therefore, of the last fifty years shows clearly once again how intimately European colonisation and anthropological discoveries have gone hand in hand: first to establish a 'Matriarchal Theory' of society as a rival of the Patriarchal; and then to confront both views alike with the practices and with the theories of 'Totemism.'

From the point of view of political science, all this mass of inquiries finds applications already in more departments than one; though it is probably still too early to appraise its influence adequately. The new Montesquieu has not yet arisen to interpret to us the 'Spirit of the Laws.' Most directly, perhaps, we can trace such influence in the 'Comparative Jurisprudence' of Hermann Post, whose first work on the 'Evolution of Marriage' appeared, as we have seen, in 1875. Post's general attitude is best seen in his 'Introduction to the Study of Ethnological Jurisprudence,' which was published in 1886, and in his 'African Jurisprudence' of 1887.¹ As the result of a survey of social organisations, considered as machinery in motion, Post points out very justly that it is useless to attempt to explain social phenomena on the basis of the psychological activities of individuals, as is too commonly assumed, because all individuals whose conduct we can possibly observe have themselves been educated in some society or other, and presume in all their social acts the assumptions on which that society itself proceeds. 'I take the legal customs of all peoples of the earth,' so he wrote in 1884,² 'the residual outcome of the living legal consciousness of humanity, for the starting-point of my inquiry into the science of law; and then, on this basis, I propound the question, What is law? If by this road I arrive eventually at an abstract conception of law, or at an idea of law, then the whole fabric so created consists, from base to summit, of flesh and blood.' It is the same method, of course, which had already yielded such remarkable results to Montesquieu, and even to Locke. The point of view is no longer that of a Maine or a McLennan, students of patriarchal or of matriarchal institutions by themselves. It is that of a spectator of human society as a whole; and such a point of view only became possible at all when it was already certain that no great section of humanity remained altogether unexplored, however fragmentary our knowledge might still be, of much that we ought to have recorded. And its immediate outcome has been to throw into the strongest possible relief the dependence of the form and still more of the actual content of all human societies on something which is not in the human mind at all, but is the infinite variety of that external Nature which Society exists to fend off from Man, and also to let Man dominate if he can.

This was, of course, already the standpoint of Comte, with his emphasis on the *monde ambiant*. But Comte, the citizen of a State which except in Canada had failed to colonise, and therefore had little direct contact with non-European types of society, confined himself far too exclusively to European data. His strength is precisely where the science of France was so magnificently strong in his day, in the domain of pure physics; it is his analogies between politics and physics which are so illuminating in his work, as in that of his English compeer, Herbert Spencer;³ and it is the weakness of both in the direction of anthropology which mainly accounts for the shortness of their respective vogues.

¹ Hermann Post, *Einleitung in das Studium der ethnologischen Jurisprudenz* (Oldenburg 1886); *Afrikanische Jurisprudenz* (1887). His position is however already clear in his first synthetic work, *Der Ursprung des Rechts*, 1876, as well as in his earlier book on Marriage. For a good summary of Post's views see Th. Achelis, *Die Entwicklung der modernen Ethnologie* (Berlin, 1889), p. 113-128, and the same writer's *Moderne Ethnologie* (1896).

² Post, *Die Grundlagen des Rechts* (1884).

³ Compare Quetelet's *Essai de Physique Sociale* (1841), as a symptom of the trend of French thought at this stage.

Friedrich Ratzel: Anthro-geography.

At the point which we have now reached in this rapid survey of our science, it was obviously to Geography—the systematic study of those external forces of Nature as an ordered whole—that Anthropology stretched out its hands; and it did not ask in vain. But while English geography had remained exploratory, descriptive, and (like English geology) *historical* in its outlook, the new German science of *Erdkunde*—‘earth-knowledge’ in the widest sense of the word—had already come into being on the basis of the labours of Ritter and the two Humboldts, and under the guidance of such men as Wagner, Richthofen, and Bastian; the last named also an anthropologist of the first rank. It was, thus, to a distinguished pupil of Wagner, Friedrich Ratzel, that anthropology owed, more than to any other man, the next forward step on these lines. In Ratzel’s mind, History and Geography went hand in hand as the precursors of a scientific Anthropology.¹ History to define *when*, and in what order, Man makes his conquests over Nature; Geography to show *where*, and within what limits, Nature presents a conquerable field for Man. Much of this, of course, was already implicit in the teaching of Adolf Bastian, whose monumental volumes on ‘Man in History’ had appeared at Leipzig as early as 1860; his ‘Contributions to Comparative Psychology’ in 1868; and his ‘Legal Relations among the Different Peoples of the Earth’ in 1872²—three years before Post’s first essay. But Bastian, inaccessible for years together in Tibet or Polynesia, was rather an inspiration to a few intimate colleagues than a great propagandist; and besides, it was not till the appearance of his ‘Doctrine of the Geographical Provinces’ in 1886³ that he touched on this precise ground, and by that time Ratzel’s ‘History of Man’ had already been out for a year.⁴

Epilogue.

These examples, I think, are sufficient to show how intimately the growth of political philosophy has interlocked at every stage with that of anthropological science. Each fresh start on the never-ending quest of *Man as he ought to be* has been the response of theory to fresh facts about *Man as he is*. And, meanwhile, the dreams and speculations of one thinker after another—even dreams and speculations which have moved nations and precipitated revolutions—have ceased to command men’s reason when they ceased to accord with their knowledge.

And we have seen more than this. We have seen the very questions which philosophers have asked, the very questions which perplexed them, no less than the solutions which they proposed, melt away and vanish, *as problems*, when the perspective of anthropology shifted and the standpoint of observation advanced. This is no new experience; nor is it peculiar either to anthropology among the natural sciences, or to political science among the aspects of the Study of Man. It is the common law of the mind’s growth, which all science manifests, and all philosophy.

And now I would make one more attempt to put on parallel lines the course of political thinking. It is not so very long ago that a great British administrator, returning from one of the gravest trials of statesmanship which our generation has seen, to meet old colleagues and class-mates at a college festival, gave it to us as the need he had most felt, in the pauses of his administration, that there did not exist at present any adequate formulation of the great outstanding features of our knowledge (as distinct from our creeds) about human societies and their mode of growth, and he commended it to the new generation of scholarship, as its highest and most necessary task, to face once more the question: What

¹ Ratzel, *Anthro-geographie*. Leipzig, vol. i. 1882; ii. 1891.

² Bastian, *Der Mensch in der Geschichte* (Leipzig, 1860); *Beiträge zur vergleichenden Psychologie* (Berlin, 1868); *Rechtsverhältnisse bei verschiedenen Völkern der Erde* (Berlin, 1872).

³ Bastian, *Zur Lehre von den geographischen Provinzen*. Berlin, 1886.

⁴ Ratzel, *Völkerkunde* (Leipzig, 1885). His *method* is best studied in the first volume of his *Anthro-geographie* (Leipzig, 1882).

are the forces, as far as we can know them now, which, as Aristotle would have put it, 'maintain or destroy States'?

But if a young student of political science were to set himself to this life work, where could he turn for his facts? What proportion of the knowable things about the human societies with which travellers' tales and the atlases acquaint him could he possibly bring into his survey, without a lifetime of personal research in every quarter of our planet?

I have in mind one such student setting out this coming session to investigate, on the lines of modern anthropology, the nature of *Authority* and the circumstances of its rise among primitive men; and the difficulty at the outset is precisely as I have described. In the case of the 'black fellows' of Australia such a student depends upon the works of some four or five men, representing (at a favourable estimate) one-twentieth even of the known tribes of the accessible parts of that continent. For British South Africa he would be hardly better served; for British North America, outside the ground covered in British Columbia by Boas and Hill-Tout, he would have almost the field to himself: and the prospect would seem to him the drearier and the more hopeless when he compared it with things on the other side of the forty-ninth parallel.

Now, our neighbours south of that line have the reputation of being practical men; in other departments of knowledge they are believed to know well 'what pays.' And I am forced to believe that it is because they know that it *pays*, to know all that can still be known about the forms of human society which are protected and supervised from Washington, that they have gone so far as they have towards rescuing that knowledge from extinction while still there is time. The Bureau of Ethnology of the United States of America is the most systematic, the most copious, and, I think, taking it all in all, the most scientific of the public agencies for the study of any group of men, *as men*. The only other which can be compared with it is the ethnographical section of the last census of India, and that was an effort to meet, against time, an emergency long predicted, but only suddenly foreseen by the men who were responsible for giving the order. Thus, humanly speaking, it is now not improbable that in one great newly-settled area of the world every tribe of natives, which now continues to inhabit it, may at least be explored, and in some cases really surveyed, before it has time to disappear. But observe, this only applies to the tribes which now continue to exist; and what a miserable fraction they are of what has already perished irrevocably! It is no use crying over spilt milk, as I said to begin with; the only sane course is to be doubly careful of whatever remains in the jug.

An Ethnological Survey for Canada.

And now I conclude with a piece of recent history, which will point its own moral. When the British Association met first outside the British Isles, it celebrated its meeting at Montreal by instituting, for the first time, a section for Anthropology; and it placed in the chair of that section one of the principal founders of modern scientific anthropology, Dr. Edward Burnett Tylor, then recently installed at Oxford, and still the revered Professor of our science there. Through his influence mainly, but with the active goodwill of the leading names in other sciences in Canada, a research committee was formed to investigate the north-west tribes of the Dominion; and for eleven consecutive years expeditions wholly or partly maintained by this Association were sent to several districts of British Columbia. These expeditions cost the Association about £1,200 in all. I am glad to think that the chief representative of this Committee's work, Dr. Franz Boas, has long since realised, in his great contributions to knowledge, the high hopes which his early reports inspired.

When the Association met the second time on Canadian soil, at Toronto, the occasion seemed opportune for a fresh step. Dr. Boas had already undertaken work on a larger scale and under other auspices. But it was thought likely that if a fresh Committee of the Association were appointed, with wider terms of reference and further grants, it would be possible to select and to train a small staff of Canadian observers, and by their means to produce such a series of

preliminary reports on typical problems of Canadian anthropology as would satisfy the Dominion Government that the need for a thorough systematic survey was a real one, and that such a survey would be practicable with the means and the men which Canada itself could supply. Among the leading members of this Ethnographic Survey Committee I need only mention three—the late Dr. George Dawson, Mr. David Boyle, and Mr. Benjamin Sulte, each eminent already in his own line of study, and all convinced of the great scientific value of what was proposed. The first year's enterprise opened well; workers were found in several districts of Canada; the Association sent out scientific instruments, and formed in London a strong consultative committee to keep the Canadian field-workers in touch with European students of the subject. But the premature death of George Dawson in 1901 broke the mainspring of the machine; the field-workers fell out of touch with one another and with the subject; the instruments were scattered, and in 1904 the Ethnographic Survey Committee was not recommended for renewal.

I need not say how great a disappointment this failure has been to those of us who believe that in this department of knowledge Canada has great contributions to make, and who know—as this meeting too knows perfectly well—that if this contribution to knowledge is not made within the next ten years, it can never be made at all. I am not speaking merely of the urgency of exact study of the Indian peoples. This indeed is obvious and urgent enough; and the magnificent results of organised effort in the United States are there to show how much you too can still rescue, if you will. But at the moment I appeal rather for the systematic study of your own European immigrants, that stream of almost all known varieties of white men with which you are drenching yearly fresh regions of the earth's surface, which if they have had experience of human settlements at all, have known man only as a predatory migratory animal, more restless than the bison, more feckless and destructive than the wolf. Of your immigrants' dealings with wild nature, you are indeed keeping rough undesigned record in the documents of your Land Surveys, and in the statistics of the spread of agriculture over what once was forest or prairie; and in time to come, *something*—though not, I fear, much—will exist to show what good (and as likely as not, also, what irremediable harm) this age of colonisation has done to the region as a whole. But what you do not keep record of is Nature's dealings with your immigrants; you do not *know*—and as long as you omit to *observe*, you are condemned not to know—the answer to the simple all-important question, *What kinds of men do best in Canada? What kind of men is Canada making out of the raw material which Europe is feeding into God's Mills on this side?*

Over in England, we are only too well aware how poor a lead we have given you. We, too, for a century now, have been feeding into other great winnowing chambers the raw crop of our villagers. We have created (to change the metaphor), in our vast towns, great vats of fermenting humanity, under conditions of life which at the best are unprecedented, and at their worst almost unimaginable. That is *our* great experiment in modern English anthropology—*What happens to Englishmen in City-slums?* and we shall hear, before this meeting ends, something of the methods by which we are attempting now to watch and record the outcome of that experiment in the making of the English of to-morrow. We are beginning to know, in the first place, what types of human animal can tolerate and survive the stern conditions of modern urban life. We are learning, still more slowly, what modes of life, what modified structure of the family, of the daily round, of society at large, can offer the adjustment to new needs of life, which human nature demands under this new, almost unbearable strain. We are seeing, more clear in the mass, even if hopelessly involved in detail, the same process of selection going on in the mental furniture of the individuals themselves; new views of life, new beliefs, new motives and modes of action; new, if only in the sense that they presuppose the destruction of the old.

That is our problem in human society at home. And yours, though it has a brighter side, is in its essentials the same. Geographers can tell you something already of the physical 'control' which is the setting to all possible societies on

Canadian soil. Scientific study of the vanishing remnants of the Redskin tribes may show you a little of the effects of this control, long continued, upon nations whom old Heylin held to be 'doubtless the offspring of the Tartars.' Sympathetic observation and friendly intercourse may still fill some blanks in our knowledge of their social state; how hunting or fishing—or, in rare cases, agriculture—forms and reforms men's manner institutions when it is the dominant interest in their lives. But what else economic habit have done in the past with the Redskins, the same climate and other economic habits are as surely doing with ourselves. In the struggle with Nature, as in the struggle with other men it is the weakest who go to the wall; it is the fittest who survive. And it is our business to *know*, and to record for those who come after us, what manner of men we were, when we came; whence we were drawn, and how we are distributed in this new land. An Imperial Bureau of Ethnology, which shall take for its study all citizens of our State, as such, is a dream which has filled great minds in the past and may some day find realisation. A Canadian Bureau is at the same time a nearer object, and a scheme of more practicable size. In the course of this meeting, information and proposals for such a Bureau of Ethnology are to be laid before this section by more competent authorities than I. My task has only been to show, in a preliminary way, what our science has done in the past, to stimulate political philosophy, and to determine its course and the order of its discoveries.

'Some men are borne,' said Edward Grimstone just three centuries ago, 'so farre in love with themselves, as they esteeme nothing else, and think that whatsoever fortune hath set without the compasse of their power and government should also be banished from their knowledge. Some others, a little more carefull; who finding themselves engaged by their birth, or abroad, to some one place, strive to understand how matters pass there, and remaine so tied to the consideration of their owne Commonweale, as they affect nothing else, carrying themselves as parties of that imperfect bodie, whereas in their curiositie they should behave themselves as members of this world.' It is as 'members of this world,' I hope, that we meet together to-day.

British Association for the Advancement of Science

WINNIPEG, 1909.

ADDRESS TO THE PHYSIOLOGICAL SECTION

BY

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THE PHYSIOLOGICAL BASIS OF SUCCESS.

DURING past years it has been customary for the Presidents of Sections in their addresses either to give a summary of recent investigations, in order to show the position and outlook of the branch of science appertaining to the Section, or to utilise the opportunity for a connected account of researches in which they themselves have been engaged, and can therefore speak with the authority of personal experience as well as with that imparted by the presidential Chair. The growing wealth of publications with the special function of giving summaries and surveys of the different branches of science, drawn up by men ranking as authorities in the subject of which they treat, renders such an interpretation of the presidential duties increasingly unnecessary, and the various journals which are open to every investigator make it difficult for me to give in an address anything which has not already seen the light in other forms. The Association itself, however, has undergone a corresponding modification. Founded as a medium of communication between workers in different parts of the country, it has gradually acquired the not less important significance of a tribunal from which men of science, leaving for a time their laboratories, can speak to an audience of intelligent laymen, including under this term all those who are engaged in the work of the world other than the advancement of science. These men would fain know the lessons that science has to teach in the living of the common life. By standing for a moment on the little pinnacle erected by the physicist, the chemist or the botanist, they can, or should be able to, gain new hints as to the conduct of the affairs of themselves, their town or their state. The enormous advance in the comfort and prosperity of our race during the last century has been due to the application of science, and this meeting of the Association may be regarded as an annual mission in which an attempt is made to bring the latest

results of scientific investigation into the daily routine of the life of the community.

We physiologists, as men who are laying the foundation on which medical knowledge must be built, have as our special preoccupation the study of man. Although every animal, and indeed every plant, comes within the sphere of our investigations, our main object is to obtain from such comparative study facts and principles which will enable us to elucidate the mechanism of man. In this task we view man, not as the psychologist or the historian does, by projecting into our object of study our own feelings and emotions, but by regarding him as a machine played upon by environmental events and reacting thereto in a way determined by its chemical and physical structure.

Can we not learn something of value in our common life by adopting this objective point of view and regarding man as the latest result of a continuous process of evolution which, begun in far-off ages, has formed, proved and rejected myriads of types before man himself appeared on the surface of the globe?

Adaptation.

In his study of living beings, the physiologist has one guiding principle which plays but little part in the sciences of the chemist and physicist, namely, the principle of adaptation. Adaptation or purposiveness is the leading characteristic of every one of the functions to which we devote in our text-books the chapters dealing with assimilation, respiration, movement, growth, reproduction, and even death itself. Spencer has defined life as 'the continuous adjustment of internal relations to external relations.' Every phase of activity in a living being is a sequence of some antecedent change in its environment, and is so adapted to this change as to tend to its neutralisation and so to the survival of the organism. This is what is meant by adaptation. It will be seen that not only does it involve the teleological conception that every normal activity must be for the good of the organism, but also that it must apply to *all* the relations of living beings. It must therefore be the guiding principle, not only in physiology, with its special pre-occupation with the internal relations of the *parts* of the organism, but also in the other branches of biology, which treat of the relations of the living animal to its environment and of the factors which determine its survival in the struggle for existence. Adaptation therefore must be the deciding factor in the origin of species and in the succession of the different forms of life upon this earth.

Origin of Life.

A living organism may be regarded as a highly unstable chemical system which tends to increase itself continuously under the average conditions to which it is subject, but undergoes disintegration as a result of any variation from this average. The essential condition for the survival of the organism is that any such disintegration shall result in so modifying the relation of the system to the environment that it is once more restored to the average in which assimilation can be resumed.

We may imagine that the first step in the evolution of life was taken when, during the chaotic chemical interchanges which accompanied the cooling down of the molten surface of the earth, some compound was formed, probably with absorption of heat, endowed with the property of polymerisation and of growth at the expense of surrounding material. Such a substance could continue to grow only at the expense of energy derived from the surrounding medium, and would undergo destruction with any stormy change in its environment. Out of the many such compounds which might have come into being, only such would survive in which the process of exothermic disintegration tended towards a condition of greater stability, so that the process might come to an end spontaneously and the organism or compound be enabled to await the more favour-

able conditions necessary for the continuance of its growth. With the continued cooling of the earth, the new production of endothermic compounds would probably become rarer and rarer. The beginning of life, as we know it, was possibly the formation of some complex, analogous to the present chlorophyll corpuscles, with the power of absorbing the newly penetrating sun's rays and of utilising these rays for the endothermic formation of further unstable compounds. Once given an unstable system such as we have imagined, with two phases, viz. (1) a condition of assimilation or growth by the endothermic formation of new material; (2) a condition of 'exhaustion,' in which the exothermic destructive changes excited by unfavourable external conditions came to an end spontaneously—the great principle of natural selection or survival of the fittest would suffice to account for the evolution of the ever-increasing complexity of living beings which has occurred in the later history of this globe. The adaptations, i.e., the reactions of the primitive organism to changes in its environment, must become continually more complex, for only by means of increasing variety of reaction can the stability of the system be secured within greater and greater range of external conditions. The difference between higher and lower forms is therefore merely one of complexity of reaction.

The naked protoplasm of the plasmodium of Myxomycetes, if placed upon a piece of wet blotting-paper, will crawl towards an infusion of dead leaves, or away from a solution of quinine. It is the same process of adaptation, the deciding factor in the struggle for existence, which impels the greatest thinkers of our times to spend long years of toil in the invention of the means for the offence and defence of their community or for the protection of mankind against disease and death. The same law which determines the downward growth of the root in plants is responsible for the existence to-day of all the sciences of which mankind is proud.

The difference between higher and lower forms is thus not so much qualitative as quantitative. In every case, whatever part of the living world we take as an example, we find the same apparent perfection of adaptation. Whereas, however, in the lower forms the adaptation is within strictly defined limits, with rise in type the range of adaptation steadily increases. Especially is this marked if we take those groups which stand, so to speak, at the head of their class. It is therefore important to try and find out by a study of various forms the physiological mechanism or mechanisms which determine the increased range of adaptation. By thus studying the physiological factors, which may have made for success in the struggle for dominance among the various representatives of the living world, we may obtain an insight into the factors which will make for success in the further evolution that our race is destined to undergo.

It is possible that, even at this time, objections may be raised to the application to man of conclusions derived from a study of animals lower in the scale. It has indeed been urged, on various grounds, that man is to be regarded as exempt from the natural laws which apply to all other living beings. When we inquire into the grounds for assuming this anomie, this outlawed condition of man, we generally meet with the argument that man creates his own environment and cannot therefore be considered to be in any way a product of it. This modification or creation of environment is, however, but one of the means of adaptation employed by man in common with the whole living kingdom. From the first appearance of life on the globe we find that one of the methods adopted by organisms for their self-preservation is the production of some artificial surroundings which protect them from the buffeting of environmental change. What is the mucilaginous envelope produced by micro-organisms in presence of an irritant, or the cuticle or shell secreted by the outermost cells of an animal, but the creation of such an environment? All unicellular organisms, as well as the units composing the lowest metazoa, are exposed to and have to resist every change in concentration and composition of the surrounding water. When, however, a body cavity or *coelom*, filled probably at first with sea-water, made its appearance, all the inner cells of the organism were withdrawn from the distributing influence of variations in the surrounding medium. The coelomic

fluid is renewed and maintained uniform in composition by the action of the organism itself, so that we may speak of it as an environment created by the organism. The formation of a body cavity filled with salt solution at once increased the range of adaptation of the animals endowed therewith. Thus it enabled them to leave the sea, because they carried with them the watery environment which was essential for the normal activity of their constituent cell units. The assumption of a terrestrial existence on most parts of the earth's surface involved, however, the exposure to greater ranges of temperature than was the case in the sea, and indicated the necessity for still further increase in the range of adaptation. Every vital process has its optimum temperature at which it is carried out rapidly and effectively. At or a little above freezing point the chemical processes concerned in life are suspended, so that over a wide range of the animal kingdom there must be an almost complete suspension of vital processes during the winter months, and at all times of the year a great dependence of the activity of these processes on the surrounding temperature. It is evident that a great advantage in the struggle for existence was gained by the first animals which succeeded in securing thermal as well as chemical constancy of environment for their cells, thus rendering them independent of changes in the external medium. It is interesting to note that the maintenance of the temperature of warm-blooded animals at a constant height is a function of the higher parts of the central nervous system. An animal with spinal cord alone reacts to changes of external temperature exactly like a cold-blooded animal, the activity of its chemical changes rising and falling with the temperature. In the intact mammal, by accurately balancing heat loss from the surface against heat production in the muscles, the central nervous system ensures that the body fluid which is supplied to all the active cells has a temperature which is independent of that of the surrounding medium. These are fundamental examples of adaptation effected by creation of an environment peculiar to the animal. Numberless others could be cited which differ only in degree from the activity of man himself. In some parts of this country, for instance, the activity of the beaver in creating an artificial environment has until lately been more marked than that of man himself. We are not justified, then, in regarding mankind as immune to the operation of natural forces which have determined the sequence of life on the surface of the globe. The same laws which have determined his evolution and his present position as the dominant type on the earth's surface will determine also his future destiny.

We are not, however, dealing with or interested in simple survival. Lower forms of life are probably as abundant on the surface of the globe as they were at any time in its history. Survival, as Darwin pointed out, is a question of differentiation. When in savage warfare a whole tribe is taken captive by the victorious enemy, the leaders and fighting men will be destroyed, while the slaves will continue to exist as the property of the victors. Survival, then, may be determined either by rise or by degradation of type. Success involves the idea of dominance, which can be secured only by that type which is the better endowed with the mechanisms of adaptation required in the struggle against other organisms.

Among the many forms of living matter which may have come into being in the earlier stages of the history of the earth, one form apparently became predominant and must be regarded as the ancestor of all forms of life, whether animal or vegetable, viz., the nucleated cell. The almost complete identity of the phenomena involved in cell division throughout the living kingdom indicates that all unicellular organisms and all organisms composed of cells have descended from a common ancestor, and that the mode of its reproduction has been impressed upon all its descendants throughout the millions of years which have elapsed since the type was first evolved. The universal distribution of living cells renders it practically impossible for us to test the possibility of a spontaneous abiogenesis or new formation of living from non-living matter at the present time. We cannot imagine that all the various phenomena which we associate with life were attributes of the primitive life stuff. Even if we had such stuff

at our disposal, it would be difficult to decide whether we should ascribe the possession of life to it, and there is no doubt that any such half-way material would, directly it was formed, be utilised as pabulum by the higher types of organism already abounding on the surface of the globe.

Integration and Differentiation.

An important step in the evolution of higher forms was taken when, by the aggregation of unicellular organisms, the lowest metazoon was formed. In its most primitive forms the metazoon consists simply of a cell colony, but one in which all individuals are not of equal significance. Those to the outer side of the mass, being exposed to different environmental advantages from those within, must even during the lifetime of the individual have acquired different characteristics. Moreover, the sole aim of such aggregation being to admit of co-operation by differentiation of function between the various cell units, the latter become modified according to their position, some cells becoming chiefly alimentary, others motor, and others reproductive. Co-operation and differentiation are, however, of no use without co-ordination. Each part of the organism must be in a position to be affected by changes going on in distant parts, otherwise co-operation could not be effected. This co-operation in the lowest metazoon seems to be carried out by utilisation of the sensibility to chemical stimuli already possessed by the unicellular organism. We have thus co-ordination by means of chemical substances ('hormones') produced in certain cells and carried thence by the tissue fluids to other cells of the body, a mechanism of communication which we find even in the highest animals, including man himself. To such chemical stimuli we may probably ascribe the accumulation of wandering mesoderm cells—i.e., phagocytes—in an organism such as a sponge, around a seat of injury or any foreign substance that has been introduced. By this mechanism it is possible for distant parts of the body to react to stimulation of any one part of the surface. Communication by this means is, however, slow, and may be compared to the state of affairs in civilised countries before the invention of the telegraph, when messengers had to ride to different parts of the kingdom in order to arouse the whole nation for defence or attack.

Foresight and Control.

Increased speed of reaction and therefore increased powers in the struggle for existence were obtained when a nervous system was formed, by a modification of the cells forming the outer surface of the organism. By the growth of long processes from these cells a conducting network was provided, running through all parts of the body and affording a channel for the rapid propagation of excitation from the surface to the deeper parts, as well as from one part of the surface to another. From this same layer were produced the cells which, as muscle fibres, would act as the motive mechanism of the organism. Thus, from the beginning, the chief means of attack or escape were laid down in close connection with the surface from which the stimuli were received. A further step in the evolution of the nervous system consisted in the withdrawal of certain of the sensory or receptor cells from the surface, so that a specially irritable organ, the central nervous system, was evolved, which could serve as a distributing centre for the messages or calls to action initiated by changes occurring at the surface of the body. At its first appearance this central nervous system would hardly deserve the epithet of 'central,' since it formed a layer lying some distance below the surface, and extending over a considerable area; though we find that very soon there is an aggregation of the special cells to form ganglia, each of which might be regarded as presiding over the reactions of that part of the animal in which it is situated. Thus in the segmental wormlike animals a pair of ganglia

is present in each body segment, and the chain of ganglia are united by longitudinal strands of nerve fibres to form the ganglionated cord, or central nervous system.

Such a diffused nervous system, in which all ganglia were of equal value, could, however, only act for the common weal of the whole body when a reaction initiated by stimulation at one part was not counteracted by an opposing reaction excited from another part of the surface. For survival it is necessary that in the presence of danger, *i.e.*, an environment threatening the life of the individual or race, the whole activities of the organism should be concentrated on the one common purpose, whether of escape or defence. This could be effected only by making one part of the central nervous system predominant over all other parts, and the part which was chosen for this predominance was the part situated in the neighbourhood of the mouth. This, in animals which move about, is the part which always precedes the rest of the body, and therefore the part which first experiences the sense impressions, favourable or dangerous, arising from the environment. It is this end that has to appreciate the presence or approach of food material, as well as the nature of the medium into which the animal is being driven by the movements of its body. Thus a predominance of the front end of the nervous system was determined by the special development at this end of those sense organs or sensory cells which are *projicient*—*i.e.* are stimulated by changes in the environment proceeding from disturbances at a distance from the animal. The sensory organs of vision and the organs which correspond to our olfactory sense organs and are aroused by minute changes in chemical composition of the surrounding medium, are always found especially at the front or mouth end of the organism. The chances of an animal in the struggle for existence are determined by the degree to which the responses of the animal to the *immediate* environment are held in check in consequence of stimuli arising from *approaching* events. The animal, without power to see or smell or hear its enemy, will receive no impulse to fly until it is already within its enemy's jaws. It must therefore be an advantage to any animal that the whole of its nervous system should be subservient to those ganglia or central collections of nerve cells which are in direct connection with the projicient sense organs in the head. This subservience is secured by endowing the head centre with a power, firstly, of controlling and abolishing the activities (*i.e.*, all those aroused by external stimuli) of all other parts of the central nervous system, and, secondly, of arousing these parts to a reaction immediately determined by the impression received from the projicient sense organs of the head and originated by some change in the surroundings of the animal which has not yet affected the actual surface of its body.

Education by Experience.

The factors which so far determine success in the struggle for predominance are, in the first place, foresight and power to react to coming events, and, in the second place, control of the whole activities of the organism by that part of the central nervous system which presides over the reaction. The animal therefore profits most which can subordinate the impulses of the present to the exigencies of the future.

An organism thus endowed is still, however, in the range of its reactions, a long way behind the type which has attained dominance to-day. The machinery we have described, when present in its simplest form, suffices for the carrying out of reactions or adaptations which are determined immediately by sense impressions, advantage being given to those reactions which are initiated by afferent stimuli affecting the projicient sense organs at the head end of the animal. With the formation of the vertebrate type, and probably even before, a new faculty makes its appearance. Up to this point the reactions of an animal have been what is termed 'fatal,' not in the sense of bringing death to the animal, but as inexorably fixed by the structure of the nervous system inherited by the animal from its precursors. Thus it is of advantage to a moth that it should be

attracted by, and fly towards light objects—*e.g.*, white flowers—and such a reactivity is a function of the structure of its nervous system. When the light object happens to be a candle flame the same response takes place. The first time that the moth flies into and through the candle flame, it may only be scorched. It does not, however, learn wisdom, but the reaction is repeated so long as the moth can receive the light stimuli, so that the response, which in the average of cases is for the good of the race, destroys the individual under an environment which is different from that under which it was evolved. There is in this case no possibility of educating the individual. The race has to be educated to new conditions by the ruthless destruction of millions of individuals, until only those survive and impress their stamp on future generations whose machinery, by the accumulation and selection of minute variations, has undergone sufficient modification to determine their automatic and 'fatal' avoidance of the harmful stimulus.

The next great step in the evolution of our race was the modification of the nervous system which should render possible the education of the individual. The mechanism for this educatability was supplied by the addition, to the controlling sensory ganglia of the head, of a mass of nervous matter which could act, so to speak, as an accessory circuit to the various reflex paths already existing in the original collection of nerve ganglia. This accessory circuit, or upper brain, comes to act as an organ of memory. Without it a child might, like the moth, be attracted by a candle flame and approach it with its hand. The injury ensuing on contact with the flame would inhibit the first movement and cause a drawing back of the hand. In the simple reflex mechanism there is no reason why the same series of events should not be repeated indefinitely, as in the case of the moth. The central nervous system, however, is so constituted that every passage of an impulse along any given channel makes it easier for subsequent impulses to follow the same path. In the new nerve centre, which presents a derived circuit for all impulses traversing the lower centres, the response to the attractive impulse of the flame is succeeded immediately by the strong inhibitory impulses set up by the pain of the burn. Painful impressions are always predominant. Since they are harmful, the continued existence of the animal depends on the reaction caused by such impressions taking the precedence of and inhibiting all others. The effect therefore of such a painful experience on the new upper brain must far outweigh that of the previous impulse of attraction. The next time that a similar attractive impression is experienced the derived impulse traversing the upper brain arouses, not the previous primary reaction, but the secondary one, *viz.*, that determined by the painful impressions attending contact with the flame. As a result, the whole of the lower tracts, along which the primary reaction would have travelled, are blocked, and the reaction—now an educated one—consists in withdrawal from or avoidance of the formerly attractive object. The burnt child has learnt to dread the fire.

The upper brain represents a nerve mechanism without distinct paths, or rather with numberless paths presenting at first equal resistance in the various directions. As a result of experience, definite tracts are laid down in this system, so that the individual has the advantage not only of his lower reflex machinery for reaction, but also of a machinery which with advance in life is adapted more and more to the environment in which he happens to be. This educable part of the nervous system—*i.e.*, the one in which the direction of impulses depends on past experience and on habit—is represented in vertebrates by the cerebral hemispheres. From their first appearance they increase steadily in size as we ascend the animal scale, until in man they exceed by many times in bulk the whole of the rest of the nervous system.

We have thus, laid down automatically, increased power of foresight, founded on the Law of Uniformity. The candle flame injures the skin once when the finger is brought in contact with it. We assume that the same result will follow each time that this operation is repeated. This uniformity is also assumed in the growth of the central nervous system and furnishes the basis on which the

nerve paths in the brain are laid down. The one act of injury which has followed the first trial of contact suffices in most cases to inhibit and to prevent any subsequent repetition of the act.

The Faculty of Speech.

If we consider for a moment the vastness and complexity of the stream of impressions which must be constantly pouring into the central nervous system from all the sense organs of the body, and the fact that, at any rate in the growing animal, every one of these impulses is, so to speak, stored in the upper brain, and affects the whole future behaviour of the animal, even the millions of nerve cells and fibres which are to be found in the human nervous system would seem to be insufficient to carry out the task thrown upon them. Further development of the adaptive powers of the animal would probably have been rendered impossible by the very exigencies of space and nutrition, had it not been for the development of the power of speech. A word is a fairly simple motor act and produces a correspondingly simple sensory impression. Every word, however, is a shorthand expression of a vast sum of experience, and by using words as counters it becomes possible to increase enormously the power of the nervous system to deal with its own experience. Education now involves the learning of these counters and of their significance in sense experience; and the reactions of the highest animal, man, are for the most part carried out in response to words and are governed by past education of the experience-content involved in each word.

The power of speech was probably developed in the first place as a means of communication among primitive man living in groups or societies; as a means, that is to say, of procuring co-operation of different individuals in a task in which the survival of the whole race was involved. But it has attained still further significance. Without speech the individual can profit by his own experience and to a certain limited extent by the control exercised by the older and more experienced members of his tribe. As soon as experience can be symbolised in words, it can be dissociated from the individual and becomes a part of the common heritage of the race, so that the whole past experience of the race can be utilised in the education—i.e., the laying down of nerve tracts—in the individual himself. On the other hand, the community receives the advantage of the foresight possessed by any individual who happens to be endowed with a central nervous system which transcends that of his fellows in its powers of dealing with sense impressions or other symbols. The foresight thus acquired by the whole community must be of advantage to it and serve for its preservation. It is therefore natural that in the processes of development and division of labour, which occur among the members of a community just as among the cell units composing an animal, a class of individuals should have been developed, who are separated from the ordinary avocations, and are, or should be, maintained by the community, in order that they may apply their whole energies to the study of sequences of sense impressions. These are set into words which, as summary statements of sequence, are known to us as the Laws of Nature. These natural laws become the property of the whole community, become embodied by education into the nervous system of its individuals, and serve therefore as the experience which will determine the future behaviour of its constituent units. This study of the sequence of phenomena is the office of Science. Through Science the whole race thus becomes endowed with a foresight which may extend far beyond contemporary events and may include in its horizon not only the individual life, but that of the race itself as of races to come.

Social Conduct.

I have spoken as if every act of the animal were determined by the complex interaction of nervous processes whose paths through the higher parts of the brain had been laid down by previous experience, whether of phenomena or of words as symbolical of phenomena. The average conduct, however, of the individual, determined at first in this way, became by repetition automatic—i.e., the nerve paths are so facilitated by frequent use that a given impulse can take only the direction which is set by custom. The general adoption of the same line of conduct by all the individuals of a community in face of a given condition of the environment gave in most cases an advantage to those individuals who were endowed with a nervous system of such a character that the path could be laid down quickly and with very little repetition. Thus we get a tendency, partly by selection, largely by education, to the establishment of reactions which, like the instincts of animals, are almost automatic in character. As MacDougall has pointed out, the representations in consciousness of automatic tendencies are the emotions. Moral conduct, being that behaviour which is adapted to the individual's position in his community, is largely determined by these paths of automatic action, and the moral individual is he whose automatic actions and consequent emotions are most in accord with the welfare of his community, or at any rate with what has been accepted as the rule of conduct for the community.

Rise in Type dependent on Brain.

Thus, in the evolution of the higher from the lower type, the physiological mechanisms, which have proved the decisive factors, can be summed up under the headings of integration, foresight and control. In the process of integration we have not only a combination of units previously discrete, but also differentiation of structure and function among the units. They have lost, to a large extent, their previous independence of action and, indeed, power of independent action, the whole of their energies being now applied to fulfilling their part in the common work of the organism. At first bound together by but slight ties and capable in many cases of separating to form new cell colonies, they have finally arrived at a condition in which each one is absolutely dependent for its existence on its connection with the rest of the organism and is also essential to the well-being of every other part of the organism.

This solidarity, this subjection of all selfish activity to a common end, namely, preservation of the organism, could only be effected by a gradual increase in the control of all parts by one master tissue of the body, whose actions were determined by impulses arriving from sense organs which themselves were set into activity by coming events. We thus have with the rise in type a gradually rising scale in powers of foresight, in control by the central nervous system, and in the solidarity of the units of which the organism is composed.

In the struggle for existence the rise in type has depended therefore on the central nervous system and its servants. Rise in type implies increased range of adaptation, and we have seen that this increased range, from the very beginning of a nervous system, was bound up with the powers of this system. Whatever opinion we may finally arrive at with regard to the types of animals which we may claim as our ancestors on the line of descent, there can be no doubt that Gaskell is right in the fundamental idea which has guided his investigations into the origin of vertebrates. As he says, 'the law for the whole animal kingdom is the same as for the individual. Success in this world depends upon brains.' The work by this observer which has lately appeared sets forth in greater detail than I have been able to give you to-day the grounds on which this assertion is based, and furnishes one of the most noteworthy contributions to the principles of evolution which have been published during recent years.

We must not, however, give too restrictive or common a meaning to the expression 'brains' used by Gaskell in the dictum quoted above. By this word we imply the whole reactive system of the animal. In the case of man, as of some other animals, his behaviour depends not merely on his intellectual qualities or powers, to which the term 'brain' is often in popular language confined, but on his position as a member of a group or society. His automatic activities in response to his ordinary environment, all those social acts which we ascribe in ourselves to our emotions or conscience, are determined by the existence of tracts in the higher parts of his brain, access to which has been opened by the ruthless method of natural selection and which have been deepened and broadened under the influence of the pleasurable and painful impressions which are included in the process of education. All the higher development of man is bound up with his existence as a member of a community, and in trying to find out the factors which will determine the survival of any type of man, we must give our attention, not to the man, but to the tribe or community of which he is a member, and must try to find out what kind of behaviour of the tribe will lead to its predominance in the struggle for existence.

Political Evolution.

The comparison of the body politic with the human body is as old as political economy itself, and there is indeed no reason for assuming that the principles which determine the success of the animals formed by the aggregation of unicellular organisms should not apply to the greater aggregations or communities of the multicellular organisms themselves. It must be remembered, however, that the principles to which I have drawn your attention are not those that determine survival, but those which determine rise of type, what I have called success. Evolution may be regressive as well as progressive. Degeneration, as Lankester has shown, may play as great a part as evolution of higher forms in determining survival. The world still contains myriads of unicellular organisms as well as animals and plants of all degrees and complexity and of rank in the scale of life. All these forms are subordinate to man, and when in contact with him are made to serve his purposes. In the same way all mankind will not rise in type. Many races will die out, especially those who just fall short of the highest type, while others by degradation or differentiation may continue to exist as parasites or servants of the higher type.

Mere association into a community is not sufficient to ensure success; there must also be differentiation of function among the parts, and an entire subordination of the activity of each part to the welfare of the whole. It is this lesson which we English-speaking races have at the present time most need to learn. In the behaviour of man almost every act is represented in consciousness as some emotion, experience or desire. The state of subordination of the activities of all units to the common weal of the community has its counterpart in consciousness as the 'spirit of service.' The enormous value of such a condition of solidarity among the individuals constituting a nation, inspired, as we should say, by this spirit of service, has been shown to us lately by Japan. In our own case the subordination of individual to state interests, such as is necessary for the aggregation of smaller primitive into larger and more complex communities, has always presented considerable difficulty and been accomplished only after severe struggle. Thus the work begun by Alexander Hamilton and Washington, the creation of the United States, is still, even after the unifying process of a civil war, incomplete and marred by contending state and individual interests. The same sort of difficulties are being experienced in the integration of the units, nominally under British control, into one great nation, in which all parts shall work for the good of the whole and for mutual protection in the struggle for survival.

The Lesson of Evolution.

Just as pain is the great educator of the individual and is responsible for the laying down of the nervous paths, which will determine his whole future conduct and the control of his lower by his higher centres, so hardship has acted as the integrator of nations. It is possible that some such factor with its attendant risks of extermination may still be necessary before we attain the unification of the British Empire, which would seem to be a necessary condition for its future success. But if only our countrymen can read the lesson of evolution and are endowed with sufficient foresight, there is no reason why they should not, by associating themselves into a great community, avoid the lesson of the rod. Such a community, if imbued by a spirit of service and guided by exact knowledge, might be successful above all others. In this community not only must there be subordination of individual to communal interests, but the behaviour of the community as a whole must be determined by anticipation of events—*i.e.* by the systematised knowledge which we call Science. The universities of a nation must be like the eyes of an animal, and the messages that these universities have to deliver must serve for the guidance and direction of the whole community.

This does not imply that the scientific men, who compose the universities and are the sense organs of the community, should be also the rulers. The reactions of a man or of a higher mammal are not determined immediately by impulses coming from his eyes or ears, but are guided by these in association with, and after they have been weighed against, a rich web of past experience, the organ of which is the higher brain. It is this organ which, as the statesman of the cell community, exercises absolute control. And it is well that those who predicate an absolute equality or identity among all the units of a community should remember that, although all parts of the body are active and have their part to play in the common work, there is a hierarchy in the tissues—different grades in their value and in their conditions. Thus every nutritional mechanism of the body is subordinate to the needs of the guiding cells of the brain. If an animal be starved, its tissues waste; first its fat goes, then its muscles, then its skeletal structures, finally even the heart. The brain is supplied with oxygen and nourishment up to the last. When this, too, fails, the animal dies. The leading cells have first call on the resources of the body. Their needs, however, are soon satisfied, and the actual amount of food or oxygen used by them is insignificant as compared with the greedy demands of a working muscle or gland cell. In like manner every community, if it is to succeed, must be governed, and all its resources controlled by men with foreseeing power and rich experience—*i.e.*, with the wisdom that will enable them to profit by the teachings of science, so that every part of the organism may be put into such a condition as to do its optimum of work for the community as a whole.

At the present time it seems to me that, although it is the fashion to acquiesce in evolution because it is accepted by biologists, we do not sufficiently realise the importance of this principle in our daily life, or its value as a guide to conduct and policy. It is probable that this doctrine had more influence on the behaviour of thinking men in the period of storm and controversy which followed its promulgation fifty years ago, than it has at the present day of lukewarm emotions and second-hand opinions. Yet, according to their agreement with biological laws, the political theories of to-day must stand or fall. It is true that in most of them the doctrine of evolution is invoked as supporting one or other of their chief tenets. The socialist has grasped the all-importance of the spirit of service, of the subordination of the individual to the community. The aristocrat, in theory at any rate, would emphasise the necessity of placing the ruling power in the hands of the individuals most highly endowed with

intelligence and with experience in the affairs of nations. He also appreciates the necessity of complete control of all parts by the central government, though in many cases the sense organs which he uses for guidance are the traditions of past experience rather than the science of to-day. The liberal or individualist asserts the necessity of giving to each individual equal opportunities, so that there may be a free fight between all individuals in which only the most highly gifted will survive. It might be possible for another Darwin to give us a politic which would combine what is true in each of these rival theories, and would be in strict accord with our knowledge of the history of the race and of mankind. As a matter of fact the affairs of our states are not determined according to any of these theories, but by politicians, whose measures for the conduct of the community depend in the last resort on the suffrages of their electors—i.e., on the favour of the people as a whole. It has been rightly said that every nation has the government which it deserves. Hence it is all-important that the people themselves should realise the meaning of the message which Darwin delivered fifty years ago. On the choice of the people, not of its politicians, on its power to foresee and to realise the laws which determine success in the struggle for existence, depends the future of our race. It is the people that must elect men as rulers in virtue of their wisdom rather than of their promises. It is the people that must insist on the provision of the organs of foresight, the workshops of exact knowledge. It is the individual who must be prepared to give up his own freedom and ease for the welfare of the community.

Whether our type is the one that will give birth to the super-man it is impossible to foresee. There are, however, two alternatives before us. As incoherent units we may acquiesce in an existence subordinate to or parasitic on any type which may happen to achieve success, or as members of a great organised community we may make a bid for determining the future of the world and for securing the dominance of our race, our thoughts and ideals.

British Association for the Advancement of Science.

WINNIPEG, 1909.

ADDRESS

TO THE

BOTANICAL SECTION

BY

LIEUT.-COLONEL DAVID PRAIN, C.I.E., LL.D., F.R.S.,

PRESIDENT OF THE SECTION.

Sutor ne supra crepidam judicaret, probably an old saying when Pliny wrote, is still a safe guide. The limitations of life and of knowledge are different, and human effort is thereby so conditioned that progress depends on specialisation in study. Specialisation lessens the temptation to forget this caution; but the force of the proverb is not weakened. It also conveys a behest, and compliance with this behest helps to counteract the narrowed outlook which specialisation sometimes encourages.

Those whose studies are confined to some limited field often welcome a sketch of the aims and methods of work with which they are not familiar. Such a sketch may be held to have served its purpose if the subject discussed, and its relationship to cognate studies, be rendered intelligible.

No apology, therefore, is made for the subject now taken up, even if it be sometimes hinted that this subject—Systematic Botany—is inimical to originality, the antithesis of scientific, and outside the limits of botany proper. These views depend on half-truths and arbitrary connotations. They do not affect the fact that the primary purpose of systematic study is to advance natural knowledge. The systematic worker, in furthering this object, does not halt to consider whether his work be applied rather than original, technical rather than scientific.

As a matter of history, the scope of systematic study practically coincides with what botany once implied; as a matter of fact, it corresponds to what zoology implies now. The accident that man, on his physical side, is like the beasts that perish has led to the recognition of animal physiology and anatomy as independent sciences. Owing to the absence of any such fortuitous circumstance vegetable anatomy and physiology remain under the ancestral roof. These off-shoots of botany are as vigorous as their zoological counterparts. They may be entitled to think that systematic methods are old-fashioned, and it may be desirable that they should set up separate establishments or form alliances with the corresponding off-shoots of zoology. But nothing in all this justifies the eviction of systematic botany from the family home.

The statement that systematic methods are old-fashioned may be accepted without conceding that these methods are out of date. Systematic work, while

sharing in the general advance in knowledge, has been able, amid far-reaching changes, to maintain continuity of method in the pursuit of its double purpose. This has been a benefit to botany as a whole when crucial discoveries or illuminating theories have, in other fields, led to a reorientation of view requiring the use of fresh tablets for the record of new results.

Disintegration and readjustment due to altered outlook are familiar processes. Histology, parting company with organography to serve physiology, is now an independent study, one of whose branches occasionally declines to accept any doctrine unconfirmed by cytological methods. The study of problems relating to nutrition and reproduction has been considered the especial task of physiology. Now, the chemist at times claims the problems of nutrition as part of his field, and we look for advances in our knowledge of reproductive problems to the cytologist and the student of genetics. These instances are adduced from without because relative exemption from disintegration is a distinctive feature of systematic study. The two-sided task of the systematist is to provide a census of the known forms of plant life and to explain the relationships of these forms to each other. The work on one side is mainly descriptive, on the other mainly taxonomic, but the two are so interdependent, and their operations so intimately blended, that it is difficult to treat them apart. Reorientation in botanical study has led to seismic disturbances in the taxonomic field, but the materials supplied by descriptive work have remained unaffected, and therefore have been ready for use in the repair or reconstruction of shattered 'systems.'

The exemption from radical change in method, which marks systematic work, is due to those characteristics that expose it to the charges of discouraging originality and of calling only for technical skill. It also largely explains why systematic study, especially on the descriptive side, is not attractive to minds disposed towards experimental inquiry. The labour involved is as exacting, accurate record and balanced judgment are as necessary, in descriptive as in experimental research. 'A skill that is not to be acquired by random study at spare moments' is as essential in descriptive as in other work, while the relief that variation in method affords is precluded. Increased experience, here as elsewhere, leads to more satisfactory results, but without, in this case, mitigating the toil of securing them. The testing of theories, often an inspiring task in experimental research, in the descriptive field retards progress. But if in descriptive work imagination and the spirit of adventure are undesirable, these qualities are not inhibited by systematic study as a whole. Imagination is legitimate and useful in the taxonomic field, and in another line of activity—the acquisition of the material on which descriptive work is based—the spirit of adventure is essential to success.

The untravelling descriptive worker is not without consolations. His work is as necessary to botany as that of the cartographer to geography, or the grammarian to literature. His results are means to the ends that others have in view. If these results often appeal to coming rather than to contemporary workers, the descriptive writer is at least largely spared the doubtful benefit of immediate appreciation. He can pursue his studies unaffected by any considerations save those of adding to the sum of human knowledge and of bringing a necessary task appreciably nearer completion. In descriptive study it is the work rather than the personality of the worker that tells. Yet the work is not without human interest, because systematic writings rarely fail to reflect the character of the writers. The intimate knowledge of descriptive treatises, which floristic or monographic study entails, usually leads to mental estimates of the actual authors. The evidence on which these estimates depend is unwittingly given and unconsciously appreciated. But its value is not thereby diminished, and estimates so formed may prove useful checks on contemporary judgments.

The descriptive worker as a rule makes his work 'the primary business of his life, which he studies and practises as if nothing else in the world mattered.' But he does not hold aloof from those engaged in other lines of botanical activity. His evidence is mainly obtained from organography and organogeny; but, just because his results are for the use of others, the descriptive botanist has to keep abreast of all that is done in every branch of his science. New weapons are constantly being forged, and not in morphological workshops only; with these and their uses the descriptive worker must be familiar, for the need to employ them may arise at any moment. If he does not always abandon old friends for new, this is not because the systematist is unaware of their existence, or unprepared to apply new methods. The descriptive worker employs his tools as a craftsman; like other craftsmen, he finds that tools do not always fulfil the hopes of their designers. In descriptive work, too, as elsewhere, a steam hammer is not required to break every nut; the staff and sling may be arms as effective as those of the hoplite. There are occasions when the descriptive writer does appear to hold aloof by declining to accept proffered evidence. But his motive is not arrogant; it is only altruistic. If he is to avoid the risk of causing those who depend on his results to reason in a circle, the descriptive writer must obtain these results, if not without extraneous aid, at least without help from those for whose immediate use they are provided.

Taxonomic study is pursued in an environment which differs from that surrounding descriptive work. The descriptive student can hardly see the wood for its trees. The taxonomic student works in more open country, and can look on the wood as a whole. He has, too, the benefit of companionship. The palæobotanist meets him, with all the lore of mine and quarry, as one ready to exchange counsel; other workers attend to give or gather information.

The community of interest which unites the systematic worker, chiefly concerned with existing plant-types, and the palæobotanist, primarily interested in types now extinct, is strengthened by the bond which identity of purpose supplies. But the two are differently circumstanced; the systematic worker is ordinarily better acquainted with the characters than with the relationships of his types; the palæobotanist usually knows more of the relationships of his types than he does, or ever may do, of their characters. The material of the palæobotanist rarely lets him rely on ordinary descriptive methods in defining his plants; he has to depend largely on anatomical evidence, which supplements and confirms, but hardly replaces, the data of organography. On the taxonomic side the palæobotanist is restricted to phylogenetic methods; here again he is handicapped, though less than on the descriptive side, by the fragmentary character of his specimens. The palæobotanist hardly does more than the phylogenist, hardly as much as the anatomist, towards advancing the object all have in common.

The same community of interest unites in their labours the organographic systematist and the morphologist whose interests are phylogenetic. Here, however, though the task of the two be complementary, the mode of attack is so different as almost to mask their identity of purpose. The comparative morphologist studies the planes of cleavage indicated by salient differences in structure and development. The system he evolves is composed of the entities, sometimes more or less subjective, that combinations of characters suggest. The method in intention, and largely in effect, passes from the general to the more particular, though the process is tempered by the fact that the characters used are derived from such types as exhibit them. The organographic systematist, after summing up the characters which mark individual types, aggregates these according to their kinds. Having estimated the features that characterise individual kinds, he aggregates these according to their families. Families are thereafter aggregated in higher groups, and these groups are

TRANSACTIONS OF SECTION K.

subjected to further aggregation. The system thus evolved is composed of those entities, always in theory objective, that successive aggregations indicate, and the process is one of constantly widening generalisation.

The comparative morphologist, though glad when his results can be practically applied, follows truth for its own sake. His work is thus on a higher plane than that of the organographic systematist, whose aggregations are primarily utilitarian. But the work of the latter is not less valuable because its scientific character is incidental. Were our knowledge of plant-types exhaustive, a generally accepted artificial arrangement of these would be as useful to the applied botanist as a professedly natural one. But our knowledge is incomplete, and the accession and intercalation of new types renders any artificial, and most attempts at a natural, system sooner or later unworkable. The more closely an arrangement approximates to the natural system, the less can the intercalation of new forms affect its stability. The more stable a system is, the more easily will its details be remembered and the more useful will it prove in practical reference work. Here, therefore, for once, self-interest and love of truth go hand in hand.

Since the organographic systematist learns their characters from his groups, while the comparative morphologist defines groups by the characters he selects, their results, were knowledge complete, should be identical, and this identity should prove their accuracy. But knowledge is finite, and these results are not always uniform. The want of uniformity is, however, often exaggerated because the reasons are not always appreciated.

One cause is the difference in personal equation, which affects alike the worker who deals with things and him who considers attributes. It would be contrary to expectation were every phylogenist to assign the same value to each character, or every systematist to apply the same limitation to each type or group of types. The divergence of view on the part of two observers may show a small initial angle; it may nevertheless lead them to positions far apart. But while divergence of view is the most obvious explanation of the want of uniformity apparent in systematic results, it is the least effective cause. This inherent tendency to differ manifests itself in contrary directions; in the long run individual variations are apt to cancel each other.

The nature of the work counts for more than the predisposition of the worker. The aggregations on organographic lines, which were the main guides to the composition of the higher groups until phylogenetic study was seriously undertaken, do not assist the comparative morphologist. The characters on which phylogenetic conclusions may be based increase in value in proportion to the width of their incidence, so that the greater their value for phylogenetic purposes the less do they aid the descriptive worker in discriminating between one plant-type and another. Often they are characters which for practical reasons the descriptive worker must avoid. Organography, then, may not give evidence as to characters whereof cognisance cannot be taken, while for another reason the comparative morphologist may not use characters derived from descriptive sources. The object of the phylogenist is to take his share in advancing our knowledge of taxonomy; to seek from the systematist the evidence on which his results are based would be to vitiate the reasoning of both. All that the phylogenist can ask the descriptive worker to do is to supply the units that require classification.

The comparative morphologist, relying mainly on anatomical and embryological evidence, at first had a hope that his method of study might enable him to supply his own units and thereby render further taxonomic work based on organography unnecessary. This hope remains unfulfilled, and the phylogenist, as a rule, limits his efforts to a narrower field. The organographic systematist realises that in the present state of our knowledge the study of the incidence of selected characters gives more satisfactory results as

regards the composition of the higher phyla than repeated aggregation can attain, while the comparative morphologist recognises that, as matters stand, the approximations of organography in respect of types and kinds are more satisfactory than any he can yet offer. Since, however, the progress in one case is outwards, in the other the reverse, a zone of contact is inevitable. This zone, in which the influence of both methods of study is felt, is occupied by those groups immediately higher in value than the natural families of plants, and it is here that discrepancies in the results attained chiefly manifest themselves. These discrepancies take the form of unavoidable differences of opinion as regards the composition of collections of natural families. If a family A possesses ten characters of ordinal import, whereof it shares eight with a family B and only two with a family C, while the characters combined in A are, as regards B and C, mutually exclusive, the organographic systematist is ordinarily induced to group A and B together and to exclude C from that particular aggregation of families. If, on the other hand, the phylogenist finds that the two characters common to the families A and C are met with in other families, D, E, F, he will ordinarily be led to place A, C, D, E, F in the same higher group from which the family B, notwithstanding its greater general agreement with A than any of the others, must be excluded. This source of discrepancy is, however, less potent than might be expected. When the evidence advanced by either is very strong, the other worker readily accepts it; in doubtful cases mutual accommodation takes place, the one worker limiting his groups, the other applying his criteria with less rigidity.

The healthy disregard for formal consistency which admits of adjustments to further practical ends does not, however, alter the fact that a system thus attained can only approximate to the natural arrangement at which both workers aim. Gaps in knowledge may be bridged with histological or teratological aid, or safely crossed with the help of some sudden intuition or happy speculation. But the existence of anomalous types and groups serves as a reminder that much has yet to be learned with regard to living types, while the widest gap in our knowledge of these is a fissure as compared with the chasms that confront the palæontologist. In this the taxonomist of either type finds the incentive to further effort.

The automatic adjustment of differences due to idiosyncrasy, and the mutual accommodation of those arising from method of work, still leave considerable want of harmony in taxonomic results to be accounted for. What appear to be rival systems of classification compete for recognition. As each such system professes to be the nearest attainable approximation to the natural arrangement, the evidence of a state of dissension and confusion in the taxonomic field appears to those unfamiliar with systematic work to be incontrovertible. Dissension may be admitted; confusion there is none. Pictures of the same subject by different artists may be very unlike, yet equally true; what appear to be rival systems are only manifestations of one.

It is not difficult to form a conception of this system; it is less easy to share the conception with others. Let us imagine a closed space approximately spherical in shape, its surface studded with symbols that mark the relative positions of existing plant-types. Let us imagine the lines of descent of all these types to have been definitely traced and effectively mapped. We find, starting from near the centre of our sphere, a radiating system of lines; we find these lines to be subject to repeated dichotomy and embranchment which may take place at any point; we find the resultant lines departing from the original direction at any angle and in any plane; we find the *nodus* of any individual dichotomy or embranchment capable of serving as the focus of origin for a subsidiary system comparable in everything except age with the centre of our sphere, and conceivably exceeding in the multiplicity of its ramifications the primary system itself. Some only of our lines reach the symbols

that stud our spherical surface, though every symbol is the terminal of some such line. Here a terminal is fairly isolated, and the line it limits goes far towards the centre with little or no dichotomy or embranchment. Elsewhere our terminals are closely set, the lines they limit running inwards in company till some proximate *nodus* is reached. Moreover, within our sphere, in the abrupt terminals of various lines we can dimly trace the vestiges of other spheres, not always concentric with our present sphere, once studded with symbols marking the existence of types now extinct. Imagine further the centre of our hypothetical space as not necessarily a primary centre, but merely the *nodus* of some dichotomy or embranchment in a system of which ours is but a residuary fragment.

As we are practically limited to superficial delineation, an intelligible picture of our system is more than the science of perspective and the art of chiaroscuro can be asked to provide. What is unattainable on the flat is still more impossible in sequence. Serial presentation involves a point of departure; convenience, predilection, hazard, may dictate what this shall be, and determine the sequence adopted. The result is not a variety of systems, but a series of variants of one system. Considering how complex the problem is, the number of variants is remarkably small. In any case the differences met with are inconsequent; they do not affect the facts, and the facts alone really count. The trained taxonomist knows that no serial disposition can indicate, even vaguely, the relative position and import of all these facts. Plane presentation, though more adequate than serial by a dimension, falls short of accuracy; the surface on which the bulk of the facts may be displayed can have no lateral boundary. Even if its presentation on a globe be attempted, the diagram must be incomplete; many of the points to be shown lie beneath the surface. Convention might overcome the difficulty involved in the indication of extinct types, but the diagram would still fail by a dimension to demonstrate the descent of the forms superficially represented.

Intercourse with the phylogenist, while directly influencing the relationship of the organographic systematist to taxonomy, has indirectly modified his attitude towards the diagnosis and limitation of plant-types. Taxonomic study based on evidence other than descriptive has stimulated histological research and fired the anatomist with an ambition to replace by his methods those of organography. It is certainly not for want of industry or care that the success of the phylogenist in the taxonomic field has not also attended the diagnostic work of the anatomist. This failure to replace organographic by anatomical methods is due to the fact that the qualities which make histological evidence useful in generalisation lessen its value in discrimination. That anatomical characters may be of great use even in diagnosis has been less fully appreciated than it might by those habituated to organographic methods. On the other hand, anatomists who have not benefited by an apprenticeship in descriptive study at times overlook the fact that the value of histological evidence in diagnostic work is indirect. Codification of the scattered results of systematic anatomy has now shown the descriptive worker how useful histological methods are when skilfully and properly used, and has at the same time made it apparent to the anatomist that, in respect of grades lower than ordinal, his methods are more fitted for proof than for demonstration. Their alliance is now cordial and complete.

While descriptive and anatomical study conjointly make for accurate discrimination, opinion and circumstance combine to prevent uniform delimitation of plant-forms or 'species,' and no conceivable compromise can overcome this difficulty. With the term 'species' is bound up a double controversy—what idea the word conveys, and what entity the word connotes. Into the first we need not enter; we must assume that our ideas are sufficiently uniform to render the term intelligible. The second we cannot take up here;

we must accept the position as we find it, and note, in a spirit of detachment, how in actual practice the systematic botanist does delimit his 'species.' In doing this we have to discriminate between the effect which observed facts produce on different minds, and that which different mental states produce on the records of facts. The results obtained may be essentially identical though arrived at in different ways; as, however, the results are not always uniform, the existence and effect of these two factors must be carefully noted.

It is rather unusual to find that workers whose powers of observation are equal take precisely the same view of every member of a group of nearly allied forms. One, from predisposition or accident, is influenced rather by the characters whereby the forms differ; another is more impressed by those wherein they agree. In monographic work especially the same worker may find himself alternately more alive to the affinities and more struck by the discrepancies among related forms. At one time he feels that his difficulties may be best solved by recognising all these forms as distinct, at another he inclines to the view that they may be but states of one protean species. Where the capacity for detecting differences is naturally strong, the disposition is towards segregation; where there is a keen eye for affinities, the reverse. The facts in both cases are the same; their influence on minds in which the faculty of observation, though equally developed, has a natural bias in a particular direction may thus be different.

This inherent variation in mental quality, of which the observer may personally be unaware, and over which he may have incomplete control, is not, however, so potent a factor as a difference in mental attitude, usually the result of training or tradition. The existence of two distinct attitudes on the part of authors towards their 'species' is common knowledge. In the absence of more suitable terms we may speak of them as the 'parental' and the 'judicial.' To the parental worker his species are children, whose appeals, even when *ad misericordiam*, are sympathetically received. To the judicial worker his species are claimants, whose pretensions must be dispassionately weighed. The former treats the recognition of a species as a privilege, the exercise of which reflects honour. The latter views this task as a duty, the performance of which involves responsibility. With amply characterised forms the mental attitude is inconsequent, but when critical forms are reviewed it is all-important. Here the benefit of a doubt is the practical basis of final decision. This benefit in the case of the parentally disposed worker may lead to the recognition of a slenderly endowed species; in the case of the judicially inclined, to the incorporation of an admittedly critical form in some already described species, the conception of which may thereby be unduly modified.

These attitudes do not in practice divide descriptive workers into two definite classes. Some writers display one attitude at one period, the other at another period of their career. Occasionally the two alternate more than once in a writer's history. Cases are known in which one attitude is consistently adopted towards species of one natural family, the other towards species of a different family.

When want of uniformity in delimitation is due to the varying effect of the same facts on different observers there is no room for either praise or blame. Capacity for appreciating affinities is complementary to that for discrimination. The fact that now one, now the other tendency is more highly developed makes for general progress. Workers in whom the two may be more evenly balanced can strike a mean between the discordant results of colleagues more highly endowed than they are in either direction. But those who possess a capacity for compromise do not mistake this for righteousness; they are apt to wish themselves more gifted with the opposing qualities of those whose work they assess.

When cases in which want of uniformity in delimitation due to difference in mental attitude on the part of independent workers are considered, we again find that praise and blame are inappropriate. If both attitudes have defects to be guarded against, both have merits that deserve cultivation. The defects are patent and rarely overlooked; ; the careful systematist, more critical of his results than anyone else can be, is alive to the risks which attend stereotyped treatment, and on his guard against the excesses to which this may lead. It is more often forgotten that both attitudes have their uses, and that each should be exhibited at appropriate times. Here, however, no middle way is possible; the mean between the two attitudes has the qualities of a base alloy. It is the attitude of indifference, fatal to scientific progress, and productive of results that are useless in technical research.

The ideal arrangement in monographic study is the collaboration of two workers, one highly endowed with the discriminating, the other with the aggregating faculty. But for the statement of their joint results both must adopt the judicial attitude. On the other hand, in floristic work, in isolated systematic contributions, and in all descriptive work undertaken on behalf of economic research, the better because the more useful results are supplied by workers in whom capacity and attitude combine to induce the recognition rather than the reduction of easily characterised forms.

In the present state of our knowledge uniformity in the delimitation of what are termed 'species' is unattainable. We are in no danger of forgetting this fact; what we do sometimes overlook is that, circumstanced as we are, such uniformity is undesirable. The wish to be consistent is laudable; when it becomes a craving it blunts the sense of proportion and may lead to verbal agreement being mistaken for actual uniformity. The thoughtful systematist, when he considers this question without prepossession, finds that forms which in one collocation need only be accorded a subordinate position must, under other conditions, receive separate recognition.

The normal effect on specific limitation of the causes that militate against uniformity is easily understood, and the resulting discrepancies can be allowed for in statistical statements. There are, however, cases where the capacity for appreciating differential characters or points of agreement is so highly developed as to obscure or even inhibit the complementary capacity. The effects are then ultra-normal; nicety of discrimination exceeds the 'fine cutting' allowable in floristic work; aggregation exceeds the limits useful in monography. No common measure is applicable to the results, and the ordinary systematist, who has definite and practical objects in view, expresses his impatient disapproval in unmistakable terms. The work of those addicted to one habit he characterises as 'hair-splitting'; that of those who adopt the other he speaks of as 'lumping.' The industry displayed in elaborating monographs which attribute a thousand species to genera wherein the normal systematist can hardly find a score must often be effort misplaced. The same remark applies to the excessive aggregation that substitutes for a series of quite intelligible forms an intricate hierarchy of sub-species, varieties, sub-varieties, and races. Orgies of reduction are moreover open to an objection from which debauches of differentiation are free. Discrimination can only be effected as the result of study; the finer the discrimination, the closer this study must be. Reduction offers fatal facilities for slovenly work, over which it throws the cloak of an erudition that may be specious. When dealing with excessive differentiation the normal systematist is on solid ground; when following extreme reduction he may become entangled in a morass. Yet workers of both classes only exhibit the defects, for ordinary purposes, of striking merits, and there are occasions when the results that each obtains may be of value to science.

PRESIDENTIAL ADDRESS.

Its mnemonic quality renders taxonomic work practically useful. Its application in economic research does the same for specific determination. Economic workers are chiefly interested in useful or harmful species; to others they would be indifferent were these not liable to be mistaken for such as are of direct interest. The identification of economic species and their discrimination from neutral allies is not always simple, because species that are useful or noxious are often those least perfectly known. The qualities that render them important frequently first attract attention; these may be associated with particular organs or tissues, and samples of these parts alone may be available. Ordinarily, when material is incomplete, critical examination has to be postponed. In economic work, however, this may not be possible, and the systematist, just as in dealing with archaeological or fossil remains, may here have to make the most of samples and fragments in lieu of specimens. Cultural help and anatomical evidence sometimes lead to approximate conclusions; often, however, as with neutral species, definite determination must await the communication by the field botanist of adequate material. Even then a difficulty, comparable with that frequently met with in archaeological and palæobotanical study, may be encountered. As archaeological or fossil material may, owing to the conditions to which it has been subjected, look unlike corresponding fresh material of the same or similar plants, so may trade samples, owing to special treatment, bear little outward resemblance to the same organs and tissues when fresh.

When material of economic plants is ample another difficulty may be encountered. Domesticated species often undergo perplexing variation. In studying this variation the systematist may have to seek linguistic and archaeological help, and be led into ethnological and historical by-paths. In classifying the forms that such domesticated plants assume he gladly avails himself of aid from those whose capacity for detecting affinities is unusually developed. But even with extraneous assistance the systematist, in this field, sometimes fails to attain final results. He can, however, always pave the way for the student of genetics, whose work involves the study of the 'species' as such. As regards forms of economic importance which neither organography nor anatomy can characterise, but which the chemist or biologist can discriminate, physiological methods are required to explain the genesis or elision of qualities evoked or expunged under particular conditions.

A highly developed capacity for aggregation, if properly controlled, is also useful in the study of plant distribution from a physiographical standpoint. The systematist shows his sympathy with phytogeographical needs in two very practical ways. He declines, out of consideration for the geographical botanist, to deal with inadequate material, and for the same reason he refuses, in monographic studies, to be influenced by geographical evidence. The monographer is conscious that if he pronounces two nearly related forms distinct, merely because they inhabit two different areas, he is digging a pit into which the phytogeographer may fall when the latter has to decide for or against a relationship between the floras of these two tracts. But the fact that, with existing knowledge, uniform delimitation of species is impossible, seriously weakens the value of normal systematic results for phytogeographical purposes. The units termed 'species' that are most useful in floristic and economic study are often too finely cut to serve distributional ends. When all existing plant-types have been treated on monographic lines the results may with relative safety be used by the phytogeographer, since errors due to personal equation may be regarded as self-eliminating. As matters now stand, however, the geographical botanist obtains his evidence partly from monographs, partly from floras, and is apt to be misled. Yet even in floristic work the systematist sees that the 'species' which it is his duty to recognise often arrange themselves in groups of nearly allied forms. These

groups, which need not be entitled to sectional rank, while very variable as regards the number of species they contain, are more uniform than species in respect of their mutual relationships. They are therefore more useful than species as units for phytogeographical purposes. In defining these groups the faculty for aggregation is essential, and those in whom this faculty is highly developed may here be profitably employed, even when their discriminating powers show a certain amount of atrophy.

The cases, by no means rare, of workers who, with a comparatively poor eye for species, display great talent in their treatment of genera, afford indirect but striking proof that the faculty for aggregation may be more highly developed than its complement, and that the dominance of this faculty may ensure useful results. But the *a priori* expectation that in dealing with families this dominance should be still more valuable is not borne out by experience, for in this case it is recognised that aggregation has probably been pushed too far. This error has not been attributable to the faculty for aggregation so much as to the evidence at its disposal; the corrective has largely been supplied by the use of anatomical methods as supplementary to organographic data.

The physiologist in studying processes is not always obliged to take account of the identity of the plants which are their theatres of action. He has at hand many readily accessible and stereotyped subjects whose identity is a matter of common knowledge, and as his experience increases he learns that he may sometimes neglect the identity even of these. If he asks the systematist to determine some type on which his attention is especially focused, the physiologist only does this in order that he may be in a position to repeat all the conditions of an experiment required to verify or modify a conclusion. A passive attitude towards systematic study has thus been created in the mind of the physiologist; this passivity has been intensified by the fact that the direct help which the physiologist can render to systematic study is limited. Physiological criteria are indeed directly applied for diagnostic purposes in one narrow field, where organography and anatomy are synonymous and inadequate. But if it be true that the diagnostic characters on which the bacteriologist relies belong to some non-corpuscular concomitant of his organism, this attempt to apply physiological characters to systematic ends has failed. In many cases physiological characters do influence taxonomic study. Differences in the alternation of generations, specialised habits connected with nutrition, peculiarities as regards response to stimulation, variation in the matter of protective endowments, admit of application in systematic work, and are constantly so applied in the characterisation of every taxonomic grade. But the evidence as to these characters reaches the systematist through secondary channels, so that the help which physiology renders is indirect, and the passivity of the physiologist remains unaffected.

This passivity has at last been shaken by the development of the study of plant distribution from a physiological standpoint. The practical value of this study has been affected by the employment of a terminology needlessly cumbrous for a subject that lends itself readily to simple statement, and by the neglect to explain the status, or verify the identity, of the units included in its plant associations. A reaction against the use of cryptic terms has now set in, and the physiological passivity which has led workers in this field to ignore systematic canons when identifying the units discussed shows signs of disappearing. The ecologist, it is true, must classify his units in accordance with characters that differ essentially from those on which reliance can be placed by the systematist. But the characters made use of must be possessed by his units, and the ecologist now realises that, in effecting his purpose, he is

as immediately dependent on descriptive results as the economic worker or the geographical botanist, and that, if his work is to endure, his determinations must be as precise as those of the monographer, his limitations as uniform as those of the phytogeographer. The needs of the ecologist are, however, peculiar, and his units must be standardised accordingly. Ecological units are not the groups of species, uniform as to relationship, which the geographical botanist requires; nor are they the pragmatistical 'species' of floristic and economic work. They are the states, now fewer, now more numerous, that these floristic 'species' assume in response to various influences; and ecological associations can only be appreciated and explained when all such states have been accurately defined and uniformly delimited. In accomplishing this task the faculty for detecting differences is the first essential, and the physiologist has here provided a field of study wherein workers, whose tendency to nicety of discrimination unfits them for normal systematic study, may find ample scope for their peculiar talent, and accomplish work of real and lasting value.

We find, then, that the taxonomy of the wider and more general groups is now mainly based on phylogenetic study, and is largely scientific in character and application. The taxonomy of the narrower and more particular groups, based on organographic data supplemented by anatomical evidence, is often somewhat empirical in character, and is largely applied for technical purposes. Among the grades chiefly so applied, the 'species' is a matter of convenience, variously limited in response to special requirements, while the 'family' is a matter of judgment, crystallising slowly into definite form as evidence accumulates. But the 'genus' is relatively stable, and, in consequence of its stability, has long been 'a thing of dignity.' The distinctive air thus imparted to botany is best appreciated when a zoological index is examined.

The use of scientific names, more precise than popular terms and more convenient than descriptive phrases, facilitates the work of reference in applied study. These names are accidents which do not affect the taxonomic status of the units to which they are applied, but do, however, reflect the want of uniformity in the limitation of these units. The non-systematist who has to apply systematic results appreciates that, as knowledge now stands, this is unavoidable, and makes allowance for the state of affairs. But applied workers complain that, in addition to this, descriptive writers show a tendency to care more for names than for the forms they connote, and wantonly alter the designations of familiar forms. The complaint is just, yet the action is not wanton. The tendency in its present form is of recent origin, and, paradoxical as the statement may seem, is the outcome of a wish for uniformity and stability in nomenclature. Of these two qualities the latter is of more importance in applied work, and therefore the more essential. Unfortunately the systematist has given a preponderating attention to the former, and, in his effort to attain a somewhat purposeless consistency, has allowed his science to wait upon the arts of bibliography. He has placed his neck under a galling and fantastic yoke, for nomenclature, though a good and faithful servant, is an exacting and singularly inept master.

To err is human, and the standard of diagnostic work, high as it is, falls short of the standard which the systematic worker desires to attain. It is this fact that explains the remarkable openness of mind, and the great readiness to accept correction, to which systematic study conduces. To this also is attributable the singular freedom of systematic research from the practice of making capital of the fancied shortcomings of fellow-workers. Exhibitions of this commercial spirit are not altogether unknown, and in one narrow field,

where systematic results are practically applied, they are sufficiently common to appear characteristic. But they are contrary to the traditional spirit of systematic study, which is uncongenial to the arts of *réclame*.

The subject is by no means exhausted. Time, however, forbids more ; but the purpose of this sketch will have been fulfilled if it has helped those whose work lies elsewhere to appreciate more clearly what systematic study tries to accomplish, and to realise the place it fills in the household of our common mistress, the *Scientia amabilis*.

British Association for the Advancement of Science.

WINNIPEG, 1909.

ADDRESS TO THE AGRICULTURAL SUB-SECTION

BY
MAJOR P. G. CRAIGIE, C.B., F.S.S.,
CHAIRMAN OF THE SUB-SECTION.

THE occupant of this chair, in the great annual convention of the promoters and appliers of science, cannot fail at the outset of a new session to put on record his emphatic endorsement of the claim, so strongly and so reasonably pressed by his distinguished predecessor at Dublin, that distinctively agricultural problems, instead of being regarded as a subsidiary sub-section of any single division of the Association, should be accorded the full dignity and convenience of a 'Section.' Specialised research is to-day one of the governing features of scientific inquiry. It is but fitting, therefore, that those who are trying to equip the agriculturist with all the knowledge which recent speculation and experiment have to offer for the fuller and more economic development of the soil should at least be allotted equal space and sectional rank with the engineer, whose problems are discussed in Section G, or with the schoolmaster, whose educational methods are debated in Section L.

If there were any country in the world where an apology could legitimately be offered for relegating agricultural science to a secondary position, it is certainly not that in which we meet to-day. In this wide Dominion of Canada, in this progressive province of Manitoba, in this great city of Winnipeg, where the agricultural industry must dominate the interests of the people, hardly any subject in the whole range of study can claim a more paramount degree of attention than the utilisation of the land for the use of man.

This is by no means a matter which can be disposed of as an occasional side-issue in the deliberations of any single Section. If we agriculturists have been tardy in coming to be taught by the scientists, we are in earnest now in the application for instruction that we make. Neither is it to any one science we appeal. Even the stern mathematician or physicist of Section A can teach us something, arithmetical and meteorological, for the right conduct of our business and the wiser forecasting of our plans. The chemists of Section B have, in an infinite variety of tasks, to come to the aid of the farmer, and they have doubtless much to tell of the magic they can promise in the direction of fertilising methods. Section C must be raided for the experts who know the contents of the soil itself and its capacities. Section D may have much to pass on to us concerning

the live stock and the insect enemies of our farms. Section E may enlighten us on the world-wide distribution of crops and the new regions awaiting the skill of the husbandman. To Section F we look for warnings as to the economic conditions and barriers which—as we are apt to forget—hedge round our industry, and for the statistics which must govern the varying direction which we give to our enterprise from time to time. The mechanical operations of our calling suggest to us the practical assistance which Section G can surely offer. Nor does even Section H lie wholly remote from the inquiries we may need to make as to the resources of the globe and the wants of diverse communities. The physiology of Section I opens regions of research quite germane to many of our daily studies. Under Section K, as an overlord, we rest to-day assured that if every botanist is not a farmer, every farmer must in a sense be a practical botanist, for ever face to face with the plant and its environment. Perhaps also, in common with all the rest of the world, we may have something to our advantage to hear from the pedagogues of Section L, who may advise our scientific counsellors as to the best form in which even the practical farmer may be taught.

Addressing ourselves, however, to the immediate task in the sub-section allotted to us, I suggest to you to-day that, having regard to the place where we meet, I may, as a proper prelude to your debates, invite you to consider, even if only in the broadest way, what are the leading factors that govern the fluctuations of this our industry of agriculture all the world over, and in new countries in particular. The first factor of all is undoubtedly population—its growth, its rapidly varying local distribution, and its changing and diversified needs. It is for man that crops are raised, whether these crops are to furnish food for direct consumption or for the sustenance of live stock, or whether they furnish us with our clothing, like the wool and the cotton of other lands, or with the materials for shelter, as the great timber crops which your vast forests here may bear. When we know what is the demand at any given place and time, we shall be prepared to give a more exact examination to the means of turning out the effective supply at the right moment and in the right place, be it of wheat, of meat, of fruit, of wool, of flax, of cotton, or of timber.

Sir Horace Plunkett told us last summer that he hoped to find in an Agricultural Section 'some humanised supplement to the separated milk of statistics.' Perhaps he unconsciously reflected in that remark the suspicion that in earlier days the agricultural debates, which, for want of a better place, took place in the Economic and Statistics Section, unduly paraded the bare figures of the position. But I myself confess that, however mortals may shrink from the rigid arbitrament of arithmetic, neither the teaching of the scientist nor the rhetorical advice of the philosopher will lead the agricultural student of the future, even if he have the luxury of a complete Section of his own, to any fertile result, unless he begins by a clear diagnosis of the facts as they stand, on the one hand as regards population, on the other as regards production. We shall by no means waste time if we try to investigate, with some approach to exactness, what are the areas still available for extended cultivation, and who and where are the consumers of our products, and what are their present and future demands.

Obviously, however, in the limits of an Address like this it is impracticable to make, in any detail, a world survey such as this implies, and it is only the most patent of the changes in the world's populations and their agricultural demands which I can put before you. There was a time when the human family lived in self-contained groups, extracting their requirements from the soil which lay around them. So lately as one hundred years ago there was very little of the international trade in food or other agricul-

tural products such as is familiar to our practice to-day. The nations largely lived on their own territories, and the world has wide sections still where production is limited by local needs. But even a hundred years ago or more perpetual questions were emerging as to the time when men should have multiplied more rapidly than food. The transportation revolutions of the nineteenth century may be almost said to have laid that scare by their aid to the mobility alike of the world's populations and of the world's produce. For the migration of men from dense settlements to open lands on the one hand, and the transport of their produce to the cities of the old world on the other, have simplified, and may simplify still further, the solution. It is all a question of distribution.

If the world holds to-day just twice as many souls (as the best demographic authorities seem to assume) as it did only some two generations back, this growth has been by no means uniform, and the development is governed and provoked by the pressure of population on sustenance. Sometimes, I think, we are apt to forget what Professor Marshall, of Cambridge, has so well laid down, that 'man is the centre of the problem of production as well as that of consumption, and also of that further problem of the relation between the two which goes by the name of distribution and exchange.' Vastly has the latter problem been simplified by the giant strides the second half of last century has seen in annulling distance and in facilitating transport, till all the world bids fair to become a single community. Whether the present distinguished British Ambassador to the United States was right in looking forward to the gradual unification of the type of the world's inhabitants by the diverse processes of ultimate extinction and absorption of inferior races, I think we will agree with him that the spread into new regions of conquering or colonising races has provoked desires for, and made practicable the supply of, far more varied wants than once were even contemplated, or could indeed have been made available, while the producing areas were sundered widely from the consuming centres.

The sixteen hundred million souls this earth of ours now carries are at present by no means evenly spread over its surface, and a population chart reveals the most extraordinary diversity in the density of the people on the soil. More than one-half are on the continent of Asia, and of these a large section are densely clustered in India, China, and Japan. In Europe, where the average density is double that of Asia, and approximately one-fourth of the world's inhabitants are gathered, many portions are nevertheless still far less thickly peopled than the Eastern States just named. Populations, over any considerable areas, exceeding 500 to the square mile, may be found on the world's map not only in parts of the United Kingdom, in Belgium, or in Saxony, but yet again on the Lower Ganges, on the Chinese coast, and even in portions of the narrow valley of the Nile. But the Indian or the Chinaman are not, broadly speaking, to be ranked among the communities of which we are thinking when we concentrate our attention on the increasing transport of breadstuffs or of meat from the New World to the Old, which has become the prominent feature of the agriculture of our own day, whatever attention may have to be given to the conditions of the Far East at some distant date.

The great movements of agricultural products which have signalled the last half-century are not for the most currents of food supply into Asia, or into Africa, or North America, despite certain limited exceptions which are just beginning to attract attention, as possibly hereafter significant in the case of imports of wheat into Japan or China, of Australian meat into Eastern Asia and South Africa. The Asiatic or the African agriculturist is for the most part content to find the primary necessities of life close at

hand. It is mainly Europe, and indeed Western Europe, that calls to-day for the import of breadstuffs or meat or dairy produce. There the growing volume of sea-borne imports has not only materially influenced the agriculture of old settled countries, but at the same time has signalled to the European toiler that space and plenty awaits him oversea, and has stimulated the development of new spheres of cultivation at a rate which the relatively sparse population of the New World, unless largely recruited by immigration, could never accomplish.

I ventured some years ago, from the chair of the Royal Statistical Society, to review the recent changes we have seen in the structure of the world's populations, and urged the greater wisdom of bringing the men to the food rather than the food to the men. The centripetal force which was, in all parts of the earth and not in the oldest countries only, packing more and more together the human family in vast industrial centres, which drew the materials of their handicraft and the food for their maintenance from far distant lands, seemed to my judgment a much less healthy form of development than the older centrifugal impulse which led man to move himself to the newer regions, where the produce was nearer to the mouth of the consumer, and where he could fulfil the oldest obligation of the race to go forth and replenish the earth and subdue it. The vision that meets us here of ample land awaiting man, of possibilities of agricultural production which can only be realised by well-considered and augmented immigration, impresses the visitor from an old and overcrowded country. Before and above all speculations of what transport has done, and may yet do, to carry masses of agricultural produce across the ocean, I must claim, as the better prospect, a steady settlement of these wide acres by a population resting on the soil which this great Dominion offers, and drawing from it, by a more diversified and more general and more wholesome type of farming, a far better, and in the long run a more economic, return than the mere extraction of grain for export can ever promise.

Taking the thirteen States of Western and Central Europe as an example of what I mean, there were added there, in the last seventy years of the nineteenth century, on a comparatively limited surface, something like 100,000,000 new consumers to the 167,000,000 persons previously resident on the 1,700,000 square miles of territory occupied by this group of nations. These numbers, too, take no count of the emigration which has lightened the pressure on the soils of the home lands of Europe. Clearly the maintenance of nearly 70 per cent. more consumers must have meant either a vast development of local agricultural production or a vast demand upon the acreage of the new land of the West, or both. The defective nature of the early statistics obstructs the search one naturally makes into the extent on which these new populations on the old lands have been fed on larger local areas, or from larger yields on non-expansive areas. Adopting, therefore, a much shorter range of view, the lifetime of a single generation has given us 30 per cent. more consumers in Western and Central Europe than were there in 1870, the German element rising apparently by 50 per cent., the Scandinavian, Belgian, and Dutch group of small nationalities by 44 per cent., and the United Kingdom by 40 per cent. in this interval, while these developments were of course reduced in their effect on the total by the slower growth of the South-Western nations and the nearly stationary condition of France.

No larger areas, but rather smaller ones, of the chief food grains are apparent in Great Britain or Scandinavia or North-Western Europe. The German areas of wheat and rye show practically little change, and although, if the Hungarian areas are larger in the centre of Europe, the general movement is not upward in respect of food-producing area. Even in live-

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stock the numbers scarcely keep pace with population, for although the herds and the swine of Western and Central Europe have risen by nearly a fourth in the one case and three-fifths in the other, the sheep, except in Great Britain, are much fewer now.

On the average of the first quinquennium of the present century the home production of wheat represented only about 20 per cent. of the consumption in the United Kingdom or in Holland, 23 per cent. (apparently) in Belgium, 64 per cent. in Germany, and perhaps 80 per cent. in Italy; and the imported grain to fill the deficits was considerably over 400,000,000 bushels. Nearly half of this came, of course, from Eastern Europe, and particularly Russia. Such a mass of produce would require 20,000,000 acres elsewhere, even if the exporters could raise it, as most have certainly *not* done, at twenty bushels per acre, and nearly double that area if the yield was only that of some of our largest exporters to-day.

The actual reductions of area in Western Europe are not in the aggregate extensive, although Belgium has seen her grain area shrink from 30 to 25 per cent. of her total surface, France from 28 to 25.5 per cent., and the United Kingdom from 12 to 10 per cent. The grain-growing capacity of European States varies greatly, and it would be interesting, were the data everywhere available, to see how far we have distinct evidence of an appreciable if not any great advance in the yields extracted from the non-expanding areas under the more recent conditions of scientific knowledge. Nowhere is so large a share of the total surface under grain as in Roumania, an Eastern European State and not inconsiderable wheat exporter, and there, at all events, the total grain acreage developed between 1886 and 1906 by nearly 25 per cent., and the surface under wheat by 72 per cent. The yield there, according to some official reports, was something over fifteen bushels per acre in the five years before 1890, and in those ending 1906 it was over nineteen bushels—the latest year nearly touching twenty-three bushels; the barley yields of the same State rising from an average in the former quinquennium of thirteen bushels to over nineteen bushels in the latter.

In Hungary, another European grain exporter, the wheat acreage has been materially developed, rising from over 7,000,000 acres to 9,500,000 in twenty years to 1906, and but slightly receding since, while the yields are also materially greater.

France, with a drop in wheat acreage of 1,000,000 out of 17,000,000 acres, has between 1884 and 1908 raised the average of her production on a five years' mean from 17.8 bushels to 20.2 bushels, and thus turned out somewhat more produce from a lessened surface.

Germany, on a constant but much smaller wheat area of 4,700,000 acres, with a quinquennial average yield of 20.3 bushels, would seem to have raised this to 27.9 in 1899-1903, touching a still higher level in more recent seasons, when 30 bushels were apparently approached, although some changes in her statistical methods of inquiry may slightly reduce this comparison.

Some effort to feed new mouths from old acres has thus indeed been made. Nevertheless, without disregarding altogether the qualifications which a careful statistician would deem it his duty to admit, one may broadly say Western Europe looks mainly for the growing needs of her consumers to the still exporting States of Eastern Europe, to the New World regions of North and South America, and in a minor degree to Australasia.

Before we quit our session here in Winnipeg we may expect to learn something of scientific interest and of economic guidance respecting the response of Canada to the Old World's call. But it is not for grain alone that densely peopled countries turn to the new fields of the West. Probably the geographical conditions of our place of assembly this year will not

lead us at all closely into discussion on the variations in the sources and fluctuations in the volume of the wool supply, or that of cotton, but the possible development of livestock on the territories of newly settled countries may be expected to come well within our purview, and afford us lessons in the development of the export trade in meat and dairy products, and the relation of the Canadian to the surplus of other States. The Royal Statistical Society of London had a paper this summer by an old colleague of mine, Mr. R. H. Hooker, which, although primarily devoted to the supply of Great Britain herself, and the price of meat in her markets, has a world-wide view of what is going on all around us in the conditions of production and of transport in a commodity as important to human life as wheat itself.

Fully a quarter of a century has gone by since, on a former visit to Canadian soil at Montreal in 1884, I raised a debate on this subject of the production and consumption of meat, and the various conditions of its transport. The twenty-five years that have passed since then have not rendered that particular topic a less important one for the consumers of old countries or the farmers of new, but ever-varying factors are presented by the opening of new territories to exploitation and the denser massing of accumulated populations with growing needs, and increasing preference for the most concentrated form of aliment. Among the most recent factors to be remembered as influencing one side of the meat trade future are the admissions of qualified experts in the United States as to the degree in which the growth of population there was beginning to trench upon the meat surplus of that Republic. On the other hand, the producer will not fail to bear in mind the rapidly advancing importance of partially developed areas and the great advantage of the more economic forms of dead-meat transport now adopted in South America, and will weigh against these the degree in which the herds of the vast prairies of North-Western Canada may be further utilised when questions of handling economically the resultant meat supply may be effectively elaborated.

To-day, however, and here especially, one cannot but be reminded that in whatever direction we look for the aid of science to stimulate the development of Canadian resources, or to help the producers now in these provinces in measuring the probabilities that lie before them, or to summon eager emigrants to the land you have to offer them, there is an intense and ever-engrossing interest in the present and the future of wheat. Alike, therefore, to the statistician and economist on the one hand, and to the experimentalist and investigator on the other, we turn to ask what advice they can give to the farmer of a new country with an area so vast as the North-West of Canada presents, whether and how far and at what rate, with profit to himself and with benefit to the bread consumer across the ocean, he can push the extension of the well-nigh eight million acres of wheat land which the Dominion claims to show her visitors in 1909.

The problem, important as it is to this particular region where we are met, cannot, however, rightly be treated as a purely Canadian question. It is a problem of world-wide interest and of great magnitude and more complexity than has been sometimes recognised, for it is none other than the issue of the race between population and production so far as at least one primary essential of human diet—bread—is concerned.

Within a year of the last visit to this Dominion of the British Association the question was raised by no less an authority than the then President of that body at the Bristol meeting of 1898, whether the possible wheatfields of the globe possessed a potential capacity of expansion sufficient to meet the hypothetical needs of the bread-eaters of even one generation ahead; whether, in fact, a dearth of wheat supply was not already within sight, and by 1931 would be upon us. The suggestion that the wheat-producing soil of

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the world was already becoming unequal to the strain put upon it by the multiplication of men was not unnaturally met by a vigorous criticism. The mere suspicion that some day, however, there would not be land enough to go round, that famine could be averted only by the beneficial magic of the chemist, is too vital a possibility—even if some of us do not place the date so near or rely so fully on some of the computations made—not to command a very careful examination of the remedy propounded, the promise of the artificial production of nitrate in such a volume and at such a price as would raise the average of the world's production from 12·7 to 20, if not even to 30 bushels of wheat per acre.

The fixation of nitrogen, not as a dream but as a certainty, was, it will be remembered, claimed by Sir William Crookes as the condition on which the great Caucasian race was to retain its prominence in the world, and avoid being squeezed out of existence by races to whom wheaten bread is not the 'staff of life.'

Personally, I confess I am not so pessimistic as to the surface still available for wheat-growing even without this aid. If we grant that the so-called contributory areas, at a date two or three years before the close of last century, were just what was then stated, that the bread-eating population of that date was rightly guessed at 516,500,000—a much more difficult certainty to reach in the manner adopted by the American statistician whose figures were adopted—and that both the growth of population and of 'unit consumption' would proceed exactly in the ratio suggested, it may legitimately be asked, does it nevertheless follow that no such increment of area can be looked for as would satisfy the larger mass of consumers calculated for as likely to be dependent upon wheat in 1911 or 1931 on the scale here laid down?

I should not, in any statistical investigation into these questions, be contented to assume the probability of the exact continuance of previous ratios in the rate of production, or that of individual consumption over such periods, and my experience of very big averages makes me shy of adopting a simple mean of such wide diversities as correctly representing the head-rate consumption of wheat. These are points which might be more fittingly debated elsewhere. I want to narrow the issue now to the actual and more recent course of the wheat-growing surface; for it seems to me that the lesson of such figures as we have in the past, and as those of Mr. Wood Davis's tables, is rather one of irregular than of arrested extension. The periodical opening up of new areas, very often in advance of consumptive requirements of the time, would seem almost invariably to be followed by a pause while prices recover from the over-supply, and that again by new developments and exploitation in new directions, or by better methods on the areas made tributary to the wants of the ever-increasing men.

We may admit that the course of the wheat acreage from 1870 to 1884 and thence onward to 1898 showed—first, a material advance outstripping that of population, then an admitted and serious check, with a subsequent advance, although one below that of the bread consumers of the world.

Let me ask, however, if a later view of the wheat area at the disposal of the world's consumers is not well qualified materially to diminish, if not to dissipate, the 'cosmic scare' which, no doubt contrary to the real design of the distinguished chemist who followed Mr. Davis's estimates, was induced by the figures of 1898. My own comparisons of the later growth of acreage covers only the decade from 1897 to 1907, or as nearly to these years as figures permit, and in the form I originally designed it might bring into view something under 230,000,000 acres as the world's present extent of wheat-field. But, to place matters on a more comparative level, I am willing to omit the large Indian totals and some few of the

TRANSACTIONS OF SUB-SECTION K.

distant regions which, partly on account of the somewhat uncertain identity of the areas they include at different dates, and partly on account of their relatively small contribution to the bread of the Western world, do not find a place in the estimates with which I am now making a comparison. For the leading groups of other areas the figures stand in millions of acres to a single decimal:—

Groups	1897	1907	Increase in 10 years
Russian Empire	46.6	59.5	12.9
United States	39.5	45.2	5.7
Three chief European Wheat States	37.6	39.8	2.2
The Rest of Europe	20.8	21.4	.6
Argentina and Uruguay	6.7	15.0	8.3
Canada	3.0	6.6	3.6
Australasia	5.0	6.0	1.0
Total	159.2		34.3

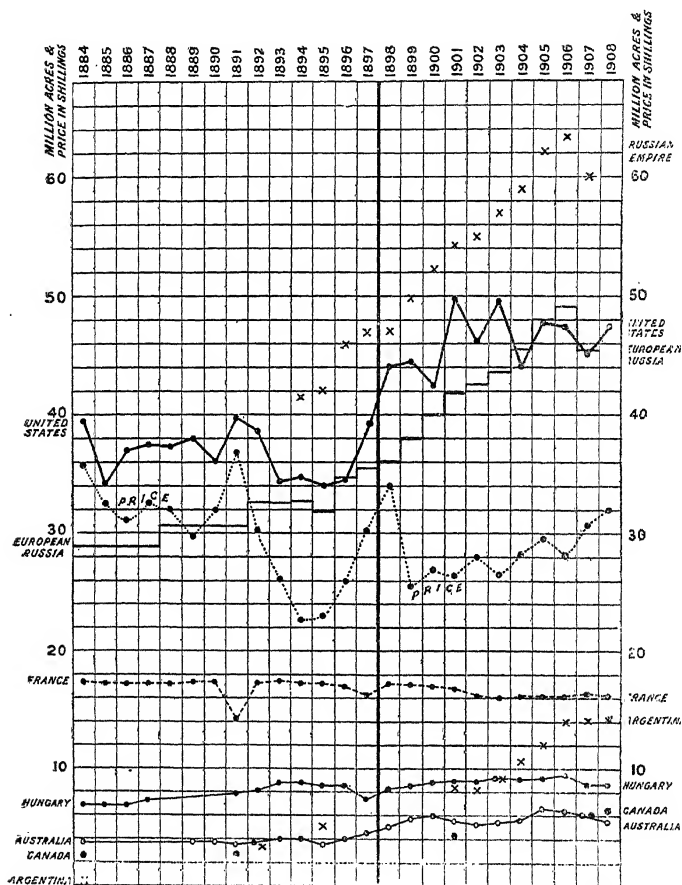
Now, whatever be the estimated increase in wheat-eating population between these two dates, it cannot in the aggregate be $21\frac{1}{2}$ per cent., as is the growth of the wheat surface in these States. Nor will the result be materially affected if allowance were to be made for the three or four million acres represented by the exports of unnamed States in this table, or even by the inclusion of any minor units of wheat-growing, such as Portugal, or Greece, or Switzerland, for which Mr. Wood Davis estimated from sources not recognised in our official statistics, their totals being well under a single million acres, and the variation, if any, probably insignificant.

If, therefore, the growth of men outstripped the growth of wheat, as we have been warned was the case between 1884 and 1897, the growth of wheatfields has been well over the rate of population increase since that exceptional period, just as it was in the still earlier period between 1871 and 1884. Nor is the check to the rye acreage and its decline by 4 per cent., which seemed to have happened concurrently with the wheat check between 1884-1897, continuing; for that, in the aggregate, seems to have returned to, though it has not perhaps much exceeded, the older level.

Comparisons at single terminal points have always a danger which may be avoided by examining more carefully the leading facts year by year. On the diagram which I introduce here I have tried, therefore, roughly to sketch the curves which indicate the growth of wheat acreage, both before and since 1898, in Russia, the United States, Argentina, Australia, and Canada, as typical of the exporting centres, while the acreage in France and Hungary has been added for comparison. The effect is, I think, to bring out the very much greater extension which has been going on during the last decade than could well have been looked for on the basis of the 1884-97 figures.

For the Russian Empire as a whole data are available only since 1895, but I have shown by a separate and steadily mounting line the wheat area of the fifty governments of European Russia, which are comparative for the entire period, and the latter are quite sufficient to establish my conclusion. There is, too, a suggestiveness about the course of prices (in shillings per quarter) in England, the chief recipient of wheat exports, which I have traced by a separate curve across this diagram. This may perhaps aid those who are disposed to make a closer study of the figures. That study may not improbably suggest that in the very latest year—for I

have carried the diagram to 1908 where I can—we may be once again hearing another check, or temporary halt, in the course of wheat extension, such as that which puzzled inquirers more than ten years ago, but which proved only a pause in the task of finding all the bread the consumers wanted under the stimulus of better prices. The further leap of prices in 1909 to beyond the 40s. limit in England may effectively encourage extension.



The exceptional arrest of wheat-growing in the United States between the years 1880-1896, when—if we may accept the official statistics as actually representing fact—the rapid rise, which actually doubled the wheat acreage between 1870 to 1880, stopped altogether, was, I believe, the preponderating factor which suggested a general halt in wheat-growing. It should therefore be looked at more closely, and to get rid of the danger of attaching too much

Acreage of Wheat in million acres.

Year	Russian Empire	Of which in European Russia	United States	France	Hungary	Argentina	Australasia	Canada
1884	—	28.9	39.5	17.4	6.8	0.6	3.8	2.4
1885	—	—	34.2	17.2	6.8	—	—	—
1886	—	—	36.8	17.2	3.8	—	—	—
1887	—	—	37.6	17.2	7.3	—	—	—
1888	—	30.6	37.3	17.2	—	—	—	—
1889	—	—	38.1	17.4	—	—	3.8	—
1890	—	—	36.1	17.4	—	—	3.7	—
1891	—	—	39.9	14.2	7.9	—	3.4	2.7
1892	—	32.6	38.6	17.3	8.1	3.3	3.7	—
1893	—	32.4	34.4	17.5	8.6	—	4.0	—
1894	41.6	32.9	34.9	17.3	8.5	—	4.0	—
1895	42.2	31.9	34.0	17.3	8.3	5.1	3.6	—
1896	45.9	34.8	34.6	17.0	8.3	—	4.0	—
1897	46.7	35.6	38.5	16.3	7.4	—	4.5	—
1898	47.0	36.0	44.1	17.2	8.2	—	5.0	—
1899	49.7	38.0	44.6	17.1	8.4	—	5.9	—
1900	52.3	40.0	42.5	17.0	8.8	—	6.0	—
1901	54.3	41.9	49.9	16.8	8.9	8.3	5.6	4.2
1902	55.1	42.6	46.2	16.2	8.9	8.1	5.2	—
1903	57.2	43.8	49.5	16.0	9.2	9.1	5.5	—
1904	59.2	45.6	44.1	16.1	9.1	10.7	5.8	—
1905	62.2	48.1	47.9	16.1	9.2	12.1	6.5	—
1906	63.6	49.0	47.3	16.1	9.5	14.0	6.3	—
1907	60.0	45.5	45.2	16.3	8.6	14.1	6.1	6.1
1908	—	—	47.6	16.1	8.5	14.2	5.6	6.6

importance to the data of single years, the quinquennial average movement in the States over the whole of the last forty years may be summarised as under:—

Five-year Periods	Acreage in U.S.A.	Wheat Acreage Levels
	acres	
1868-72	19,500,000	} Extending rapidly up to 1880
1873-77	25,500,000	
1878-82	35,500,000	
1883-87	37,000,000	} Nearly stationary from 1880 to 1896
1888-92	38,000,000	
1893-97	35,500,000	} Again extending to maxima reached in 1901 and 1903, with a later slight decline in the latest years
1898-1902	45,500,000	
1903-1907	46,800,000	

Population in the States has, of course, augmented steadily all over the forty years, from 37,000,000 to 86,000,000, yet all through the stationary years, as well as those of advancing acreage, exports of wheat and flour continued—as much as a third of the crop being shipped abroad in some years—and the transfer of the wheat lands north-westward in the States was doubtless the striking feature of the recovery. Rightly to understand the revolution in the wheat-growing of certain States of the Union would require a treatise for which time could not be given here.

Let me, however, recur again to the general position. In the table already

given for the past decade the latest increase to be accounted for is 34,000,000 acres. I ask you to note that the Russian quota forms more than a third of the whole. Now it was Russia that was in a very special degree the subject of unfavourable remark in the wheat problem controversy of ten years ago. She was spoken of, I remember, as having reduced her consumption of bread by 14 per cent., and only by this means continuing her exports in defiance of her true needs, and contributing to the rest of the world therefore a merely provisional and precarious excess. I am not aware how the calculation here alluded to had been arrived at, nor have statisticians perhaps a very robust faith in the estimated numbers of the Russian population before the great census of 1897, but the subsequent history of her apparent wheat surplus is interesting.

The exports of wheat from Russia, which we were warned could not continue, and which had doubtless been unusually large between 1893 and 1898, shrank for three years after that date as if they would realise the prophecy which would relegate Russia from the ranks of exporters to the task of feeding her own population. But that mysterious empire has since then resumed her large supplies, and from 1902 to 1906 the exports ranged higher than before. Although forming only 24 per cent. of her estimated wheat crop, Russia's exports averaged 141,000,000 bushels over the first five years of this century, against 104,000,000 bushels over the whole preceding fifteen years. Quite lately we seem to see some restriction, but the history of the trade forbids a confident opinion that she has reached the end of her contributions to other lands.

So far as the areas under wheat are recorded, the Russian agriculturist keeps on extending his industry, and, low as the yields may frequently be, they are tending upward under, it may be presumed, some reform of the very primitive conditions of production. Within the fifty governments of European Russia alone, and omitting the Polish or Caucasian figures, which do not go so far back, the average area of 29,000,000 acres only in the 'eighties became 40,000,000 at the close of the century, rising to a maximum of 49,000,000 acres in 1906, a point from which a decline was shown in 1907 to 45,600,000 acres. This, however, even taking the latest and lower figure, is an advance of 10,000,000 acres in the last decade, or nearly 30 per cent.—surely considerably in advance of even the Russian growth of population, great as that is.

It has, I think, not been sufficiently realised that in the two decades stretching from 1887 to 1906, European Russia has added 1,000,000 acres of wheat per annum. This is not only a 70 per cent. advance in twenty years, but it is double the absolute area of 10,000,000 acres which the United States added in this interval. From such official estimates as are furnished, the total produce of these fifty governments, where alone the figures are continuous, increased in a still higher ratio. The average production, which did not exceed 180,000,000 bushels in the five years before 1879, or 226,000,000 bushels in the quinquennium ending 1889, reached what appears to have been a maximum in 1904, and was averaged at 415,000,000 bushels for the whole five years' period then ending. If the later years are again at a lower level, they represent very nearly double the produce before 1879. The yield per acre, which stood below eight bushels to the acre between 1883 and 1892, averaged nine bushels over the next ten years, and has been 10·9, 10·4, and 11·4 bushels respectively in the three seasons ending 1904. In the south-western region, where the yield was just over eleven bushels in the decade ending 1892, it seems to have averaged fifteen in the ten years ending 1902, while over eighteen and nineteen bushels were reported in 1903-1904.

These figures omit the Polish, Caucasian, and Asiatic districts, for which a much smaller retrospect is possible. The acreage in Poland is

small—little over a million—and nearly constant in extent. But the wheat of Northern Caucasia, first accounted for in 1894, has risen from 5,600,000 acres to 8,300,000 in 1906, and the Siberian totals, after increasing, apparently but slightly, from 3,400,000 acres in 1895 to 4,800,000 acres at the close of the century, do not seem much to exceed 5,000,000 acres now. Russian wheat production does not therefore seem a wholly arrested process.

I own I was hardly prepared for this old nation's progress in wheat-growing, and I have no doubt that I shall be told that Russia has been exchanging one form of bread corn for another; in particular, that dependence on rye has decreased as production of wheat has grown. There is some truth undoubtedly in this, for the comparatively stationary character of the rye area indicates that the Russian people, increasing as they are and continuing still an export of rye to Germany and elsewhere, may themselves eat somewhat more wheat and rather less rye, and it is true also that a fluctuating record has attended the surface under the coarser and larger cereal crop. Its 'low-water' point—61,900,000 acres—occurs in 1893, while its present figure is 66,000,000 acres. Relatively, therefore, while the rye shows no progress such as wheat, it cannot be said that the rye area has been utilised for the more valuable cereal, and the fact remains that there is more rye grown to-day, even in European Russia, than at any date since the last decade of last century began. Relatively to population, the available data show, the aggregate crops of wheat and rye together, in Russia as a whole, are materially greater than before.

Inquiry shows that the wheat extension in Russia has been made possible by an actual addition to the arable land, and not by deduction from other crops. A recent investigation quoted by a competent American authority informs us that some 23,000,000 acres of new arable land has been accounted for between 1881 and 1904, and, moreover, that a greater surface of this nominally arable area is now actually under cultivation than at the earlier date. These figures stand:—

Year	Total Arable Land	Under Crop	Wheat	Rye
	acres	acres	acres	acres
1881	288,000,000	174,600,000	28,900,000	61,600,000
1904	310,700,000	205,900,000	45,600,000	65,600,000

It will be noted that this inquiry ends a year or two since, but had it been continued to 1906 the comparison would have been accentuated, and as it stands the additional area cropped in one way or another exceeds 31,000,000 acres.

In Mr. Wood Davis's later memorandum he combats the idea that the expected wheat crops from four relatively new areas of production—Siberia, Argentina, Australasia, and Canada—would meet the shortage he found threatened by his estimate. Not unnaturally he regarded an 8,100,000 addition of acres in these four regions in fifteen years as a very insufficient and unpromising quota to feed over ten times that number of new bread-eaters on the globe between 1883-4 and 1898-9.

Assuming he rightly gave the increment of wheat between these dates as under, if I add to his table the latest data that I have, these new and gradually opening areas will show a rate of progress much greater in the nine succeeding years than before, even if there was no further increase in Siberia; for as to the areas to be included there I am certain. The figures I give in millions of acres:—

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—	1883-84	1898-99	Fifteen years increase	1907-08	Nine years increase
Siberia . .	2.0	3.3	1.3	3.3	—
Argentina . .	1.4	6.1	4.7	14.2	8.1
Australasia . .	3.2	4.5	1.3	5.6	1.1
Canada . .	2.4	3.2	.8	6.6	3.4
Total . .	9.0	17.1	8.1	29.7	12.6

In the forecasts offered ten years ago Argentina as a wheat-grower was given a dozen years from 1898 to reach a possible acreage of 12,000,000 acres. She has reached that figure and passed it in less than a decade, and later current official estimates seem to concede to that region a close approximation to 15,000,000 acres to-day. As the actual pace here has bettered so considerably that prophesied, one may legitimately question the further limitations which allowed to Argentina no prospect of ever reaching a wheat area of 30,000,000 acres at any time. That these prophecies by no means coincide with later and probably quite similarly vague forecasts in the other direction goes without saying. In a recent official publication by the U.S.A. Government containing the report of an expert on the resources of Argentina and her farming methods, the competitive prospects of the great grain-exporting Republic of the South were scarcely so lightly treated. For my own part I rather agree with an officer of the Argentine Government there quoted (Señor Tidblom), who candidly admits that it was impossible with any accuracy to forecast the ultimate wheat area of Argentina, although I observe he adds that there were 'more than 80,000,000 acres in the Republic that could be *immediately* devoted to successful wheat-farming if we had the farmers to do it.' I have seen, though I could not accept, even more sanguine estimates in other quarters, which, with a yield of only ten bushels per acre, promised a crop of 1,238,000,000 bushels at some future date, and would involve an area of wheat land approaching 124,000,000 acres.

No one, I think, can note the strides which Argentina has taken in rapidly augmenting her wheat areas and exports, and that concurrently with the commanding place she is assuming as a meat rearer and exporter to the older peoples of Europe, without some recognition that a great future is possible. On the other hand, apart from climatic conditions, the future must be largely governed by the factor of population; and the nature of the Italian immigrants, their mode of culture, their non-intention in many instances to remain and own the land or identify themselves with the country—preferring to exploit one farm after another and reside on them until they make a small competence wherewith to return to Europe—are all reasons against the extremely favourable prospects which I have here adverted to.

Small relatively to the great extent of surface included in the Commonwealth of Australia is the proportion under wheat, but the Commonwealth is none the less as a rule an exporter. A little more than thirty years ago only about 1,400,000 acres were grown. This seems to have been a good deal more than doubled in the five years 1876-81, when a much smaller rate of increase followed for fifteen years—a check apparently reflecting the same tendency to arrest which we have seen so typically illustrated in the United States. Again, after 1896, just as in the great Western Republic, wheat-growing became again in favour, and the rapid spurt which followed brought the Commonwealth total to 5,700,000 acres as the century closed. Thereafter the rate of growth seemed checked anew, and after passing

a maximum of just under 6,300,000 acres, it stands to-day under 6,000,000 acres. Twice during the last twenty years has Australia shown on balance a net importation of wheat, but from 1903 to 1907 the quantity exported has averaged 36,000,000 bushels, and it is not without interest to observe that the Australian exports of the present century have not all been consumed in Britain—South Africa, the western coasts of South America, and even some parts of India sharing in the surplus product of the Antipodean Continent.

The conditions and the future of Australian wheat have been quite recently dealt with in an interesting paper by Mr. A. E. Humphreys, read before the Society of Arts in London. It is here pointed out that the soils on which it is grown are rich in assimilable nitrogen, requiring little manurial expenditure in that direction, but poor in their percentage of phosphoric acid, while the climatic conditions as regards moisture have proved remarkably difficult. Efforts have been made, and apparently, if recent experiences be confirmed, with success, to breed new varieties of the wheat plant adapted to the peculiar climatic conditions of Australia and likely to increase the low average yields hitherto obtained. It is obvious that under Australian conditions the breeding of varieties of the wheat plant which will thrive on a low rainfall would make all the difference to Australia as a source of wheat exports. From 1902-1907 the Australian average yield was only half that of Manitoba, or nine bushels per acre; but this included one year of disastrous drought (1902-1903), wherein the Commonwealth average fell below $2\frac{1}{2}$ bushels to the acre. In New South Wales and Victoria, wherein more than half the acreage lay, it was even below this according to the official figures. Such instances offer the strongest evidence that could be offered of the extreme variability of Australian conditions, and make one almost hesitate to quote Mr. Humphreys' own cheerful estimate that in the State of New South Wales alone, wherein nearly a third of the Australian acreage is found to-day, or 1,886,000 acres, there was a possible area of good wheat land of nearly ten times this, or 18,000,000 acres.

To the last I have left another sphere of wheat extension, and one that will be most of all familiar to my audience. Yet here again the forecast of the Canadian future made in 1898 was surely unduly pessimistic. The opinion then quoted by Sir William Crookes as that of trustworthy authorities assigned to the Dominion a bare total of 6,000,000 acres under wheat as all that could be expected to be reached within a dozen years. That period has not yet fully come, but I observe that by December 31, 1908, the official figures show an acreage as reached within the decade which exceeds by 10 per cent. the maximum allotted to 1910. If I were to add the figure now ascertained for the 1909 crop, a total of 7,750,000 acres is now reckoned upon, so that here again the forecast has been outstripped. The further proposal to estimate the maximum of the Canadian potential capacity for wheat production by 1923 at no more than 12,000,000 acres will therefore, I imagine, meet severe critics in Winnipeg to-day.

I greatly wish that our contribution to the knowledge of the economic future of Canadian development may be, as the result of discussions here, some approach to an agreement to avoid all exaggeration on the one hand or on the other in these forecasts of future wheat-growing in the North-West; but I am very conscious of the risk of all far-reaching prophecy in a problem where the more or less uncertain growth of the immigrant population plays as great a part as the soil or the climate.

Sir William Crookes, in endorsing the most modest estimates of the capacity of this region, mentions that he had before him calculations which, I think most of us will agree, were, to say the least, exaggerated in an

opposite direction, attributing to Canada 500,000,000 acres of profitably utilisable wheat land. Against such inflated prophecies he argued that the whole area employed in both temperate zones of the world for growing all the staple food-crops was not more than 580,000,000 acres, and that in no country had more than 9 per cent. of the area been devoted to wheat culture. But error of estimate in one direction or another is quite inevitable when the available data on which to form a conclusion are so scanty. Replying later to journalistic criticism, Sir William, it must be remembered, acknowledged the undoubted fertility of portions of the North-West provinces; but, basing the conclusion on official meteorological statistics and on supplementary data supplied by Mr. Wood Davis as to the July and August temperatures of these regions, he suggested that 'from one-half to one-third only' of Manitoba—the south-west portion already fully occupied—was adapted to wheat. It was doubtless in the light of these climatic records that he inclined to regard 200,000 square miles of the whole 300,000 square miles comprising Assiniboia, Alberta, and Saskatchewan, as these regions were then defined, as lying 'outside the districts of profitable wheat-growing,' while even of the remainder it was apparently suggested that it would take thirty years from 1898 to place as much as 18,000,000 acres *under all grain crops*. Can we here to-day, with another ten years' experience, reach a somewhat greater accuracy in this search into the possibilities before us?

As illustrating the remarkable discordance of view hitherto existing, it is well to have before us, as a starting point for debate, some specimens of later but still most widely varying estimates of the capabilities of this country. These I quote from the cautious report rendered by Professor Mavor to the British Board of Trade in 1904, midway through the decade now closing. More or less speculative as it is fully acknowledged all estimates must be which purport to define the area 'physically or economically susceptible of wheat production,' that painstaking investigator set aside, as of little value, hypothetical curves setting forth the 'northern limit of cereal production,' reliable data for which 'were not forthcoming, and if they were they would be constantly changing.' After enumerating under fourteen different heads and sub-heads a formidable list of distinct but materially qualifying 'conditions' or factors covering questions of soil, of temperature, and meteorology, of moisture, sunshine, and acclimatisation of the plant, Professor Mavor suggests that, broadly speaking, the cleavage of the areas of different fertility runs obliquely from south-east to north-west through the great quadrilateral of the Canadian North-West. Alike in the north-eastern and in the south-western angle the conditions seemed to him more or less unfavourable. The south-eastern and north-western corners and the belt connecting them, however, presented relatively favourable conditions; an exception qualifying this sub-division was, however, suggested in the extreme north-west.

The vagueness of the statistical basis on which any numerical estimate of future wheat areas must rest cannot better be shown than by briefly referring to the results of five independent estimates which are quoted in this report. For the details of these estimates it is necessary to refer any student of the report to the analysis of each, differing as they do materially in their methods and in the classification of the areas comprised within the Manitoba, Assiniboia, Saskatchewan, and Alberta of that date. As regards the total area for settlement and for annual wheat-growing respectively, the first three of these estimates varied in placing the surface fit for settlement or susceptible of cultivation as low as 92,000,000 acres, and as high as 171,000,000, the annual surface available for wheat in these districts

ranging from 13,750,000 acres to 42,750,000 acres, and the resultant possible produce from 254,000,000 bushels to 812,000,000 bushels.

It should be added, to make these figures clear, that all the estimators quoted assume as a condition precedent to their accomplishment such an influx of population and settlement of the country as would be adequate to secure the cultivation of the hypothetical cultivable area.

With Professor Mavor, we may think that both the lower estimates are over-cautious and the third perhaps over-sanguine, while most properly he reminds us that beyond the physical capacity of any region, the question of economic advantage remains to be solved, under what may be conditions prevalent at a distant time, what effect a rise of price might have, and whether the farmers of the future would devote so much of their land as is here suggested, and so much of their working capital, to wheat alone. I ought to add that a fourth estimate referred to in the report takes the graphic form of a map, distinguishing the suggested area where the wheat crop is certain, where less certainty exists from the effect of summer frosts, and where, again, the crop is uncertain from insufficient moisture. Yet another estimate was quoted as made in 1892, but endorsed as not overstating possibilities of the future in July 1904, and this classified somewhat more than half of the land of Manitoba as 'land suitable for farming,' or 23,000,000 acres, allotting to the rest of the North-West 52,000,000 acres more, or in all 75,000,000 acres. The same estimator, forecasting the results for 1912 (or three years from the present time), allotted to Manitoba a probable wheat production of 168,340,000 bushels, and to Alberta, Assiniboia, and Saskatchewan 181,600,000 bushels. This crop of 350,000,000 bushels of wheat was in addition to an estimate of a further 200,000,000 bushels of oats and 50,000,000 bushels of barley. I have little hesitation in concluding, with Professor Mavor, that such widely divergent results, arrived at, as we are told, by competent estimators, illustrated the impossibility at the time of that report of setting out precise limits of cultivation in a region in which so much has yet to be done. To-day I would ask, Has the lapse of another quinquennium, full of interesting movements in both the population and the crops of the North-West, enabled us to reach any greater certainty? If so, the opportunity of this meeting affords an occasion to submit the conclusions, optimistic or pessimistic, practical or theoretical, economic or scientific, to the test of friendly and thorough discussion.

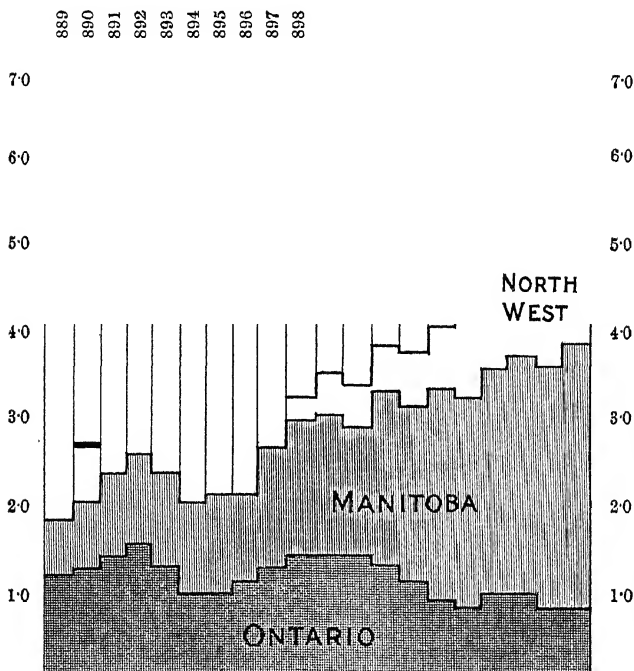
It is a relief to turn from the perplexing variety of these speculations as to the future to the relatively more solid ground afforded by the actual records of wheat extension here. If the progress of the past, and here once again more especially of the very latest decade, is to govern the prospect of the years to come, the wheat area of Canada must still possess a great expansive power.

There are defects of continuous statistics showing from year to year the total acreage of the Dominion, although the recent good work of the Census and Statistics Office promises that this will henceforth be remedied. But outside of the three great wheat-growing sections—Ontario, Manitoba, and the North-West—the surface under this cereal is not material. By the latest figures available the four Eastern Provinces do not now grow 170,000 acres collectively, while the small surface in British Columbia, not appearing in the last general Bulletin, was only 15,000 acres at the last census. In the roughly sketched diagram I insert here, therefore, the course of wheat-growing on 97 per cent. of the 6,611,000 acres accounted for in 1908 may be conveniently, if only approximately, traced.

The decline in Ontario, where, as in other older settlements, wheat-growing shrinks as more diversified forms of agriculture evolve, is much more than compensated for when the acreage of Manitoba, and in later years the

rest of the North-West, is superadded, as in the columns of this diagram, and the rapidity of the recent extension, which—had the 1909 figures reached my hands sooner would have carried the total area far beyond the seven million limit—testifies to the energy in the task of bread-raising which this hopeful section of the British Empire displays.¹

But whatever determinations we can reach on the hypothetical questions here propounded, whether we may regard the greater rate of wheat-field extension in the world at large, which has marked the last decade, as disposing of immediate alarm for the bread supply of the next generation, or whether we find in the recent whisper of augmenting prices corroboration



of the gain of population on subsistence, it is clear that our statistical records require a further development and a much improved continuity, especially in the new regions of the wheat supply of the future. Nor yet, again, can we dispense with the urgent lesson that science has much to teach us in making more use than we do of the areas acknowledged

¹ Were the preliminary estimates for 1909 taken into account, the total acreage would have been given as 7,750,000 acres—a rise of 1,139,000 acres in the latest twelve months. This is indeed the net result, for the West has added 1,402,000 acres—of which 1,289,000 were in Saskatchewan and 113,000 in Alberta—while there are declines in the East and in Ontario of an almost exact equivalent of the last-quoted figure, or 114,000 acres, and likewise a reduction of as much as 149,000 acres in Manitoba since 1908.

to be under more or less rudimentary cultivation. If Sir William Crookes was right in adopting the American statistician's average of 12.7 bushels per acre as the mean of the recognisable wheat-fields of the world, the prospect of the extra seven bushels he sought as immediately desirable will make us eager to learn the very latest triumphs of the laboratory in winning for the soil a freer measure of the nitrogen of the air. Even here in Manitoba, where a much higher yield seems on the average to be maintained under existing conditions, and where the cultivators with their 18 bushels average start from a vastly higher level, the promise of such a scientific ally should gladden the hearts of the hard-working pioneer.

One caution, however, I feel it my duty to give, as a practical rather than a scientific agriculturist. Whatever wonders are offered in the way of manurial adjuncts or mechanical contrivances, do not let our advisers overlook the paramount consideration of the cost which the newer systems may involve. For the extensive farming of a young country it is above all requisite to remember that expensive methods of cultivation are not as feasible as in the intensive husbandry of old settled regions. Hopefully as we may wait on the chemist's help, I confess that, for my own part, I incline still more confidently to the botanist, under whose aegis of protection agriculture has this year been placed by the decision of the authorities. The producer of new and prolific and yet disease-resisting and frost-defying breeds of wheat plants is to-day more than ever encouraged by what has been done in many lands of late in this direction, to suit the crop to its environment. Nothing could be a greater boon to the wheat farmers, handicapped by a short and irregular supply of summer warmth, and the occasional but often untimely invasion of the frost fiend, than the production of varieties of wheat at once prolific and early ripening, and suited to the relatively scanty moisture of semi-arid regions. What success Canadian investigators, with their renowned experimental system, have had in this direction we hope to hear at Winnipeg, while some of us who have listened to Professor Biffen, of the Agricultural Department of Cambridge University, look for hopeful results from the application of Mendelian laws to the breeding of wheat.

In closing, let me add that though it is a quarter of a century since I last was here, the message I gave local agriculturists then is one I am tempted to repeat now. It is no use to treat the vast territories you have at your disposal as if they were a mere wheat mine to be exploited in all haste and without regard to its permanence and its future profitable development. It is unwise to proceed as if bread were the only item of food requiring attention at your hands, and to regard a spasmodic rush of grain for a limited number of years from a poorly tilled surface as the only way to profitable returns. The stale maxim of not carrying all your eggs in one basket has a very profound truth to rest upon. The farming of the future must ultimately be one of more careful tillage, more scientific rotations, and of consideration for the changes in the grouping of population and in the world-wide conditions of man and his varying wants. What is going on all over the world has to be learned and studied well, and wheat pioneers of the North-West must not forget the possibility of yet new competitors arising in the single task of wheat-growing, whether they are to be looked for in the still developing sections of the Russian Empire and the still open levels of Argentina, the little known regions of Manchuria, the basin of the Tigris and Euphrates, the more completely irrigated plains of India, the tablelands of Central Africa, or perhaps under new conditions and a more developed control of the reserves of water supply on the southern shores of the Mediterranean or even in the long tilled valley of the Nile.

British Association for the Advancement of :

WINNIPEG, 1909.

ADDRESS TO THE EDUCATIONAL SCIENCE SECTION

BY

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The Educational Factors of Imperialism.

AMONG all civilised races and in all epochs of the world's history there has existed an inveterate belief that the particular age in which men live is fundamentally distinct from those that have preceded it.

Even in the most stagnant periods the illusion has prevailed that *the present day* is a period of flux and movement more or less organic, and as such either to be welcomed or to be deplored.

Notoriously difficult, however, as it is to gauge the temper of an age while we live in its midst, yet the phenomena in England at the beginning of the twentieth century seem so unmistakably marked, that even a superficial thinker can hardly fail to recognise the spheres in which the symptoms of change and unrest are clearly operating. They are surely in these two—the sphere of education and the sphere of Imperial sentiment.

It may not appear inapposite, therefore, if, meeting as we do in this city of phenomenal growth and infinite enterprise, our thoughts were to be directed in my inaugural address on the Science of Education towards discovering what may be either called the Imperial factors in education, or conversely, and perhaps more properly, the educational factors in Imperialism.

It may be perhaps safely said in this great Dominion what might possibly be disputed in the academic groves of our ancient English universities, that there was no width of educational outlook within our own little island until the last thirty years of the nineteenth century.

The only strongholds of learning which presumed to give the lead to English secondary education were to be found on the banks of the Isis and the Cam. In these antique, I hesitate to say antiquated, fastnesses, the 'grand old fortifying classical curriculum' was, till lately, regarded as the main, if not the only, highroad to educational salvation. They preserved,

indeed they preserve to this day, almost the same entrance bars against admission to their thresholds as existed in pre-Reformation days. And, conformably with the pursuit of these ideal studies, the vast mass of their emoluments were, and still are, appropriated to the pursuit of the ancient models of education.

The result of this monopoly on the lower rungs of the educational ladder has been obvious, and, to a scientific thinker, lamentable. The *curricula* of the Public Secondary schools have been narrowed, or rather have never been widened coincidently with the development of new spheres of knowledge and enterprise. The students in those institutions have been dominated from above, for just as 'where the carcass is, there will the eagles be gathered together,' so where the emoluments have been, thither do the cleverest students concentrate their intellectual forces.

The ambition of the ablest boys has been inevitably and exclusively concentrated on a single line of study, and (as often happens in the minds of the young) other no less humane but entirely unendowed departments of human knowledge have been laughed down and despised. Opprobrious epithets even have been bestowed on the study of the natural sciences, while those modern linguistic achievements which opened the door to the treasures of French and German literature are still nothing accounted of in the great schools of England.

But (more marvellous than all) even the scientific acquisition of and familiarity with the literature of the mother tongue have been entirely neglected, because no room could be found for it in a time-table, three-quarters of which is confined for the great mass of boy students in the historic schools of England (whatever their tastes and capabilities) to the exclusive study of the grammar, literature, and composition in the languages of ancient Greece and Rome. And the particular methods pursued in this confined curriculum have rendered the course more straitened still. The acquisition of the literatures of the two dead languages and of the great thoughts buried with them have given place to a meticulous study of the subtleties of scholarship, and students are taught to wanton in the abnormalities of the words and phrases in which those literatures were enshrined, so that in the mind of the classical scholar the form has become, or at any rate became till quite lately, more important than the substance.

Nor is this all. Those who cannot find any stomach for such drenching doses of mediæval learning are actually driven away prematurely as lost souls from those moss-grown seats of learning, which we acclaim as the great public schools of England; and, with moral characters only half-fledged, have either been condemned to the limbo of private tuition or sent as 'submerged tenths' to find, or lose, their fortunes in the great dependencies and dominions of the Empire like that in which I am speaking to-day. There has been no serious attempt made till the twentieth century by the leaders of our best-known places of secondary education to discover the bents and aptitudes of the boys committed to their charge and to give them any educational chance, if they have not possessed that particular kind of perception which could find its way through the subtleties of a Euripides or a Horace. Boys have been entirely denied the opportunity of showing their mental powers in any other sphere of learning. How many unsung Hampdens or mute inglorious Miltons of mechanical genius have been lost to the world by the non-elastic systems prevailing (even now) in our best-known educational institutions, is a tremendous responsibility for conscientious trainers of the young to contemplate and atone for.

In how many, or rather how few, places of learning in England, at the present time, can the establishment of scientifically equipped carpentering

and engineering shops be found, in which a young mind which finds it impossible to digest the crude morsels of Latin and Greek grammar can find resource and development? In how few schools has the connection between mind and hand and eye been scientifically trained? Such establishments, even in the first decade of the twentieth century, can be counted on the fingers of one hand.

And yet, in spite of it all, the surprising fact remains—a fact which speaks volumes for the innate vigour and originality of the English race—that, out of the stream of young men which flows out annually from our public schools¹ and colleges, so many accommodate themselves as happily as they do to the startlingly new conditions which confront them when they pass over the seas, and swell the tide of population in great centres of industry and enterprise such as that in which we stand to-day. Their educational vision, however, has had such a narrow and limited horizon that no wonder a large proportion are not very adaptable to the practical life of the prairie and the forest, or even of the counting-house and the office stool. Am I, or am I not correct in hazarding the conjecture that many specimens of this really fine English breed from the old country come to you here in this Dominion without an elementary knowledge of the laws of the world in which they live, full of antiquated prejudice and tradition, derived principally from the straitened area of their island-home experience, so that not seldom they put their hand to the plough (either literally or metaphorically) and look back, becoming wastrels instead of forceful citizens in this ever-widening Empire. 'No English need apply' has been, if I mistake not, written as a memorandum inside the breast of more than one leader of industry in this great continent, and small wonder is it when the cramping character of the ultra-mediaeval training which our young men have received at some of our historical public secondary schools in England is taken into account.

What remedy (you may ask) have I to propose? My answer is this: I want to force upon the attention of English educationists certain Imperial factors which should occupy an indispensable place in the educational curricula of the great schools in the Mother Country.

I would give a prominent place to the scientific teaching of geography, and particularly to historical geography, with special reference, of course, to the origin, growth, and progress of the British Empire. Such a volume as the 'Sketch of a Historical Geography,' by Keith Johnston, should be placed in the hands of every boy, and be known by him from cover to cover. It can hardly be realised that in many of our great classical schools to this day not more than one or at most two hours a week are devoted to this subject, and that it is often not taught at all beyond the middle classes in a school.

Again, I would enforce an elementary knowledge of science on every boy who passes through the stage of secondary education.

I am aware that many hard things have been said about the teaching of science in secondary education. A learned professor, who is the President of another Section of the Association, has passed his opinion that, as taught in our schools, it has proved of little practical or educational value. But because the methods employed have been halting, insufficient, and unscientific, it by no means follows that it should be left out of the category of school subjects. On the contrary, it appears astounding that two-thirds of the public school boys of England should grow to man's estate without even an elementary knowledge of the laws of the world in which they live.

¹ It should be noted in the forefront of this address that the expression 'public schools' is used throughout in its English (not in its more proper and American) sense—i.e., as the educational centres of the upper classes.

Lord Avebury, in his Presidential Address at the International Moral Education Meeting held in London last autumn, told his audience an amusing story of how, walking back one beautiful summer night from the House of Commons arm-in-arm with a leading luminary on the Government benches, his companion, who had been at Eton and Oxford, gazing at the greater luminary in the heavens, pensively observed: 'I wonder, my dear Lubbock, whether we shall ever know why the moon changes her shape once a week at least?'

To one who aspires to seek his fortune in the wide and half-unexplored continents of Greater Britain the value of the knowledge of chemistry, geology, botany, and arboriculture, can hardly be over-estimated. And yet many present here could bear critical witness to the fact that a large proportion of young men go out to the North-West totally unequipped after their public school training with even the most elementary knowledge of those departments of science to which I have alluded. No wonder, again, 'No English need apply.' Every youth we export to you ought educationally to bear this label on his back: 'Every seed tested before being sent out.'

But above and beyond all there should be brought into the foreground a co-ordinated study of English language and English literature. Nothing impressed me more in my visit to the United States in 1903 as one of the Mosely Commission, than to observe how greatly the cultivated classes in the Federation outstripped our island-bred people in the facility and power with which they manipulated the English tongue. Awkwardness, poverty of expression, and stammering utterance mark many Englishmen of high academic distinction. But the American who, on account of the incessant tide of immigration, has to assimilate the congeries of all the nations of the earth in the shortest possible space of time, has so co-ordinated the study of his ancestral tongue in the schools of his country, that the pupil emerges completely equipped for the use of persuasive and oratorical language wherein to express his thoughts and wherewith to gain his ends.

In connection with this may I add that it was, indeed, a happy augury that, at the eve of the meeting of the British Association in this great Dominion, there should have been a gathering of delegates of the Imperial Press in the centre of our small island home. 'Little they know of England, who only England know.' The phenomenal, or rather abysmal ignorance of the geography and of the vastness of the productive power of the British Empire which exists among the upper and middle classes in England would be ludicrous if it were not so deplorable. The loyalty and devotion of the Colonies, right unto the utmost corners of the earth, admit of no dispute. It is observable on every hand and in every national crisis. The doubt is of the loyalty of the centre of the Empire towards its extremities, through the crass ignorance which exists as to the geographical and political meaning of that Empire. I would annihilate that ignorance, as aforesaid, by putting political, historical, and physical geography in the forefront of our educational system; by lectures from your able men in Canada, or Australia, and South Africa, vivified by lantern slides, and encouraged and endowed by the Mother Country. I would bring all visible means of presentment to bear on the education of childhood, boyhood, and youth in the Motherland.

Let me touch on one further educational factor of Imperialism. The sentiment of patriotism, unlike that of charity, is not equally capable of indefinite intension and extension. The peculiar system of education which finds vogue in England in most of our greatest institutions--the institutions from which are drawn the future leaders of the nation--is, as everyone knows, the barrack system, otherwise called the boarding system. It is not the time or place here to enlarge on the obvious advantages of that system,

its unique characteristics, its power of moulding character and developing enterprise. But it has its cramping and confining side—it has a tendency to localise patriotism, to narrow a young man's mental horizon, and to ignore whatever lies outside its immediate survey. Hence the abnormal and gladiatorial devotion to games and comparatively selfish amusements, which absorb, and, in my opinion, not seldom paralyse and stifle wider, more generous, more enlightened—in fine, more Imperial instincts. However much in the field of sports the individual youth may subordinate his own self-regarding impulses to the welfare of the tiny community for which he is exercising his energies, his horizon is not wide enough to bid him rise to a sentiment of self-sacrifice and self-abandonment on behalf of a greater and more abstract ideal—love of Fatherland and loyalty to Empire.

But it is a welcome thing to be able to point to a larger sentiment lately awakened in this direction. There is no doubt that the patriotic spirit in our schools and colleges has, from whatever cause, received a great impetus in the last two years, and that the general principles of an intelligent defence of our shores from foreign aggression have been taught and construed into terms of scientific training and co-operative action with a rapidity equally surprising and welcome to those who, a few years ago, looked with something more than apprehension on the supineness of the youth of England in all patriotic regards.

‘The flannelled fool and muddled oaf,’

though they have not yet received their quietus, have been less rampant lately in our educational institutions, and something like an Imperial instinct, born of increasing knowledge both of the glory and dangers of our vast Empire, has, at least in the more cultured classes, taken the place of apathy, disregard, and ignorance. In hours formerly lavished to an abnormal extent on trivial amusements, and even in hours hitherto devoted to more academically intellectual training, we find young men in our schools and colleges now with arms in their hands, shooting, signalling, scouting, and studying scientifically the art of defensive warfare. This at least is ‘a beam in darkness, of which we pray that it may grow.’

Time and your patience will not allow me to touch on more than the fringe of the great educational problems which have to be solved before we can approach in English education to what I venture to call the ideal of Imperial responsibility.

In criticising the old mediæval system of education which prevailed in England till comparatively recent years, and which still has far too great a hold on the more venerable and important institutions of our island home, I would not have you suppose that I am an advocate of a complete, or even approximately complete, basis of utilitarian education. It is an easy charge for those who desire *stare super antiquas rias*, to throw in one's teeth. I have little hesitation in expressing my belief that the time has come (and I speak as one whose training was that of a classical scholar, for I was brought up in the strictest sect of academical Pharisees)—I say I have no hesitation in expressing my belief that the time has come, not only that the study of the two ancient languages should be reduced to one for all except scholastic specialists, but also that both should yield pride of place in our educational system to the claims of English, modern languages, mathematics, natural science, and, not least, manual training, so that our young men should be fitly equipped to put their hand to any work which may confront them amid all the complex problems and critical situations to be found within the world-wide boundaries of the British Empire.

Germany, France, and the United States have been beforehand with us in the working out of such a reformed system of education. I am by no

means one of those who believe that we should be wise in copying the methods in their entirety of any of these three peoples in their educational methods. Undoubtedly in all three there has been a more organised connection between the actual teaching given in their respective schools and the industrial, social, and political needs of the respective peoples. But no one nation is exactly like another nation in its temper and genius, and I should be sorry to advocate, for instance, the highly organised system of State education in Germany, under which it could be predicted to a certainty that boys and girls in every secondary or primary school on any given Friday morning should be studying (say) the geographical importance of Natal or the outlines of the coast of Lincolnshire. There must be many educational differences, because the idiosyncrasies of each nation differ from those of another, and I do not think we need ever fear that our intrinsic individuality will be crushed into any Teutonic cast-iron mould or ground down beneath the heel of some bureaucratic educational despotism. But that we ought to change our ways still more than we have, and adopt saner educational models, many searchings of heart through a long educational career have gradually, but overwhelmingly, convinced me. If we are apt to think, speak, and act Imperially, our education must take form from a strong Imperial sentiment, and must aim at instilling Imperial instincts in the young lives which that education is meant to control and develop.

I have spoken hitherto of this subject mainly from the point of view of secondary education, with which I am the most conversant; not only for that reason, however, but because most of those who are destined to proceed to the distant outlying parts of the British Empire, and, when there, to take prominent parts in the development of that Empire, obtain their educational equipment from the secondary schools of England. It is therefore, on curricula offered or desiderated in them that I have exclusively dwelt. But I do not blink the fact that the proper educational organisation of our elementary schools, on the one hand, and of our universities on the other, exercises a large influence on the solution of Imperial problems.

On elementary education, however, I do not propose to touch in this address, mainly because I look forward to experts in primary schools directing the thoughts of this Association more directly to them. But I will touch with great brevity on the subject of University education.

Whether Oxford and Cambridge—particularly Oxford—will ever so reform themselves as to contribute largely to such solution remains to be seen. Personally, I look with far greater confidence to the more recently organised universities—those of London, Leeds, Sheffield, Manchester, and the like—to equip men educationally with those moral, physical, and intellectual qualities which are most in requisition in our great dependencies and commonwealths.

Such institutions, from their newness, their eagerness, their freedom from antiquated prejudices and vested interests, are more likely to be counted upon for many years to come to send forth a stream of young men who have learned in the school of hardness to face the difficulties and to adapt themselves to the austere conditions which are inseparable from life in unworked regions and half-discovered continents. And it is at once a hopeful and inspiring thought that the great Dominion of Canada will welcome such to herself as sufficient and efficient citizens of her all but boundless territories, that she will recognise in them 'bone of her bone and flesh of her flesh,' physically, mentally, and morally capable, in company with those of her own sons who have long settled in the land, of extending the borders of the Empire by enlarging its resources, and of lifting, securing, and consolidating thereby the destinies of the Anglo-Saxon race.

PRESIDENTIAL ADDRESS.

There is still one more educational factor on which I would ask attention before I close this address. It is this—the necessity of a closer touch educationally (in the sense of ‘academically’) between the secondary schools and colleges of the Mother Country and similar institutions in the great Dominion and commonwealths which own her parentage. How this can be effected without great modification of our existing English system it is hard to see. But one point is quite clear. We must give up that part of our system which insists on choking the passage of the student from point to point in his educational career by subjecting him to countless examinations on entrance and throughout his academical course. It would be of incalculable advantage to the Empire at large if an extension of educational intercommunion, such as was inaugurated by the noble benefactions of the late Cecil Rhodes, could be secured throughout the Empire. Undoubtedly examination would be the surest test for determining the question of the admission of a student to the privileges of further education, if such examination could be conducted within a limited geographical area. But it is quite an impossible system if adopted as between the outlying parts of a great empire. The United States of America have taught us a better way. For instance, in the State of Minnesota, the university has legislated that if and when the Principal of a high school of recognised position certifies that a student has successfully pursued for a specified length of time those studies in that high school that would entitle him to admission to the university, he should be admitted thereto without further delay or hindrance. What a paralysing curse the Charybdis of examination has been to all true learning, only those who have suffered from it for thirty years can bear adequate testimony. It would be one of the most fertilising sources from which to secure good and progressive citizens, if instead of admitting within her borders all or any who came of their own spontaneity or from compulsion (leaving their country perchance for their country’s good) the Government authorities in the Dominion could get into closer touch with the educational authorities of the Mother Country, who would act as guarantee that the material sent out by the Mother Country should be of an approved and first-rate quality. This might be worked on the American ‘accredited school’ system, under which the authorities of the school sending the pupil should feel the *maximum* of responsibility in recommending his admission to the academical, or the technical, or the industrial organisations existing in the Dominion.

Since penning the first sentences of the above paragraph last June my eye has been caught by a notice which appeared in the columns of the ‘Times’ on the 28th day of that month while I was engaged in the very act of correcting the proofs of this address; but I prefer to leave the paragraph written as it stands, as the notice in question is an eloquent commentary on my suggestion of educational intercommunion.

I may, perhaps, be allowed to read the extract from the ‘Times’ *verbatim*, though it may be familiar to some at least among my audience. It is headed ‘International Interchange of Students—a New Movement.’

‘We have received,’ says the ‘Times,’ ‘the following interesting particulars of a new educational movement to provide for the interchange of University students among the English-speaking peoples:—

‘The object is to provide opportunities for as many as possible of the educated youth of the United Kingdom, Canada, and the United States (who, it is reasonable to suppose, will become leaders in thought, action, civic and national government in the future) to obtain some real insight into the life, customs, and progress of other nations at a time when their own opinions are

forming, with a *minimum* of inconvenience to their academic work and the least possible expense, with a view to broadening their conceptions and rendering them of greater economic and social value, such knowledge being, it is believed, essential for effectual leadership.

'The additional objects of the movement are to increase the value and efficiency of, as well as to extend, present University training by the provision of certain Travelling Scholarships for practical observation in other countries under suitable guidance. These scholarships will enable those students to benefit who might otherwise be unable to do so through financial restrictions. It also enables the administration to exercise greater power of direction in the form the travel is to take. In addition to academic qualifications, the selected candidate should be what is popularly known as an "all-round" man; the selection to be along the lines of the Rhodes Scholarships.

'The further objects are to extend the influence of such education indirectly among the men who are not selected as scholars (through intercourse with those who have travelled) by systematic arrangements of the periods' eligibility while they are still undergraduates.

'To promote interest in imperial, international, and domestic relations, civic and social problems, and to foster a mutual sympathy and understanding imperially and internationally among students.

'To afford technical and industrial students facilities to examine into questions of particular interest to them in manufactures, etc., by observation in other countries and by providing them with introductions to leaders in industrial activity.

'To promote interest in travel as an educational factor among the authorities of Universities, with a view to the possibility of some kind of such training being included in the regular curricula.

'To promote interest in other Universities, their aims and student life, the compulsory physical training, and methods of working their ways through college, for example, being valuable points for investigation.

'To promote international interchange for academic work among English-speaking Universities; and, in the case of the British Empire, to afford facilities for students of one division to gain, under favourable circumstances, information relative to the needs, development, and potentialities of other divisions; and to promote an academic interchange of students among the Universities of the Empire.

'As already indicated, there is a widespread interest in the movements so far as the United Kingdom is concerned; while in Canada and the United States there is also a widespread recognition of the value of the scheme; and although committees have not been actually organised there as in this country, a very large body of the most prominent educationists are strongly in favour of the plan, and have promised their co-operation if the scheme is financed.

'It is proposed to establish two students' travelling bureaux, one in New York and one in London; an American secretary (resident in New York) and a British secretary (resident in London), both of whom shall be college men appointed to afford every facility to any graduate or undergraduate of any University who wishes to visit the United States, Canada, or the United Kingdom for the purpose of obtaining an insight into the student, national, and industrial life of those countries. The bureaux will undertake the work of providing information relating to United States, Canadian, British, and other English-speaking Universities for the use of students, undergraduates, and others. They will also provide information relating to educational tours of any description in English-speaking countries, and the arrangement of tours suitable to the needs of the inquirer with a view to his obtaining the greatest facilities for education with a *minimum* of expense. Furthermore it

will be their duty to provide information as to the best places for the study of educational, governmental, industrial, and social problems in the United States, Canada, the United Kingdom, and other parts of the Empire, as well as to provide introductions to leaders in the above-named spheres of activity, besides undertaking the organisation and conduct of special tours for educational purposes, if necessary.

'It is proposed to provide 28 travelling scholarships, 14 of these being available for Universities in the United Kingdom, 10 for Universities in America and four for Universities in Canada. The arrangements will be controlled by general committees, one for the United Kingdom and one for Canada and the United States, unless it is found necessary to inaugurate a separate committee for each of the latter.'

You will observe then that a scheme which I had ventured to suggest as being 'of incalculable advantage to the Empire', had, before I wrote the words quoted, been advocated entirely without my knowledge by a body of influential educational leaders in England, whose names were appended to the notice which I have read; and I need only add that it is quite certain that I am interpreting the sentiments of all here assembled in wishing God-speed to the development of the scheme, which seems likely to prove, if carried into effect, a great, if not the greatest, educational factor of Imperialism.

But it may be objected here, Is not your own horizon circumscribed? Why should educational ideals be limited, even by so extended a conception as Imperialism? Should not the ultimate aim of all education be, not the federation of one race only, but the federation of the world at large—the brotherhood of man?

I am not concerned to deny that such a lofty conception is the true end of all physical, moral, and mental training.

But if the master mind of a Milton was content to define true education to be 'that which fits a man to perform justly, skilfully and magnanimously all the offices, both public and private, of peace and war,' it may well suffice us if we extend our (at present) too narrow conceptions (the aim of which seems to be the cultivation of a mere island patriotism) to a sphere which has for its end the imperialistic sentiment of a whole race.

It may indeed be well doubted whether a race-sentiment is not an ultimate factor beyond which it is impossible in an imperfect world to go. Universal philanthropy in its most catholic sense is a sentiment which the limited conditions of the earth's surface seem to render impossible. As long as men's ambitions are an unlimited quantity, and as long as the habitable globe remains, as it ever must remain, a limited quantity, so long will the populations of the world be continually liable to shifting movements and frequent dislocations. Practical educationists, then, must inevitably confine the scientific consideration of aims and methods in education to the development of the highest interests of their race rather than of mankind at large.

And that being so, the last point on which I would insist in dealing with the educational factors of Imperialism is to emphasise the importance of what the educationists of the United States call 'civics' as the binding power which should fasten together all the separate educational faggots in any Imperial scheme of education—the duty of personal service to the State, the positive obligation which makes us all members incorporate in one Imperial system. In our love of individual freedom, in our jealousy of interference with our individual liberty of action, in our insular disregard and depreciation of intellectual forces working in our sister communities beyond

the seas, we have lost sight of this civic responsibility which has ever lain on our shoulders and from which we can never dissociate ourselves, so long as our Empire remains as part of our ancestral heritage.

It is this positive duty towards each other and our race beyond the seas which those who live in our island home have been slow in realising, and it has been a real blot on our educational system that such ideas as Imperial responsibility and Imperial necessities have not been inculcated in the young people in our schools and colleges. As an illustration, I may observe that it has been even debated and doubted in some responsible quarters in England, whether the Union Jack should wave over our educational institutions on the days of national festivity and national observance.

To sum up. By these, and other kindred means, I would urge a closer educational touch between the Mother Country and the Empire at large.

Long ago a great Minister was able to say: 'Our hold of the Colonies is in the close affection which grows from common names, from kindred blood, and from similar privileges. These are ties which, though light as air, are strong as links of iron.'

But times have changed. To-day we are confronted with the problems of a vast and complicated Empire—great commonwealths, great dominions, sundered from each other by long seas and half a world, and however closely science has geographically brought them together, we cannot in soul and sympathy, nor ultimately in destiny, remain attached, affiliated as mother and children should be, unless we grapple to each other and understand each other in the greatest of all interests—the educational training which we give to our children in the one part of our Empire, to make them suitable citizens in another.

In suggesting reforms and modifications in which this educational unity may best be expressed, forgive me if I have but touched, and touched inadequately, on the fringe of a great subject, the transcendent importance of which it requires no elaboration of mine to impress on the earnest attention of the people of this great Dominion—which great Dominion may I be allowed to salute, without flattery or favour, as the most favoured by natural beauty and by virgin wealth of all the children of our common Motherland? May I salute her in terms which formed the old toast with which the two greatest of our English public schools, Winchester and Eton, pledged each other when we met in our annual cricket contest: *Mater pulchra, filia pulchrior!*

British Association for the Advancement of Science.

SHEFFIELD, 1910.

ADDRESS

BY

THE REV. PROFESSOR T. G. BONNEY, Sc.D., LL.D., F.R.S.,
PRESIDENT.

THIRTY-ONE years have passed since the British Association met in Sheffield, and the interval has been marked by exceptional progress. A town has become a city, the head of its municipality a Lord Mayor; its area has been enlarged by more than one-fifth; its population has increased from about 280,000 to 479,000. Communication has been facilitated by the construction of nearly thirty-eight miles of electric tramways for home service and of new railways, including alternative routes to Manchester and London. The supplies of electricity, gas, and water have more than kept pace with the wants of the city. The first was just being attempted in 1879; the second has now twenty-three times as many consumers as in those days; the story¹ of the third has been told by one who knows it well, so that it is enough for me to say your water supply cannot be surpassed for quantity and quality by any in the kingdom. Nor has Sheffield fallen behind other cities in its public buildings. In 1897 your handsome Town Hall was opened by the late Queen Victoria; the new Post Office, appropriately built and adorned with material from almost local sources, was inaugurated less than two months ago. The Mappin Art Gallery commemorates the munificence of those whose name it bears, and fosters that love of the beautiful which Ruskin sought to awaken by his generous gifts. Last, but not least, Sheffield has shown that it could not rest satisfied till its citizens could ascend from their own doors to the highest rung of the educational ladder. Firth College, named after its generous founder, was born in the year of our last visit; in 1897 it received a charter as the University College of Sheffield, and in the spring of 1905 was created a University, shortly after which its fine new buildings were

opened by the late King; and last year its library, the generous gift of Dr. Edgar Allen, was inaugurated by his successor, when Prince of Wales. I must not now dwell on the great work which awaits this and other new universities. It is for them to prove that, so far from abstract thought being antagonistic to practical work, or scientific research to the labour of the factory or foundry, the one and the other can harmoniously co-operate in the advance of knowledge and the progress of civilisation.

You often permit your President on these occasions to speak of a subject in which he takes a special interest, and I prefer thus trespassing on your kindness to attempting a general review of recent progress in science. I do not, however, propose, as you might naturally expect, to discuss some branch of petrology; though for this no place could be more appropriate than Sheffield, since it was the birthplace and the lifelong home of Henry Clifton Sorby, who may truly be called the father of that science. This title he won when, a little more than sixty years ago, he began to study the structure and mineral composition of rocks by examining thin sections of them under the microscope.¹ A rare combination of a singularly versatile and active intellect with accurate thought and sound judgment, shrewd in nature, as became a Yorkshireman, yet gentle, kindly, and unselfish, he was one whom his friends loved and of whom this city may well be proud. Sorby's name will be kept alive among you by the Professorship of Geology which he has endowed in your University; but, as the funds will not be available for some time, and as that science is so intimately connected with metallurgy, coal-mining, and engineering, I venture to express a hope that some of your wealthier citizens will provide for the temporary deficiency, and thus worthily commemorate one so distinguished.

But to return. I have not selected petrology as my subject, partly because I think that the great attention which its more minute details have of late received has tended to limit rather than to broaden our views, while for a survey of our present position it is enough to refer to the suggestive and comprehensive volume published last year by Mr. A. Harker;² partly, also, because the discussion of any branch of petrology would involve so many technicalities that I fear it would be found tedious by a large majority of my audience. So I have preferred to discuss some questions relating to the effects of ice which had engaged my attention a dozen years before I attempted the study of rock slices. As much of my petrological work has been connected with mountain

¹ His subsequent investigations into the microscopic structure of steel and other alloys of iron, in the manufacture of which your city holds a foremost place, have been extended by Mr. J. E. Stead and others, and they, besides being of great value to industrial progress, have thrown important sidelights on more than one dark place in petrology.

² *The Natural History of Igneous Rocks* (1909).

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districts, it has been possible for me to carry on the latter without neglecting the former, and my study of ice-work gradually led me from the highlands into the lowlands.¹ I purpose, then, to ask your attention this evening to some aspects of the glacial history of Western Europe.

At no very distant geological epoch the climate in the northern part of the earth was much colder than it is at present. So it was also in the southern; but whether the two were contemporaneous is less certain. Still more doubtful are the extent and the work of the ice which was a consequence, and the origin of certain deposits on some northern lowlands, including those of our own islands: namely, whether they are the direct leavings of glaciers or were laid down beneath the sea by floating shore-ice and bergs. Much light will be thrown on this complex problem by endeavouring to ascertain what snow and ice have done in some region which, during the Glacial Epoch, was never submerged, and none better can be found for this purpose than the European Alps.

At the present day one school of geologists, which of late years has rapidly increased in number, claims for glaciers a very large share in the sculpture of that chain, asserting that they have not only scooped out the marginal lakes, as Sir A. Ramsay maintained full half a century ago, but have also quarried lofty cliffs, excavated great cirques, and deepened parts of the larger Alpine valleys by something like two thousand feet. The other school, while admitting that a glacier, under special circumstances, may hollow out a tarn or small lake and modify the features of rock scenery, declares that its action is abrasive rather than erosive, and that the sculpture of ridges, crags, and valleys was mainly accomplished in pre-glacial times by running water and the ordinary atmospheric agencies.

In all controversies, as time goes on, hypotheses are apt to masquerade as facts, so that I shall endeavour this evening to disentangle the two, and call attention to those which may be safely used in drawing a conclusion.

In certain mountain regions, especially those where strong limestones, granites, and other massive rocks are dominant, the valleys are often trench-like, with precipitous sides, having cirques or corries at their heads, and with rather wide and gently sloping floors, which occasionally descend in steps, the distance between these increasing with that from the watershed. Glaciers have unquestionably occupied many of these valleys, but of late years they have been supposed to have taken a large share in excavating them. In order to appreciate their action we must imagine the glens to be filled up and the district restored to its former condition of a more or less undulating upland. As the mean

¹ May I add that hereafter a statement of facts without mention of an authority means that I am speaking from personal knowledge.

temperature¹ declined snow would begin to accumulate in inequalities on the upper slopes. This, by melting and freezing, would soften and corrode the underlying material, which would then be removed by rain and wind, gravitation and avalanche. In course of time the hollow thus formed would assume more and more the outlines of a corrie or a cirque by eating into the hillside. With an increasing diameter it would be occupied, as the temperature fell, first by a permanent snowfield, then by the *névé* of a glacier. Another process now becomes important, that called 'sapping.' While ordinary glacier-scour tends, as we are told, to produce 'sweeping curves and eventually a graded slope,' 'sapping' produces 'benches and cliffs, its action being horizontal and backwards,' and often dominant over scour. The author of this hypothesis² convinced himself of its truth in the Sierra Nevada by descending a *bergschrand* 150 feet in depth, which opened out, as is so common, beneath the walls of a cirque. Beginning in the *névé*, it ultimately reached the cliff, so that for the last thirty feet the bold investigator found rock on the one hand and ice on the other. The former was traversed by fracture planes, and was in all stages of displacement and dislodgment; some blocks having fallen to the bottom, others bridging the narrow chasm, and others frozen into the *névé*. Clear ice had formed in the fissures of the cliff; it hung down in great stalactites; it had accumulated in stalagmitic masses on the floor. Beneath the *névé* the temperature would be uniform, so its action would be protective, except where it set up another kind of erosion, presently to be noticed; but in the chasm, we are informed, there would be, at any rate for a considerable part of the year, a daily alternation of freezing and thawing. Thus the cliff would be rapidly undermined and be carried back into the mountain slope, so that before long the glacier would nestle in a shelter of its own making. Farther down the valley the moving ice would become more effective than sub-glacial streams in deepening its bed; but since the *névé*-flow is almost imperceptible near the head, another agency must be invoked, that of 'plucking.' The ice grips, like a forceps, any loose or projecting fragment in its rocky bed, wrenches that from its place, and carries it away. The extraction of one tooth weakens the hold of its neighbours, and thus the glen is deepened by 'plucking,' while it is carried back by 'sapping.' Streams from melting snows on the slopes above the amphitheatre might have been expected to co-operate vigorously in making it, but of them little account seems to be taken, and we are even told that in some cases the winds probably prevented snow from resting on the rounded surface between two cirque-heads.³ As these receded,

¹ In the remainder of this Address 'temperature' is to be understood as mean temperature. The Fahrenheit scale is used.

² W. D. Johnson, *Science*, N.S., ix. (1899), pp. 106, 112.

³ This does not appear to have occurred in the Alps.

only a narrow neck would be left between them, which would be ultimately cut down into a gap or 'col.' Thus a region of deep valleys with precipitous sides and heads, of sharp ridges, and of more or less isolated peaks is substituted for a rather monotonous, if lofty, highland.

The hypothesis is ingenious, but some students of Alpine scenery think more proof desirable before they can accept it as an axiom. For instance, continuous observations are necessary to justify the assumption of diurnal variations of temperature sufficient to produce any sensible effect on rock at the bottom of a narrow chasm nearly fifty yards deep and almost enclosed by ice. Here the conditions would more probably resemble those in a *glacière*, or natural ice cave. In one of these, during the summer, curtains and festoons of ice depend from the walls; from them and from the roof water drips slowly, to be frozen into stalaginitic mounds on the floor, which is itself sometimes a thick bed of ice. On this the quantity of fallen rock *débris* is not greater than is usual in a cave, nor are the walls notably shattered, even though a gap some four yards deep may separate them from the ice. The floors of cirques, from which the *névé* has vanished, cannot as a rule be examined, because they are masked by *débris* which is brought down by the numerous cascades, little and big, which seam their walls; but glimpses of them may sometimes be obtained in the smaller corries (which would be cirques if they could), and these show no signs of either 'sapping' or 'plucking,' but some little of abrasion by moving ice. Cirques and corries also not infrequently occur on the sides as well as at the heads of valleys; such, for instance, as the two in the *massif* of the Uri Rothstock on the way to the Surenen Pass and the Fer à Cheval above Sixt. The Lago di Ritom lies between the mouth of a hanging valley and a well-defined step, and just above that is the Lago di Cadagno in a large, steep-walled corrie, which opens laterally into the Val Piora, as that of the Lago di Tremorgio does into the southern side of the Val Bedretto. Cirques may also be found where glaciers have had a comparatively brief existence, as the Creux des Vents on the Jura; or have never been formed, as on the slopes of Salina, one of the Lipari Islands, or in the limestone desert of Lower Egypt.¹ I have seen a miniature stepped valley carved by a rainstorm on a slope of Hampstead Heath; a cirque, about a yard in height and breadth, similarly excavated in the vertical wall of a gravel pit; and a corrie, measured by feet instead of furlongs, at the foot of one of the Binns near Burntisland, or, on a much reduced scale, in a bank of earth. On all these the same agent, plunging water, has left its marks—runlets of rain for the smaller, streams for the larger; convergent at first, perhaps, by accident,

¹ A. J. Jukes-Browne, *Geol. Mag.*, 1877, p. 477.

afterwards inevitably combined as the hollow widened and deepened. Each of the great cirques is still a 'land of streams,' and they are kept permanent for the greater part of the year by beds of snow on the ledges above its walls.

The 'sapping and plucking' process presents another difficulty—the steps already mentioned in the floors of valleys. These are supposed to indicate stages at which the excavating glacier transferred its operations to a higher level. But, if so, the outermost one must be the oldest, or the glacier must have been first formed in the lowest part of the incipient valley. Yet, with a falling temperature, the reverse would happen, for otherwise the snow must act as a protective mantle to the mature pre-glacial surface almost down to its base. However much age might have smoothed away youthful angularities, it would be strange if no receptacles had been left higher up to initiate the process; and even if sapping had only modified the form of an older valley, it could not have cut the steps unless it had begun its work on the lowest one. Thus, in the case of the Creux de Champ, if we hesitate to assume that the sapping process began at the mouth of the valley of the Grande Eau above Aigle, we must suppose it to have started somewhere near Ormont Dessus and to have excavated that gigantic hollow, the floor of which lies full 6,000 feet below the culminating crags of the Diablerets.

But even if 'sapping and plucking' were assigned a comparatively unimportant position in the cutting out of cirques and corries, it might still be maintained that the glaciers of the Ice Age had greatly deepened the valleys of mountain regions. That view is adopted by Professors Penck and Brückner in their work on the glaciation of the Alps,¹ the value of which even those who cannot accept some of their conclusions will thankfully admit. On one point all parties agree—that a valley cut by a fairly rapid stream in a durable rock is V-like in section. With an increase of speed the walls become more vertical; with a diminution the valley widens and has a flatter bed, over which the river, as the base-line is approached, may at last meander. Lateral streams will plough into the slopes, and may be numerous enough to convert them into alternating ridges and furrows. If a valley has been excavated in thick horizontal beds of rock varying in hardness, such as limestones and shales, its sides exhibit a succession of terrace walls and shelving banks, while a marked dip and other dominant structures produce their own modifications. It is also agreed that a valley excavated or greatly enlarged by a glacier should be U-like in section. But an Alpine valley, especially as we approach its head, very commonly takes the following form. For some hundreds of feet up from the torrent it is

¹ *Die Alpen in Eiszeitalter* (1909).

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a distinct V; above this the slopes become less rapid, changing, say, from 45° to not more than 30° , and that rather suddenly. Still higher comes a region of stone-strewn upland valleys and rugged crags, terminating in ridges and peaks of splintered rock, projecting from a mantle of ice and snow. The V-like part is often from 800 to 1,000 feet in depth; and the above-named authors maintain that this, with perhaps as much of the more open trough above, was excavated during the Glacial Epoch. Thus the floor of any one of these valleys prior to the Ice Age must often have been at least 1,800 feet above its present level.¹ As a rough estimate we may fix the deepening of one of the larger Pennine valleys, tributary to the Rhone, to have been, during the Ice Age, at least 1,600 feet in their lower parts. Most of them are now hanging valleys; the stream issuing, on the level of the main river, from a deep gorge. Their tributaries are rather variable in form; the larger as a rule being more or less V-shaped; the shorter, and especially the smaller, corresponding more with the upper part of the larger valleys; but their lips generally are less deeply notched. Whatever may have been the cause, this rapid change in slope must indicate a corresponding change of action in the erosive agent. Here and there the apex of the V may be slightly flattened, but any approach to a real U is extremely rare. The retention of the more open form in many small, elevated recesses, from which at the present day but little water descends, suggests that where one of them soon became buried under snow,² but was insignificant as a feeder of a glacier, erosion has been for ages almost at a standstill.

The V-like lower portion in the section of one of the principal valleys, which is all that some other observers have claimed for the work of a glacier, cannot be ascribed to subsequent modification by water, because ice-worn rock can be seen in many places, not only high up its sides, but also down to within a yard or two of the present torrent.

Thus valley after valley in the Alps seems to leave no escape from the following dilemma: Either a valley cut by a glacier does not differ in form from one made by running water, or one which has been excavated by the latter, if subsequently occupied, is but superficially modified by ice. This, as we can repeatedly see in the higher Alpine valleys, has not succeeded in obliterating the physical features due to the ordinary processes of erosion. Even where its effects are most striking, as

¹ The amount varies in different valleys; for instance, it was fully 2,880 feet at Amsteg on the Reuss, just over 2,000 feet at Brieg in the Rhone Valley, about 1,000 feet at Guttanen in the Aare Valley, about 1,550 feet above Zermatt, and 1,100 feet above Saas Grund.

² My own studies of mountain districts have led me to infer that on slopes of low grade the action of snow is preservative rather than destructive. That conclusion was confirmed by Professor Garwood in a communication to the Royal Geographical Society on June 20 of the present year.

in the Spitalamm below the Grimsel Hospice, it has not wholly effaced those features; and wherever a glacier in a recent retreat has exposed a rock surface, that demonstrates its inefficiency as a plough. The evidence of such cases has been pronounced inadmissible, on the ground that the glaciers of the Alps have now degenerated into senile impotence; but in valley beds over which they passed when in the full tide of their strength the flanks show remnants of rocky ridges only partly smoothed away, and rough rock exists on the 'lee-sides' of ice-worn mounds which no imaginary plucking can explain. The ice seems to have flowed over rather than to have plunged into the obstacles in its path, and even the huge steps of limestone exposed by the last retreat of the Unter Grindelwald Glacier have suffered little more than a rounding off of their angles, though that glacier must have passed over them when in fullest development, for it seems impossible to explain these by any process of sapping.

The comparatively level trough, which so often forms the uppermost part of one of the great passes across the watershed of the Alps, can hardly be explained without admitting that in each case the original watershed has been destroyed by the more rapid recession of the head of the southern valley, and this work bears every sign of having been accomplished in pre-glacial times. Sapping and plucking must have operated on a gigantic scale to separate the Viso from the Cottian watershed, to isolate the huge pyramid of the Matterhorn, with its western spur, or to make, by the recession of the Val Macugnaga, that great gap between the Strahlhorn and Monte Rosa. Some sceptics even go so far as to doubt whether the dominant forms of a non-glaciated region differ very materially from those of one which has been half-buried in snow-fields and glaciers. To my eyes, the general outlines of the mountains about the Lake of Gennesaret and the northern part of the Dead Sea recalled those around the Lake of Annecy and on the south-eastern shore of Leman. The sandstone crags, which rise here and there like ruined castles from the lower plateau of the Saxon Switzerland, resembled in outlines, though on a smaller scale, some of the Dolomites in the Southern Tyrol. The Lofoten Islands illustrate a half-drowned mountain range from which the glaciers have disappeared. Those were born among splintered peaks and ridges, which, though less lofty, rival in form the Aiguilles of Chamonix, and the valleys become more and more ice-worn as they descend, till the coast is fringed with skerries every one of which is a *roche moutonnée*. The *névé* in each of these valleys has been comparatively ineffective; the ice has gathered strength with the growth of the glacier. As can be seen from photographs, the scenery of the heart of the Caucasus or of the Himalayas differs in scale rather than in kind from that of the Alps. Thus the amount of abrasion varies, other things

being equal, with the latitude. The grinding away of ridges and spurs, the smoothing of the walls of troughs,¹ is greater in Norway than in the Alps; it is still greater in Greenland than in Norway, and it is greatest of all in the Antarctic, according to the reports of the expeditions led by Scott and Shackleton. But even in Polar regions, under the most favourable conditions, the dominant outlines of the mountains, as shown in the numerous photographs taken by both parties, and in Dr. Wilson's admirable drawings, differ in degree rather than in kind from those of mid-European ranges. It has been asserted that the parallel sides of the larger Alpine valleys—such as the Rhone above Martigny, the Lütischine near Lauterbrunnen, and the Val Bedretto below Airolo—prove that they have been made by the ice-plough rather than by running water; but in the first I am unable to discern more than the normal effects of a rather rapid river which has followed a trough of comparatively soft rocks; in the second, only the cliffs marking the channel cut by a similar stream through massive limestones—cliffs like those which elsewhere rise up the mountain flanks far above the levels reached by glaciers; while in the third I have failed to discover, after repeated examination, anything abnormal.

Many lake basins have been ascribed to the erosive action of glaciers. Since the late Sir A. Ramsay advanced this hypothesis numbers of lakes in various countries have been carefully investigated and the results published, the most recent of which is the splendid work on the Scottish lochs by Sir J. Murray and Mr. L. Pullar.² A contribution to science of the highest value, it has also a deeply pathetic interest, for it is a father's memorial to a much-loved son, F. P. Pullar, who, after taking a most active part in beginning the investigation, lost his life while saving others from drowning. As the time at my command is limited, and many are acquainted with the literature of the subject, I may be excused from saying more than that even these latest researches have not driven me from the position which I have maintained from the first—namely, that while many tarns in corries and lakelets in other favourable situations are probably due to excavation by ice, as in the mountainous districts of Britain, in Scandinavia, or in the higher parts of the Alps, the difficulty of invoking this agency increases with the size of the basin—as, for example, in the case of Loch Maree or the Lake of Annecy—till it becomes insuperable. Even if Glas Llyn and Llyn Llydaw were the work of a glacier, the rock basins of Gennesaret and the Dead Sea, still more those of the great lakes in North America and in Central Africa, must be assigned to other causes.

¹ If one may judge from photographs, the smoothing of the flanks of a valley is unusually conspicuous in Milton Sound, New Zealand.

² *Bathymetrical Survey of the Scottish Freshwater Lochs*. Sir J. Murray and Mr. L. Pullar, 1910.

I pass on, therefore, to mention another difficulty in this hypothesis—that the Alpine valleys were greatly deepened during the Glacial Epoch—which has not yet, I think, received sufficient attention. From three to four hundred thousand years have elapsed, according to Penck and Brückner, since the first great advance of the Alpine ice. One of the latest estimates of the thickness of the several geological formations assigns 4,000 feet¹ to the Pleistocene and Recent, 13,000 to the Pliocene, and 14,000 to the Miocene. If we assume the times of deposit to be proportional to the thicknesses, and adopt the larger figure for the first-named period, the duration of the Pliocene would be 1,300,000 years, and of the Miocene 1,400,000 years. To estimate the total vertical thickness of rock which has been removed from the Alps by denudation is far from easy, but I think 14,000 feet would be a liberal allowance, of which about one-seventh is assigned to the Ice Age. But during that age, according to a curve given by Penck and Brückner, the temperature was below its present amount for rather less than half (.47) the time. Hence it follows that, since the sculpture of the Alps must have begun at least as far back as the Miocene period, one-seventh of the work has been done by ice in not quite one-fifteenth of the time, or its action must be very potent. Such data as are at our command make it probable that a Norway glacier at the present day lowers its basin by only about eighty millimètres in 1,000 years; a Greenland glacier may remove some 421 millimètres in the same time, while the Vatnajökul in Iceland attains to 647 millimètres. If Alpine glaciers had been as effective as the last-named, they would not have removed, during their 188,000 years of occupation of the Alpine valleys, more than 121.6 metres, or just over 397 feet; and as this is not half the amount demanded by the more moderate advocates of erosion, we must either ascribe an abnormal activity to the vanished Alpine glaciers, or admit that water was much more effective as an excavator.

We must not forget that glaciers cannot have been important agents in the sculpture of the Alps during more than part of Pleistocene times. That sculpture probably began in the Oligocene period; for rather early in the next one the great masses of conglomerate, called *Nagelfluh*, show that powerful rivers had already carved for themselves valleys corresponding generally with and nearly as deep as those still in existence. Temperature during much of the Miocene period was not less than 12° F. above its present average. This would place the snow-line at about 12,000 feet.²

¹ I have doubts whether this is not too great.

² I take the fall of temperature for a rise in altitude as 1° F. for 300 feet or, when the differences in the latter are large, 3° per 1,000 feet. These estimates will, I think, be sufficiently accurate. The figures given by Hann (see for a discussion of the question, *Report of Brit. Assoc.*, 1909, p. 93) work out to 1° F. for 300 feet (up to about 10,000 feet).

In that case, if we assume the altitudes unchanged, not a snowfield would be left between the Simplon and the Maloja, the glaciers of the Pennines would shrivel into insignificance, Monte Rosa would exchange its drapery of ice for little more than a tippet of frozen snow. As the temperature fell the white robes would steal down the mountain-sides, the glaciers grow, the torrents be swollen during all the warmer months, and the work of sculpture increase in activity. Yet with a temperature even 6° higher than it now is, as it might well be at the beginning of the Pliocene period, the snow-line would be at 10,000 feet; numbers of glaciers would have disappeared, and those around the Jungfrau and the Finster Aarhorn would be hardly more important than they now are in the Western Oberland.

But denudation would begin so soon as the ground rose above the sea. Water, which cannot run off the sand exposed by the retreating tide without carving a miniature system of valleys, would never leave the nascent range intact. The Miocene Alps, even before a patch of snow could remain through the summer months, would be carved into glens and valleys. Towards the end of that period the Alps were affected by a new set of movements, which produced their most marked effects in the northern zone from the Inn to the Durance. The Oberland rose to greater importance; Mont Blanc attained its primacy; the *massif* of Dauphiné was probably developed. That, and still more the falling temperature, would increase the snowfields, glaciers, and torrents. The first would be, in the main, protective; the second, locally abrasive; the third, for the greater part of their course, erosive. No sooner had the drainage system been developed on both sides of the Alps than the valleys on the Italian side (unless we assume a very different distribution of rainfall) would work backwards more rapidly than those on the northern. Cases of trespass, such as that recorded by the long level trough on the north side of the Maloja Kulm and the precipitous descent on the southern, would become frequent. In the interglacial episodes—three in number, according to Penck and Brückner, and occupying rather more than half the epoch—the snow and ice would dwindle to something like its present amount, so that the water would resume its work. Thus I think it far more probable that the V-like portions of the Alpine valleys were in the main excavated during Pliocene ages, their upper and more open parts being largely the results of Miocene and yet earlier sculpture.

During the great advances of the ice, four in number, according to Penck and Brückner,¹ when the Rhone glacier covered the lowlands of Vaud and Geneva, welling on one occasion over the gaps in the Jura, and leaving its erratics in the neighbourhood of Lyons, it ought to have

¹ On the exact number I have not had the opportunity of forming an opinion.

given signs of its erosive no less than of its transporting power. But what are the facts? In these lowlands we can see where the ice has passed over the Molasse (a Miocene sandstone); but here, instead of having crushed, torn, and uprooted the comparatively soft rock, it has produced hardly any effect. The huge glacier from the Linth Valley crept for not a few miles over a floor of stratified gravels, on which, some eight miles below Zurich, one of its moraines, formed during the last retreat, can be seen resting, without having produced more than a slight superficial disturbance. We are asked to credit glaciers with the erosion of deep valleys and the excavation of great lakes, and yet, wherever we pass from hypotheses to facts, we find them to have been singularly inefficient workmen!

I have dwelt at considerable, some may think undue, length on the Alps, because we are sure that this region from before the close of the Miocene period has been above the sea-level. It accordingly demonstrates what effects ice can produce when working on land.

In America also, to which I must now make only a passing reference, great ice-sheets formerly existed: one occupying the district west of the Rocky Mountains, another spreading from that on the north-west of Hudson's Bay, and a third from the Laurentian hill-country. These two became confluent, and their united ice-flow covered the region of the Great Lakes, halting near the eastern coast a little south of New York, but in Ohio, Indiana, and Illinois occasionally leaving moraines only a little north of the 39th parallel of latitude.¹ Of these relics my first-hand knowledge is very small, but the admirably illustrated reports and other writings of American geologists² indicate that, if we make due allowance for the differences in environment, the tills and associated deposits on their continent are similar in character to those of the Alps.³

In our own country and in corresponding parts of Northern Europe we must take into account the possible co-operation of the sea. In these, however, geologists agree that, for at least a portion of the Ice Age, glaciers occupied the mountain districts. Here ice-worn rocks, moraines and perched blocks, tarns in corries, and perhaps lakelets in valleys, demonstrate the former presence of a mantle of snow and ice. Glaciers radiated outwards from more than one focus in Ireland, Scotland, the English Lake District, and Wales, and trespassed, at the time

¹ Some of the glacial drifts on the eastern side of the continent, as we shall find, may have been deposited in the sea.

² See the *Reports of the United States Geological Survey* (from vol. iii. onwards), *Journal of Geology*, *American Journal of Science*, and local publications too numerous to mention. Among these the studies in Greenland by Professor Chamberlin are especially valuable for the light they throw on the movement of large glaciers and the transport of *débris* in the lower part of the ice.

³ Here, however, we cannot always be so sure of the absence of the sea.

of their greatest development, upon the adjacent lowlands. They are generally believed to have advanced and retreated more than once, and their movements have been correlated by Professor J. Geikie with those already mentioned in the Alps. Into that very difficult question I must not enter; for my present purpose it is enough to say that in early Pleistocene times glaciers undoubtedly existed in the mountain districts of Britain and even formed piedmont ice-sheets on the lowlands. On the west side of England, smoothed and striated rocks have been observed near Liverpool, which can hardly be due to the movements of shore-ice, and at Little Crosby a considerable surface has been cleared from the overlying boulder clay by the exertions of the late Mr. T. M. Reade and his son, Mr. A. Lyell Reade. But, so far as I am aware, rocks thus affected have not yet been discovered in the Wirral peninsula. On the eastern side of England similar markings have been found down to the coast of Durham, but a more southern extension of land ice cannot be taken for granted. In this direction, however, so far as the tidal valley of the Thames, and in corresponding parts of the central and western lowlands, certain deposits occur which, though to a great extent of glacial origin, are in many respects different from those left by land ice in the Alpine regions and in Northern America.

They present us with problems the nature of which may be inferred from a brief statement of the facts. On the Norfolk coast we find the glacial drifts resting, sometimes on the chalk, sometimes on strata of very late Pliocene or early Pleistocene age. The latter show that in their time the strand-line must have oscillated slightly on either side of its present level. The earliest of the glacial deposits, called the Cromer Till and Contorted Drift, presents its most remarkable development in the cliffs on either side of that town. Here it consists of boulder clays and alternating beds of sand and clay; the first-named, two or three in number, somewhat limited in extent, and rather lenticular in form, are slightly sandy clays, full of pieces of chalk, flint, and other kinds of rock, some of the last having travelled from long distances. Yet more remarkable are the huge erratics of chalk, in the neighbourhood of which the sands and clays exhibit extraordinary contortions. Like the beds of till, they have not been found very far inland, for there the group appears as a whole to be represented by a stony loam, resembling a mixture of the sandy and clayey material, and this is restricted to a zone some twenty miles wide, bordering the coast of Norfolk and Suffolk; not extending south of the latter county, but being probably represented to the north of the Humber. Above these is a group of false-bedded sands and gravels, variable in thickness and character—the Mid-glacial Sands of Searles V. Wood and F. W. Harmer. They extend over a wider area,

and may be traced, according to some geologists, nearly to the western side of England, rising in that direction to a greater height above sea-level. But as it is impossible to prove that all isolated patches of these materials are identical in age, we can only be certain that some of them are older than the next deposit, a boulder clay, which extends over a large part of the lowlands in the Eastern Counties. This has a general resemblance to the Cromer Till, but its matrix is rather more clayey and is variable in colour. In and north of Yorkshire, as well as on the seaward side of the Lincolnshire wolds, it is generally brownish or purplish, but on their western side and as far as the clay goes to the south it is some shade of grey. Near to these wolds, in mid-Norfolk, and on the northern margin of Suffolk, it has a whitish tint, owing to the abundance of comminuted chalk. To the south and west of this area it is dark, from the similar presence of Kimeridge clay. Yet further west it assumes an intermediate colour by having drawn upon the Oxford clay. This boulder clay, whether the chalky or the purple, in which partings of sand sometimes occur, must once have covered, according to Mr. F. W. Harmer, an area about ten thousand square miles in extent. It spreads like a coverlet over the pre-glacial irregularities of the surface. It caps the hills, attaining sometimes an elevation of fully 500 feet above sea-level;¹ it fills up valleys,² sometimes partly, sometimes wholly, the original floors of which occasionally lie more than 100 feet below the same level. This boulder clay, often with an underlying sand or gravel, extends to the south as far as the neighbourhood of Muswell Hill and Finchley; hence its margin runs westward through Buckinghamshire, and then, bending northwards, passes to the west of Coventry. On this side of the Pennine Chain the matrix of the boulder clay is again reddish, being mainly derived from the sands and marls of the Trias; pieces of chalk and flint are rare (no doubt coming from Antrim), though other rocks are often plentiful enough. Some authorities are of opinion that the drift in most parts of Lancashire and Cheshire is separable, as on the eastern coasts, into a lower and an upper boulder clay, with intervening gravelly sands, but others think that the association of the first and third is lenticular rather than suc-

¹ Not far from Royston it is found at a height of 525 feet above O.D. See F. W. Harmer, *Pleistocene Period in the Eastern Counties*, p. 115.

² At Old North Road Station, on a tributary of the Cam, the boulder clay was pierced to a depth of 180 feet, and at Impington it goes to 60 feet below sea-level. Near Hitchin, a hidden valley, traced for seven or eight miles, was proved to a depth of 68 feet below O.D., and one near Newport in Essex, to 140 feet. Depths were also found of 120 feet at West Horseheath in Suffolk, of 120 feet on low ground two miles S.W. of Sandy in Bedfordshire, of from 100 to 160 feet below the sea at Fossdyke, Long Sutton, and Boston, and at Glemsford in the valley of the Stour, 477 feet of drift was passed through before reaching the chalk. See F. W. Harmer, *Quart. Journ. Geol. Soc.*, lxiii. (1907), p. 494.

cessive. Here also the lower clay cannot be traced very far inland, eastward or southward; the others have a wider extension, but they reach a greater elevation above sea-level than on the eastern side of England. The sand is inconstant in thickness, being sometimes hardly represented, sometimes as much as 200 feet. The upper clay runs on its more eastern side up to the chalky boulder clay, and extends on the south at least into Worcestershire. On the western side it merges with the upper member of the drifts radiating from the mountains of North Wales, which often exhibit a similar tripartite division, while (as we learn from the officers of the Geological Survey) boulder clays and gravelly sands, which it must suffice to mention, extend from the highlands of South Wales for a considerable distance to the south-east and south. Boulder clay has not been recognised in Devon or Cornwall, though occasional erratics are found which seem to demand some form of ice-transport. A limited deposit, however, of that clay, containing boulders now and then over a yard in diameter, occurs near Selsey Bill on the Sussex coast, which most geologists consider to have been formed by floating rather than by land ice.

Marine shells are not very infrequent in the lower clays of East Anglia and Yorkshire, but are commonly broken. The well-known Bridlington Crag is the most conspicuous instance, but this is explained by many geologists as an erratic—a piece of an ancient North Sea bed caught up and transported, like the other molluscs, by an advancing ice-sheet. They also claim a derivative origin for the organic contents of the overlying sands and gravels, but some authorities consider the majority to be contemporaneous. Near the western coast of England, shells in much the same state of preservation as those on the present shore are far from rare in the lower clay, where they are associated with numerous striated stones, often closely resembling those which have travelled beneath a glacier, both from the Lake District and the less distant Trias. Shells are also found in the overlying sands up the valleys of the Dee and Severn, at occasional localities, even as far inland as Bridgnorth, the heights of the deposits varying from about 120 feet to over 500 feet above the sea-level. If we also take account of the upper boulder clay, where it can be distinguished, the list of marine molluscs, ostracods, and foraminifers from these western drifts is a rather long one.¹

Marine shells, however, on the western side of England, are not restricted to the lowlands. Three instances, all occurring over 1,000 feet above sea-level, claim more than a passing mention. At Macclesfield, almost thirty miles in a straight line from the head of the estuary of the Mersey, boulder clays associated with stratified gravels

¹ W. Shone, *Quart. Journ. Geol. Soc.*, xxxiv. (1878), p. 383.

and sands have been described by several observers.¹ The clay stops at about 1,000 feet, but the sands and gravels go on to nearly 1,300 feet, while isolated erratics are found up to about 100 feet higher. Sea shells, some of which are in good condition, have been obtained at various elevations, the highest being about 1,200 feet above sea-level. About forty-eight species of molluscs have been recognised, and the fauna, with a few exceptions, more arctic in character and now found at a greater depth, is one which at the present day lives in a temperate climate at a depth of a few fathoms.

The shell-bearing gravels at Gloppa, near Oswestry, which are about thirty miles from the head of the Dee estuary, were carefully described in 1892 by Mr. A. C. Nicholson. He has enumerated fully sixty species, of which, however, many are rare. As his collection² shows, the bivalves are generally broken, but a fair number of the univalves are tolerably perfect. The deposit itself consists of alternating seams of sand and gravel, the one generally about an inch in thickness, the other varying from a few inches to a foot. The difference in the amount of rounding shown by the stones is a noteworthy feature. They are not seldom striated; some have come from Scotland, others from the Lake District, but the majority from Wales, the last being the more angular. Here and there, a block, sometimes exceeding a foot in diameter and usually from the last-named country, has been dropped among the smaller material, most of which ranges in diameter from half an inch to an inch and a half. The beds in one or two places show contortions; but as a rule, though slightly wavy and with a gentle dip rather to the west of south, they are uniformly deposited. In this respect, and in the unequal wearing of the materials, the Gloppa deposit differs from most gravels that I have seen. Its situation also is peculiar. It is on the flattened top of a rocky spur from higher hills, which falls rather steeply to the Shropshire lowland on the eastern side, and on the more western is defined by a small valley which enlarges gradually as it descends towards the Severn. If the country were gradually depressed for nearly 1,200 feet, this upland would become, first a promontory, then an island, and finally a shoal.

The third instance, on Moel Tryfaen in Carnarvonshire, was carefully investigated and described by a Committee of this Association³ about ten years ago. The shells occur in an irregularly stratified sand and gravel, resting on slate, and overlain by a boulder clay, no great

¹ *Memoirs of the Geological Survey*: 'Country around Macclesfield,' T. I. Pocock (1906), p. 85. For some notes on Moel Tryfaen and the altitudes of other localities at which marine organisms have been found see J. Gwyn Jeffreys, *Quart. Journ. Geol. Soc.*, xxxvi. (1880), p. 351. For the occurrence of such remains in the Vale of Clwyd see a paper by T. McK. Hughes in *Proc. Chester Soc. of Nat. Hist.*, 1884.

² Now deposited in the Oswestry Museum.

³ *Brit. Assoc. Report*, 1899 (1900), pp. 414-423.

distance from and a few dozen feet below the rocky summit of the hill, being about 1,300 feet above the level of the sea and at least five miles from its margin. About fifty-five species of molluscs and twenty-three of foraminifers have been identified. According to the late Dr. J. Gwyn Jeffreys,¹ the majority of the molluscs are littoral in habit, the rest such as live in from ten to twenty fathoms of water. Most of the erratics have been derived from the Welsh mountains, but some rocks from Anglesey have also been obtained, and a few pebbles of Lake District and Scotch rocks. If the sea were about 1,300 feet above its present level, Moel Tryfaen would become a small rocky island, open to the storms from the west and north, and nearly a mile and a half away from the nearest land.

I must pass more rapidly over Ireland. The signs of vanished glaciers—ice-worn rocks and characteristic boulder-clays—are numerous, and may be traced in places down to the sea-level, but the principal outflow of the ice, according to some competent observers, was from a comparatively low district, extending diagonally across the island from the south of Lough Neagh to north of Galway Bay. Glaciers, however, must have first begun to form in the mountains on the northern and southern side of this zone, and we should have expected that, whatever might happen on the lowlands, they would continue to assert themselves. In no other part of the British Islands are eskers, which some geologists think were formed when a glacier reached the sea, so strikingly developed. Here also an upper and a lower boulder clay, the former being the more sparsely distributed, are often divided by a widespread group of sands and gravels, which locally, as in Great Britain, contains, sometimes abundantly, shells and other marine organisms; more than twenty species of molluscs, with foraminifers, a barnacle, and perforations of annelids, having been described. These are found in counties Dublin and Wicklow, at various altitudes,² from a little above sea-level to a height of 1,300 feet.

Not the least perplexing of the glacial phenomena in the British Isles is the distribution of erratics, which has been already mentioned in passing. On the Norfolk coast, masses of chalk, often thousands of cubic feet in volume, occur in the lowest member of the glacial series, with occasional great blocks of sand and gravel, which must have once been frozen. But these, or at any rate the larger of them, have no doubt been derived from the immediate neighbourhood. Huge erratics also occasionally occur in the upper boulder clay—sometimes of chalk, as at Roslyn Hill near Ely and at Ridlington in Rutland, of jurassic limestone, near Great Ponton, to the south of Grantham,

¹ *Quart. Journ. Geol. Soc.*, xxxvi. (1880), p. 355.

² See T. M. Reade, *Proc. Liverpool Geol. Soc.*, 1893-94, p. 183, for some weighty arguments in favour of a marine origin for these deposits.

and of Lower Kimeridge clay near Biggleswade.¹ These also probably have not travelled more than a few miles. But others of smaller size have often made much longer journeys. The boulder clays of Eastern England are full of pieces of rock, commonly ranging from about half an inch to a foot in diameter. Among these are samples of the carboniferous, jurassic, and cretaceous rocks of Yorkshire and the adjacent counties; the red chalk from either Hunstanton, Speeton, or some part of the Lincolnshire wolds, being found as far south as the northern heights of London. Even the chalk and flint, the former of which, especially in the upper boulder clay, commonly occurs in well-worn pebbles, are frequently not the local but the northern varieties. And with these are mingled specimens from yet more distant sources—Cheviot porphyrites, South Scotch basalts, even some of the crystalline rocks of the Highlands. Whatever was the transporting agent, its general direction was southerly, with a slight deflection towards the east in the last-named cases.

But the path of these erratics has been crossed by two streams, one coming from the west, the other from the east. On the western side of the Pennine watershed the Shap granite rises at Wasdale Crag to a height of about 1,600 feet above sea-level. Boulders from it have descended the Eden valley to beyond Penrith; they have travelled in the opposite direction almost to Lancaster,² and a large number of them have actually made their way near the line of the Lake District watershed, across the upper valley of the Eden, and over the high pass of Stainmoor Forest,³ whence they descended into Upper Teesdale. Subsequently the stream seems to have bifurcated, one part passing straight out to the present sea-bed, by way of the lower course of the Tees, to be afterwards driven back on to the Yorkshire coast. The other part crossed the low watershed between the Tees and the Ouse, descended the Vale of York and spread widely over the plain.⁴ Shap boulders by some means penetrated into the valleys tributary to the Ouse on its west bank, and they have been observed as far to the south-east as Royston, near Barnsley. It is noteworthy that Lake District rocks have been occasionally recorded from Airedale and even the neighbourhood of Colne, though the granite from Shap has not been found there. The other stream started from Scandinavia. Erratics, some of which must have come from the north-western side of the Christiania Fjord, occur on or near the coast from Essex to Yorkshire, and occasionally

¹ H. Home, *Quart. Journ. Geol. Soc.*, lix. (1903), p. 375.

² A pebble of it is said to have been identified at Moel Tryfan.

³ The lowest part of the gap is about 1,400 feet. A little to the south is another gap about 200 feet lower, but none of the boulders seem to have taken that route.

⁴ A boulder was even found above Gosmont in the Eske valley, 345 feet above sea-level.

even as far north as Aberdeen, while they have been traced from the East Anglian coast to near Ware, Hitchin, and Bedford.¹ It may be important to notice that these Scandinavian erratics are often waterworn, like those dispersed over Denmark and parts of Northern Germany.

On the western side of England the course of erratics is not less remarkable. Boulders from South-Western Scotland, especially from the Kirkcudbright district, both waterworn and angular, are scattered over the lowlands as far south as Wolverhampton, Bridgnorth, and Church Stretton. They may be traced along the border of North Wales, occurring, as has been said, though generally small, up to about 1,300 feet on Moel Tryfaen, 1,100 feet at Gloppe, and more than that height on the hills east of Macclesfield. Boulders from the Lake District are scattered over much the same area and attain the same elevation, but extend, as might be expected, rather farther to the east in Lancashire. They also have been found on the eastern side of the Pennine watershed, perhaps the most remarkable instances being in the dales of the Derbyshire Derwent and on the adjacent hills as much as 1,400 feet above the sea-level.² A third remarkable stream of erratics from the neighbourhood of the Arenig mountains extends from near the estuary of the Dee right across the paths of the two streams from the north, its eastern border passing near Rugeley, Birmingham, and Bromsgrove. They also range high, occurring almost 900 feet above sea-level on Romsley Hill, north of the Clents, and being common at Gloppe. Boulders also from the basalt mass of Rowley Regis have travelled in some cases between four and five miles, and in directions ranging from rather west of south to north-east; and, though that mass hardly rises above the 700-foot contour line, one lies with an Arenig boulder on Romsley Hill. From Charnwood Forest, the crags of which range up to about 850 feet above sea-level, boulders have started which have been traced over an area to the south and west to a distance of more than twenty miles.

Such, then, are the facts, which call for an interpretation. More than one has been proposed; but it will be well, before discussing them, to arrive at some idea of the climate of these islands during the colder part of the Glacial Epoch. Unless that were associated with very great changes in the distribution of sea and land in Northern and North-Western Europe, we may assume that neither the relative position of the isotherms nor the distribution of precipitation would be very materially altered. A general fall of temperature in the northern hemisphere might so weaken the warmer ocean current from the south-west that our coasts might be approached by a cold one from the

¹ R. H. Rastall and J. Romanes, *Quart. Journ. Geol. Soc.*, lxx. (1909), p. 246.

² Communication from Dr. H. Arnold-Bemrose.

opposite direction.¹ But though these changes might diminish the difference between the temperatures of London and Leipzig, they would not make the former colder than the latter. At the present day the snow-line in the Alps on either side of the Upper Rhone Valley is not far from 8,000 feet above sea-level, and this corresponds with a temperature of about 30°. Glaciers, however, are not generally formed till about 1,000 feet higher, where the temperature is approximately 27°. Penck and Brückner place this line during the coldest part of the Ice Age at about 4,000 feet.² In that case the temperature of the Swiss lowland would be some 15° lower than now, or near the freezing point.³ If this fall were general, it would bring back the small glaciers on the Gran Sasso d'Italia and Monte Rotondo in Corsica; perhaps also among the higher parts of the Vosges and Schwarzwald.⁴ In our own country it would give a temperature of about 35° at Carnarvon and 28° on the top of Snowdon, of 32° at Fort William and 17.5° on the top of Ben Nevis. If, in addition to this, the land were 600 feet higher than now (as it probably was, at any rate in the beginning of the Glacial Epoch), there would be a further drop of 2°, so that glaciers would form in the corries of Snowdon, and the region round Ben Nevis might resemble the Götthel Alps at the present day. This change of itself would be insufficient, and any larger drop in the ocean-level would have to be continental in its effects, since we cannot assume a local upheaval of much more than the above amount without seriously interfering with the river system of North Central Europe. But these changes, especially the former, might indirectly diminish the abnormal warmth of winter on our north-western coasts.⁵ It is difficult to estimate the effect of this. If it did no more than place Carnarvon on the isotherm of Berlin (now lower by 2°), that would hardly bring a glacier from the Snowdonian region down to the sea. At the present time London is about 18° warmer than a place in the same latitude near the Labrador coast or the mouth of the Amur River, but the removal of that difference would involve greater changes in the distribution of sea and land than seems possible at an epoch comparatively speaking so recent.

¹ Facts relating to this subject will be found in *Climate and Time*, by J. Croll, ch. ii. and iii. (1875). Of course the air currents would also be affected, and perhaps diminish precipitation as the latitude increased.

² *Loc. cit.*, p. 586, *et seq.* They say the snow-line, which would mean that the temperature was only 12° lower than now; but as possibly this line might then more nearly correspond with that of glacier formation, I will provisionally accept the higher figures, especially since Corsica, the Apennines, and some other localities in Europe, seem to require a reduction of rather more than 12°.

³ It would be 32.5° at Zurich, 31.6° at Bern, 34.1° at Geneva, about 39.0° on the plain of Piedmont, and 36.0° at Lyons.

⁴ See for particulars the author's *Ice Work* ('International Scientific Series'), p. 237.

⁵ For much valuable information on these questions see a paper on the Climate of the Pleistocene Epoch (F. W. Harmer, *Quart. Journ. Geol. Soc.*, lvii. (1901), p. 405).

I am doubtful whether we can attribute to changed currents a reduction in British temperatures of so much as 11° ; but, if we did, this would amount to 28° from all causes, and give a temperature of 20° to 22° at sea-level in England, during the coldest part of the Glacial Epoch.¹ That is now found, roughly speaking, in Spitzbergen, which, since its mountains rise to much the same height, should give us a general idea of the condition of Britain in the olden time.

What would then be the state of Scandinavia? Its present temperature ranges on the west coast from about 45° in the south to 35° in the north.² But this region must now be very much, possibly 1,800 feet, lower than it was in pre-glacial, perhaps also in part of glacial, times.³ If we added 5° for this to the original 15° , and allowed so much as 18° for the diversion of the warm current, the temperature of Scandinavia would range from 7° to -3° , approximately that of Greenland northwards from Upernivik. But since the difference at the present day between Cape Farewell and Christiania (the one in an abnormally cold region, the other in one correspondingly warm) is only 7° , that allowance seems much too large, while without it Scandinavia would correspond in temperature with some part of that country from south of Upernivik to north of Frederikshaab.⁴ But if Christiania were not colder than Jakobshavn is now, or Britain than Spitzbergen, we are precluded from comparisons with the coasts of Baffin Bay or Victoria Land.

Thus the ice-sheet from Scandinavia would probably be much greater than those generated in Britain. It would, however, find an obstacle to progress westwards, which cannot be ignored. If the bed of the North Sea became dry land, owing to a general rise of 600 feet, that would still be separated from Norway by a deep channel, extending from the Christiania Fjord round the coast northward. Even then this would be everywhere more than another 600 feet deep, and almost as wide as the Strait of Dover.⁵ The ice must cross this and afterwards be forced for more than 300 miles up a slope, which, though gentle, would be in vertical height at least 600 feet. The task, if accomplished by

¹ The present temperature in Ireland over the zone (from S. of Belfast to N. of Galway Bay) which is supposed to have formed the divide of the central snowfield may be given as from 49° to 50° , nearly the same as at the sea-level in Carnarvonshire. Thus, though the district is less mountainous than Wales, it would not need a greater reduction, for the snowfall would probably be rather larger. But this reduction could hardly be less than 20° , for the glaciers would have to form nearly at the present sea-level.

² It is 44.42° at Borgen, 38.48° at Bodo, 35.42° at Hammerfest, 41.36° at Christiania and Stockholm.

³ For particulars see *Geol. Mag.*, 1899, p. 97 (W. H. Hudleston) and p. 282 (T. G. Bonney).

⁴ Christiania and Cape Farewell (Greenland) are nearly on the same latitude.

⁵ For details see *Geol. Mag.*, 1899, pp. 97 and 282.

thrust from behind, would be a heavy one, and, so far as I know, without a parallel at the present day; if the viscosity of the ice enabled it to flow, as has lately been urged,¹ we must be cautious in appealing to the great Antarctic barrier, because we now learn that more than half of it is only consolidated snow.² Moreover, if the ice floated across that channel, the thickness of the boulder-bearing layers would be diminished by melting (as in Ross's Barrier), and the more viscous the material, the greater the tendency for these to be left behind by the overflow of the cleaner upper layers. If, however, the whole region became dry land, the Scandinavian glaciers would descend into a broad valley, considerably more than 1,200 feet deep, which would afford them an easy path to the Arctic Ocean, so that only a lateral overflow, inconsiderable in volume, could spread itself over the western plateau.³ An attempt to escape this difficulty has been made by assuming the existence of an independent centre of distribution for ice and boulders near the middle of the North Sea bed⁴ (which would demand rather exceptional conditions of temperature and precipitation); but in such case either the Scandinavian ice would be fended off from England, or the boulders, prior to its advance, must have been dropped by floating ice on the neighbouring sea-floor.

If, then, our own country were but little better than Spitzbergen as a producer of ice, and Scandinavia only surpassed Southern Greenland in having a rather heavier snowfall, what interpretation may we give to the glacial phenomena of Britain? Three have been proposed. One asserts that throughout the Glacial Epoch the British Isles generally stood at a higher level, so that the ice which almost buried them flowed out on to the beds of the North and Irish Seas. The boulder clays represent its moraines. The stratified sands and gravels were deposited in lakes formed by the rivers which were dammed up by ice-sheets.⁵ A second interpretation recognises the presence of glaciers in the mountain regions, but maintains that the land, at the outset rather above its present level, gradually sank beneath the sea, till the depth of water over the eastern coast of England was fully 500 feet, and

¹ H. M. Deeley, *Geol. Mag.*, 1909, p. 239.

² E. Shackleton, *The Heart of the Antarctic*, ii. 277.

³ It has indeed been affirmed (Brögger, *Om de senglaciale og postglaciale nirsforandringer i Kristianafjeldet*, p. 682) that at the time of the great ice-sheet of Europe the sea-bottom must have been uplifted at least 8,500 feet higher than at present. This may be a ready explanation of the occurrence of certain dead shells in deep water, but, unless extremely local, it would revolutionise the drainage system of Central Europe.

⁴ *Geol. Mag.*, 1901, pp. 142, 187, 284, 332.

⁵ See Warren Upham, *Monogr. U.S. Geol. Survey*, xxv. (1896). This explanation commends itself to the majority of British geologists as an explanation of the noted parallel roads of Glenroy, but it is premature to speak of it as 'conclusively shown' (*Quart. Journ. Geol. Soc.*, lviii. (1902), 473) until a fundamental difficulty which it presents has been discussed and removed.

over the western nearly 1,400 feet, from which depression it slowly recovered. By any such submergence Great Britain and Ireland would be broken up into a cluster of hilly islands, between which the tide from an extended Atlantic would sweep eastwards twice a day, its currents running strong through the narrower sounds, while movements in the reverse direction at the ebb would be much less vigorous. The third interpretation, in some respects intermediate, was first advanced by the late Professor Carvill Lewis, who held that the peculiar boulder clays and associated sands (such as those of East Anglia), which, as was then thought, were not found more than about 450 feet above the present sea-level, had been deposited in a great fresh-water lake, held up by the ice-sheets already mentioned and by an isthmus, which at that time occupied the place of the Strait of Dover. Thus, these deposits, though indirectly due to land-ice, were actually fluvial or lacustrine. But this interpretation need not detain us, though the former existence of such lakes is still maintained, on a small scale in Britain, on a much larger one in North America, because, as was pointed out when it was first advanced, it fails to explain the numerous erratic blocks and shell-bearing sands which occur far above the margin of the hypothetical lake.

Each of the other two hypotheses involves grave difficulties. That of great confluent ice-sheets creeping over the British lowlands demands, as has been intimated, climatal conditions which are scarcely possible, and makes it hard to explain the sands and gravels, sometimes with regular alternate bedding, but more generally indicative of strong current action, which occur at various elevations to over 1,300 feet above sea-level, and seem too widespread to have been formed either beneath an ice-sheet or in lakes held up by one; for the latter, if of any size, would speedily check the velocity of influent streams. Also the mixture and crossing of boulders, which we have described, are inexplicable without the most extraordinary oscillations in the size of the contributing glaciers. To suppose that the Scandinavian ice reached to Bedfordshire and Herts and then retired in favour of North British glaciers, or *vice versa*, assumes an amount of variation which, so far as I am aware, is without a parallel elsewhere. So also the mixture of boulders from South Scotland, the Lake District, and North Wales which lie, especially in parts of Staffordshire and Shropshire, as if dropped upon the surface, far exceeds what may reasonably be attributed to variations amplified by lateral spreading of mountain glaciers on reaching a lowland, while the frequent presence of shells in the drifts, dozens of miles away from the present coast, implies a rather improbable scooping up of the sea-bed without much injury to such fragile objects. The ice also must have been curiously inconstant in

its operations. It is supposed in one place to have glided gently over its bed, in another to have gripped and torn out huge masses of rock.¹ Both actions may be possible in a mountain region, but it is very difficult to understand how they could occur in a lowland or plain. Besides this we can only account for some singular aberrations of boulders, such as Shap granite well above Grosmont in Eskdale, or the Scandinavian rhomb-porphry above Lockwood,² near Huddersfield, by assuming a flexibility in the lobes of an ice-sheet which it is hard to match at the present time. Again, the boulder clay of the eastern counties is crowded, as we have described, with pebbles of chalk, which generally are not of local origin, but have come from north of the Wash. Whether from the bed of a river or from a sea-beach, they are certainly water-worn. But if preglacial, the supply would be quickly exhausted, so that they would usually be confined to the lower part of the clay. As it is, though perhaps they run larger here, they abound throughout. The so-called moraines near York (supposed to have been left by a glacier retreating up that vale), those in the neighbourhood of Flamborough Head and of Sheringham (regarded as relics of the North Sea ice-sheet) do not, in my opinion, show any important difference in outline from ordinary hills of sands and gravels, and their materials are wholly unlike those of any indubitable moraines that I have either seen or studied in photographs. It may be said that the British glaciers passed over very different rocks from the Alpine; but the Swiss molasse ought to have supplied abundant sand, and the older interglacial gravels quantities of pebbles; yet the differences between the morainic materials on the flank of the Jura or near the town of Geneva and those close to the foot of the Alps are varietal rather than specific.

Some authorities, however, attribute such magnitude to the ice-sheets radiating from Scandinavia that they depict them, at the time of maximum extension, as not only traversing the North Sea bed and trespassing upon the coast of England, but also radiating southward to overwhelm Denmark and Holland, to invade Northern Germany and Poland, to obliterate Hanover, Berlin, and Warsaw, and to stop but little short of Dresden and Cracow, while burying Russia on the east to within no great distance of the Volga and on the south to the neighbourhood of Kief. Their presence, however, so far as I can ascertain, is inferred from evidence³ very similar to that which we have discussed in the

¹ That this has occurred at Cromer is a very dubious hypothesis (see *Geol. Mag.*, 1905, pp. 397, 524). The curious relations of the drift and chalk in the islands of Mön and Rügen are sometimes supposed to prove the same action. Knowing both well, I have no hesitation in saying that the chalk there is, as a rule, as much *in situ* as it is in the Isle of Wight.

² About half-way across England and 810 feet above sea-level. P. F. Kendall, *Quart. Journ. Geol. Soc.*, lviii. (1902), p. 498.

³ A valuable summary of it is given in *The Great Ice Age*, J. Geikie, ch. xxix., xxx. (1894).

British lowlands. That Scandinavia was at one time almost wholly buried beneath snow and ice is indubitable; it is equally so that at the outset the land stood above its present level, and that during the later stages of the Glacial Epoch parts, at any rate of Southern Norway, had sunk down to a maximum depth of 800 feet. In Germany, however, erratics are scattered over its plain and stranded on the slopes of the Harz and Riesengebirge up to about 1,400 feet above sea-level. The glacial drifts of the lowlands sometimes contain dislodged masses of neighbouring rocks like those at Cromer, and we read of other indications of ice action. I must, however, observe that since the glacial deposits of Möen, Warnemünde, and Rügen often present not only close resemblances to those of our eastern counties but also very similar difficulties, it is not permissible to quote the one in support of the other, seeing that the origin of each is equally dubious. Given a sufficient 'head' of ice in northern regions, it might be possible to transfer the remains of organisms from the bed of the Irish Sea to Moel Tryfaen, Macclesfield, and Gloppe; but at the last-named, if not at the others, we must assume the existence of steadily alternating currents in the lakes in order to explain the corresponding bedding of the deposit. This, however, is not the only difficulty. The 'Irish Sea glacier' is supposed to have been composed of streams from Ireland, South-West Scotland, and the Lake District, of which the second furnished the dominant contingent; the first-named not producing any direct effect on the western coast of Great Britain, and the third being made to feel its inferiority and 'shouldered in upon the mainland.' But even if this ever happened, ought not the Welsh ice to have joined issue with the invaders a good many miles to the north of its own coast?¹ Welsh boulders at any rate are common near the summit of Moel Tryfaen, and I have no hesitation in saying that the pebbles of riebeckite-rock, far from rare in its drifts, come from Mynydd Mawr, hardly half a league to the E.S.E., and not from Ailsa Craig.²

As such frequent appeal is made to the superior volume of the ice-sheet which poured from the Northern Hills over the bed of the Irish Sea, I will compare in more detail the ice-producing capacities of the

¹ From Moel Tryfaen to the nearest point of Scotland is well over a hundred miles, and it is a few less than this distance from Gloppe to the Lake District. In order to allow the Irish Sea ice-sheet to reach the top of Moel Tryfaen the glacier productive power of Snowdonia has been minimised (Wright, *Man and the Glacial Epoch*, pp. 171, 172). But the difference between that and the Arenig region is not great enough to make the one incompetent to protect its own borderland while the other could send an ice-sheet which could almost cover the Clent Hills and reach the neighbourhood of Birmingham. Anglesey also, if we suppose a slight elevation and a temperature of 29° at the sea-level, would become a centre of ice-distribution and an advance guard to North Wales.

² The boulders of picrite near Porth Nobla, from Llanerchymedd, though they have travelled southward, have moved away much to the west.

several districts. The present temperature of West-Central Scotland may be taken as 47° ; its surface as averaging about 2,500 feet, rising occasionally to nearly 4,000 feet above sea-level. In the western part of the Southern Uplands the temperature is a degree higher, and the average for altitude at most not above 1,500 feet. In the Lake District and the Northern Pennines the temperature is increased by another degree, and the heights are, for the one 1,800 feet with a maximum of 3,162 feet, for the other 1,200 feet and 2,892 feet. In North Wales the temperature is 50° , the average height perhaps 2,000 feet, and the culminating point 3,571 feet. For the purpose of comparing the ice-producing powers of these districts we may bring them to one temperature by adding 300 feet to the height for each degree below that of the Welsh region. This would raise the average elevation of Central and Southern Scotland to 3,400 feet and 2,100 feet respectively; for the Lake District and Northern Pennines to 2,100 feet and 1,500 feet. We may picture to ourselves what this would mean, if the snow-line were at the sea-level in North Wales, by imagining 8,000 feet added to its height and comparing it with the Alps. North Wales would then resemble a part of that chain which had an average height of about 10,000 feet above sea-level, and culminated in a peak of 11,571 feet; the Lake District would hardly differ from it; the Northern Pennines would be like a range of about 9,000 feet, its highest peak being 11,192 feet. Southern Scotland would be much the same in average height as the first and second, and would rise, though rarely, to above 11,000 feet; the average in Central Scotland would be about 11,400 feet, and the maximum about 13,000 feet. Thus, North Wales, the Lake District, and the Southern Uplands would differ little in ice-productive power; while Central Scotland would distinctly exceed them, but not more than the group around the Finsteraarhorn does that giving birth to the Rhone glacier. In one respect, however, all these districts would differ from the Alps—that, at 8,000 feet, the surface, instead of being furrowed with valleys, small and great, would be a gently shelving plateau, which would favour the formation of piedmont glaciers. Still, unless we assume the present distribution of rainfall to be completely altered (for which I do not know any reason), the relative magnitudes of the ice coming from these centres (whether separate glaciers or confluent sheets) could differ but little. Scotch ice would not appreciably ‘shoulder inland’ that from the Lake District, nor would the Welsh ice be imprisoned within its own valleys.

During the last few years, however, the lake-hypothesis of Carvill Lewis has been revived under a rather different form by some English advocates of land-ice. For instance, the former presence of ice-dammed lakes is supposed to be indicated in the upper parts of the Cleveland Hills by certain overflow channels. I may be allowed to

observe that, though this view is the outcome of much acute observation and reasoning,¹ it is wholly dependent upon the ice-barriers already mentioned, and that if they dissolve before the dry light of sceptical criticism, the lakes will 'leave not a rack behind.' I must also confess that to my eyes the so-called 'overflow channels' much more closely resemble the remnants of ancient valley-systems, formed by only moderately rapid rivers, which have been isolated by the trespass of younger and more energetic streams, and they suggest that the main features of this picturesque upland were developed before rather than after the beginning of the Glacial Epoch. I think that even 'Lake Pickering,' though it has become an accepted fact with several geologists of high repute, can be more simply explained as a two-branched 'valley of strike,' formed on the Kimeridge clay, the eastern arm of which was beheaded, even in preglacial times, by the sea.² As to Lake Oxford,³ I must confess myself still more sceptical. Some changes no doubt have occurred in later glacial and postglacial times; valleys have been here raised by deposit, there deepened sometimes by as much as 100 feet; the courses of lowland rivers may occasionally have been altered; but I doubt whether, since those times began, either ice-sheet or lake has ever concealed the site of that University city.

The submergence hypothesis assumes that, at the beginning of the Glacial Epoch, our islands stood rather above their present level, and during it gradually subsided, on the west to a greater extent than on the east, till at last the movement was reversed, and they returned nearly to their former position. During most of this time glaciers came down to the sea from the more mountainous islands, and in winter an ice-foot formed upon the shore. This, on becoming detached, carried away boulders, beach pebbles, and finer detritus. Great quantities of the last also were swept by swollen streams, into the estuaries and spread over the sea-bed by coast currents, settling down especially in the quiet depths of submerged valleys. Shore-ice in Arctic regions, as Colonel H. W. Feilden⁴ has described, can striate stones and even the rock beneath it, and is able, on a subsiding area, gradually to push boulders up to a higher level. In fact the state of the British region in those ages would not have been unlike that still existing near the coasts of the Barents and Kara Seas. Over the submerged region southward, and in some cases more or less eastward, currents would

¹ P. F. Kendall, *Quart. Journ. Geol. Soc.*, lviii. (1902), 471.

² See for instance the courses of the Medway and the Beult over the Weald clay (C. Le Neve Foster and W. Topley, *Quart. Journ. Geol. Soc.*, xxi. (1865), p. 443).

³ F. W. Harmer, *Quart. Journ. Geol. Soc.*, lxiii. (1907), p. 470.

⁴ *Quart. Journ. Geol. Soc.*, xxxiv. (1878), p. 556.

be prevalent; though changes of wind¹ would often affect the drift of the floating ice-rafts. But though the submergence hypothesis is obviously free from the serious difficulties which have been indicated in discussing the other one, gives a simple explanation of the presence of marine organisms, and accords with what can be proved to have occurred in Norway, Waigatz Island, Novaia Zemlya, on the Lower St. Lawrence, in Grinnell Land, and elsewhere,² it undoubtedly involves others. One of them—the absence of shore terraces, caves, or other sea marks—is perhaps hardly so grave as it is often thought to be. It may be met by the remark that unless the Glacial Age lasted for a very long time and the movements were interrupted by well-marked pauses, we could not expect to find any such record. In regard also to another objection, the rather rare and sporadic occurrence of marine shells, the answer would be that, on the Norway coast, where the ice-worn rock has certainly been submerged, sea-shells are far from common and occur sporadically in the raised deltaic deposits of the fjords.³ An advocate of this view might also complain, not without justice, that, if he cited an inland terrace, it was promptly dismissed as the product of an ice-dammed lake, and his frequent instances of marine shells in stratified drifts were declared to have been transported from the sea by the lobe of an ice-sheet; even if they have been carried across the path of the Arénig ice, more than forty miles, as the crow flies, from the Irish Sea up the Valley of the Severn, or forced some 1,300 feet up Moel Tryfaen.⁴ The difficulty in the latter case, he would observe, is not met by saying the ice-sheet would be able to climb that hill 'given there were a sufficient head behind it.'⁵ That ice can be driven uphill has long been known, but the existence of the 'sufficient head' must be demonstrated, not assumed. There may be 'no logical halting-place between an uplift of ten or twenty feet to surmount a *roche moulonnée* and an equally gradual

¹ See p. 23, and for the currents now dominant consult Dr. H. Bassett in Professor Herdman's Report on the Lancashire Sea Fisheries, *Trans. Biol. Soc. Liverpool*, xxiv. (1910), p. 123.

² See *Ice Work*, p. 221, and *Geol. Mag.*, 1900, p. 289.

³ If, as seems probable, the temperature was changing rather rapidly the old fauna would be pauperised and the new one make its way but slowly into the British fjords.

⁴ Critics of the submergence hypothesis seem to find a difficulty in admitting downward and upward movements, amounting sometimes to nearly 1,400 feet during Pleistocene Ages; but in the northern part of America the upheaval, at any rate, has amounted to about 1,000 feet, while on the western coast, beneath the lofty summit of Mount St. Elias, marine shells of existing species have been obtained some 5,000 feet above sea-level. It is also admitted that in several places the pre-glacial surface of the land was much above its present level. On the Red River, whatever be the explanation, foraminifers, radiolarians, and sponge spicules have been found at 700 feet above sea-level, and near Victoria, on the Saskatchewan, even up to about 1,900 feet.

⁵ P. F. Kendall in Wright's *Man and the Glacial Period*, p. 171.

elevation to the height of Moel Tryfaen,' yet there is a common-sense limitation, even to a destructive *sortes*. The argument, in fact, is more specious than valid, till we are told approximately how thick the northern ice must be to produce the requisite pressure, and whether such an accumulation would be possible. The advocates of land ice admit that, before it had covered more than a few leagues on its southward journey its thickness was less than 2,000 feet, and we are not entitled, as I have endeavoured to show, to pile up ice indefinitely on either our British highlands or the adjacent sea-bed. The same reason also forbids us largely to augment the thickness of the latter by the snowfall on its surface, as happens to the Antarctic barrier ice. Even if the thickness of the ice-cap over the Dumfries and Kirkcudbright hills had been about 2,500 feet, that, with every allowance for viscosity, would hardly give us a head sufficient to force a layer of ice from the level of the sea-bed to a height of nearly 1,400 feet above it and at a distance of more than 100 miles.

Neither can we obtain much support from the instance in Spitzbergen, described by Professors Garwood and Gregory, where the Ivory Glacier, after crossing the bed of a valley, had transported marine shells and drift from the floor (little above sea-level) to a height of about 400 feet on the opposite slope. Here the valley was narrow, and the glacier had descended from an inland ice-reservoir, much of which was at least 2,800 feet above the sea, and rose occasionally more than a thousand feet higher.¹

But other difficulties are far more grave. The thickness of the chalky boulder clay alone, as has been stated, not unfrequently exceeds 100 feet, and, though often much less, may have been reduced by denudation. This is an enormous amount to have been transported and distributed by floating ice. The materials also are not much more easily accounted for by this than by the other hypothesis. A continuous supply of well-worn chalk pebbles might indeed be kept up from a gradually rising or sinking beach, but it is difficult to see how, until the land had subsided for at least 200 feet, the chalky boulder clay could be deposited in some of the East Anglian valleys or on the Leicestershire hills. That depression, however, would seriously diminish the area of exposed chalk in Lincolnshire and Yorkshire, and the double of it would almost drown that rock. Again, the East Anglian boulder clay, as we have said, frequently abounds in fragments and finer detritus from the Kimmeridge and Oxford clays. But a large part of their outcrop would disappear before the former submergence was completed. Yet the materials of the boulder clay, though changing as it is traced across the country, more especially from east to west, seem to vary little in a

¹ *Quart. Jour. Geol. Soc.*, liv. (1898), p. 205. Earlier observations of some upthrust of materials by a glacier are noted on p. 219. —

vertical direction. The instances, also, of the transportation of boulders and smaller stones to higher levels, sometimes large in amount, as in the transference of 'brockram' from outcrops near the bed of the Eden valley to the level of Stainmoor Gap, seem to be too numerous to be readily explained by the uplifting action of shore-ice in a subsiding area. Such a process is possible, but we should anticipate it would be rather exceptional.

Submergence also readily accounts for the above-named sands and gravels, but not quite so easily for their occurrence at such very different levels. On the eastern side of England gravelly sands may be found beneath the chalky boulder clay from well below sea-level to three or four hundred feet above it. Again, since, on the submergence hypothesis, the lower boulder clay about the estuaries of the Dee and the Mersey must represent a deposit from piedmont ice in a shallow sea, the mid-glacial sand (sometimes not very clearly marked in this part) ought not to be more than forty or fifty feet above the present Ordnance datum. But at Manchester it reaches over 200 feet, while near Heywood it is at least 425 feet. In other words the sands and gravels, presumably (often certainly) mid-glacial, mantle, like the upper boulder clay, over great irregularities of the surface, and are sometimes found, as already stated, up to more than 1,200 feet. Either of these deposits may have followed the sea-line upwards or downwards, but that explanation would almost compel us to suppose that the sand was deposited during the submergence and the upper clay during the emergence; so that, with the former material, the higher in position is the newer in time, and with the latter the reverse. We must not, however, forget that in the island of Rügen we find more than one example of a stratified gravelly sand between two beds of boulder clay (containing Scandinavian erratics) which present some resemblance to the boulder clays of Eastern England, while certain glacial deposits at Warnemünde, on the Baltic coast, sometimes remind us of the Contorted Drift of Norfolk.

Towards the close of the Glacial Epoch, the deposition of the boulder clay ceased¹ and its denudation began. On the low plateaux of the Eastern Counties it is often succeeded by coarse gravels, largely composed of flint, more or less water-worn. These occasionally include small intercalations of boulder clay, have evidently been derived from it, and indicate movement by fairly strong currents. Similar gravels are found overlying the boulder clay in other parts of England, sometimes at greater heights above sea-level. Occasionally the two are intimately related. For instance, a pit on the broad, almost level, top of the Gogmagog Hills, about 200 feet above sea-level and four miles south of Cambridge, shows a current-bedded sand and gravel, overlain by a

¹ Probably deposits of a distinctly glacial origin (such as those near Hesse in Yorkshire) continued in the northern districts, but on these we need not linger.

boulder clay, obviously rearranged; while other pits in the immediate neighbourhood expose varieties and mixtures of one or the other material. But, as true boulder clay occurs in the valley below, these gravels must have been deposited, and that by rather strong currents, on a hill-top—a thing which seems impossible under anything like the existing conditions; and, even if the lowland were buried beneath ice full 200 feet in thickness, which made the hill-top into the bed of a lake, it is difficult to understand how the waters of that could be in rapid motion. Rearranged boulder clays also occur on the slopes of valleys¹ which may be explained, with perhaps some of the curious sections near Sudbury, by the slipping of materials from a higher position. But at Old Oswestry gravels with indications of ice action are found at the foot of the hills almost 700 feet below those of Gloopá.

Often the plateau gravels are followed at a lower level by terrace gravels,² which descend towards the existing rivers, and suggest that valleys have been sometimes deepened, sometimes only re-excavated. The latter gravels are obviously deposited by rivers larger and stronger than those which now wind their way seawards, but it is difficult to explain the former gravels by any fluvial action, whether the water from a melting ice-sheet ran over the land or into a lake, held up by some temporary barrier. But the sorting action of currents in a slowly shallow-ing sea would be quite competent to account for them, so they afford an indirect support to the hypothesis of submergence. It is, however, generally admitted that there have been oscillations both of level and of climate since any boulder clay was deposited in the districts south of the Humber and the Ribble. The passing of the Great Ice Age was not sudden, and glaciers may have lingered in our mountain regions when palæolithic man hunted the mammoth in the valley of the Thames, or frequented the caves of Devon and Mendip. But of these times of transition before written history became possible, and of sundry interesting topics connected with the Ice Age itself—of its cause, date, and duration, whether it was persistent or interrupted by warmer episodes, and, if so, by what number, of how often it had already recurred in the history of the earth—I must, for obvious reasons, refrain from speaking, and content myself with having endeavoured to place before you the facts of which, in my opinion, we must take account in reconstructing the physical geography of Western Europe, and especially of our own country, during the Age of Ice.

Not unnaturally you will expect a decision in favour of one or the other litigant after this long summing up. But I can only say that, in regard to the British Isles, the difficulties in either hypothesis appear so

¹ For instance, at Stanningfield in the valley of the Lark.

² These contain the instruments worked by palæolithic (Acheulean) man who, in this country at any rate, is later than the chalky boulder clay.

great that, while I consider those in the 'land-ice' hypothesis to be the more serious, I cannot as yet declare the other one to be satisfactorily established, and think we shall be wiser in working on in the hope of clearing up some of the perplexities. I may add that, for these purposes, regions like the northern coasts of Russia and Siberia appear to me more promising than those in closer proximity to the North or South Magnetic Poles. This may seem a 'lame and impotent conclusion' to so long a disquisition, but there are stages in the development of a scientific idea when the best service we can do it is by attempting to separate facts from fancies, by demanding that difficulties should be frankly faced instead of being severely ignored, by insisting that the giving of a name cannot convert the imaginary into the real, and by remembering that if hypotheses yet on their trial are treated as axioms, the result will often bring disaster, like building a tower on a foundation of sand. To scrutinise, rather than to advocate any hypothesis, has been my aim throughout this address, and, if my efforts have been to some extent successful, I trust to be forgiven, though I may have trespassed on your patience and disappointed a legitimate expectation.

British Association for the Advancement of Science.

SHEFFIELD, 1910.

ADDRESS

TO THE

MATHEMATICAL AND PHYSICAL SECTION

BY

PROFESSOR E. W. HOBSON, Sc.D., F.R.S.

PRESIDENT OF THE SECTION.

SINCE the last meeting of our Association one of the most illustrious of the British workers in science during the nineteenth century has been removed from us by the death of Sir William Huggins. In the middle of the last century Sir William Huggins commenced that pioneer work of examination of the spectra of the stars which has ensured for him enduring fame in connection with the foundation of the science of Astrophysics. The exigencies of his work of analysis of the stellar spectra led him to undertake a minute examination of the spectra of the elements with a view to the determination of as many lines as possible. To the spectroscope he later added the photographic film as an instrument of research in his studies of the heavenly bodies. In 1864 Sir William Huggins made the important observation that many of the nebulae have spectra which consist of bright lines; and two years later he observed, in the case of a new star, both bright and dark lines in the same spectrum. In 1868 his penetrating and alert mind made him the first to perceive that the Doppler principle could be applied to the determination of the velocities of stars in the line of sight, and he at once set about the application of the method. His life-work, in a domain of absorbing interest, was rewarded by a rich harvest of discovery, obtained as the result of most patient and minute investigations. The 'Atlas of Representative Stellar Spectra,' published in the names of himself and Lady Huggins, remains as a monumental record of their joint labours.

The names of the great departments of science, Mathematics, Physics, Astronomy, Meteorology, which are associated with Section A, are a sufficient indication of the vast range of investigation which comes under the purview of our Section. An opinion has been strongly expressed in some quarters that the time has come for the erection of a separate Section for Astronomy and Meteorology, in order that fuller opportunities may be afforded than hitherto for the discussion of matters of special interest to those devoted to these departments of Science. I do not share this view. I believe that, whilst the customary division into sub-sections gives reasonable facilities for the treatment of questions interesting solely to specialists in the various branches with which our Section is concerned, a policy of disruption would be injurious to the wider interests of science. The close association of the older Astronomy with Mathematics, and of the newer Astronomy with Physics, form strong presumptions against the change that has been suggested. Meteorology, so far as it goes beyond the purely empirical region, is, and must always remain, a branch of Physics. No doubt, the more technical problems which arise in connection with these subjects, though of

great importance to specialists, are often of little or no interest to workers in cognate departments. It appears to me, however, that it is unwise, in view of the general objects of the British Association, to give too much prominence in the meetings to the more technical aspects of the various departments of science. Ample opportunities for the full discussion of all the detailed problems, the solution of which forms a great and necessary part of the work of those who are advancing science in its various branches, are afforded by the special Societies which make those branches their exclusive concern. The British Association will, in my view, be performing its functions most efficiently if it gives much prominence to those aspects of each branch of science which are of interest to a public at least in some degree larger than the circle of specialists concerned with the particular branch. To afford an opportunity to workers in any one department of obtaining some knowledge of what is going on in other departments, to stimulate by means of personal intercourse with workers on other lines the sense of solidarity of men of science, to do something to counteract that tendency to narrowness of view which is a danger arising from increasing specialisation, are functions, the due performance of which may do much to further that supreme object, the advancement of science, for which the British Association exists.

I propose to address to you a few remarks, necessarily fragmentary and incomplete, upon the scope and tendencies of modern Mathematics. Not to transgress against the canon I have laid down, I shall endeavour to make my treatment of the subject as little technical as possible.

Probably no other department of knowledge plays a larger part outside its own narrower domain than Mathematics. Some of its more elementary conceptions and methods have become part of the common heritage of our civilisation, interwoven in the every-day life of the people. Perhaps the greatest labour-saving invention that the world has seen belongs to the formal side of Mathematics; I allude to our system of numerical notation. This system which, when scrutinised, affords the simplest illustration of the importance of Mathematical form, has become so much an indispensable part of our mental furniture that some effort is required to realise that an apparently so obvious idea embodies a great invention; one to which the Greeks, with their unsurpassed capacity for abstract thinking, never attained. An attempt to do a multiplication sum in Roman numerals is perhaps the readiest road to an appreciation of the advantages of this great invention. In a large group of sciences, the formal element, the common language, so to speak, is supplied by Mathematics; the range of the application of mathematical methods and symbolism is ever increasing. Without taking too literally the celebrated dictum of the great philosopher Kant, that the amount of real science to be found in any special subject is the amount of Mathematics contained therein, it must be admitted that each branch of science which is concerned with natural phenomena, when it has reached a certain stage of development, becomes accessible to, and has need of, mathematical methods and language; this stage has, for example, been reached in our time by parts of the science of Chemistry. Even Biology and Economics have begun to require mathematical methods, at least on their statistical side. As a science emerges from the stages in which it consists solely of more or less systematised descriptions of the phenomena with which it is concerned in their more superficial aspect; when the intensive magnitudes discerned in the phenomena become representable as extensive magnitudes, then is the beginning of the application of mathematical modes of thought; at a still later stage, when the phenomena become accessible to dynamical treatment, Mathematics is applicable to the subject to a still greater extent.

Mathematics shares with the closely allied subject of Astronomy the honour of being the oldest of the sciences. When we consider that it embodies, in an abstract form, some of the more obvious, and yet fundamental, aspects of our experience of the external world, this is not altogether surprising. The comparatively high degree of development which, as recent historical discoveries have disclosed, it had attained amongst the Babylonians more than five thousand years B.C., may well astonish us. These times must have been preceded by still earlier ages in which the mental evolution of man led him to the use of the tally, and of simple modes of measurement, long before the notions of number and of magnitude appeared in an explicit form.

I have said that Mathematics is the oldest of the sciences; a glance at its more recent history will show that it has the energy of perpetual youth. The output of contributions to the advance of the science during the last century and more has been so enormous that it is difficult to say whether pride in the greatness of achievement in his subject, or despair at his inability to cope with the multiplicity of its detailed developments, should be the dominant feeling of the mathematician. Few people outside the small circle of mathematical specialists have any idea of the vast growth of mathematical literature. The Royal Society Catalogue contains a list of nearly thirty-nine thousand papers on subjects of Pure Mathematics alone, which have appeared in seven hundred serials during the nineteenth century. This represents only a portion of the total output; the very large number of treatises, dissertations, and monographs published during the century being omitted. During the first decade of the twentieth century this activity has proceeded at an accelerated rate. Mathematical contributions to Mechanics, Physics, and Astronomy would greatly swell the total. A notion of the range of the literature relating not only to Pure Mathematics but also to all branches of science to which mathematical methods have been applied will be best obtained by an examination of that monumental work, the 'Encyclopädie der mathematischen Wissenschaften'—when it is completed.

The concepts of the pure mathematician, no less than those of physicist, had their origin in physical experience analysed and clarified by the reflective activities of the human mind; but the two sets of concepts stand on different planes in regard to the degree of abstraction which is necessary in their formation. Those of the mathematician are more remote from actual unanalysed precepts than are those of the physicist, having undergone in their formation a more complete idealisation and removal of elements inessential in regard to the purposes for which they are constructed. This difference in the planes of thought frequently gives rise to a certain misunderstanding between the mathematician and the physicist, due in the case of either to an inadequate appreciation of the point of view of the other. On the one hand it is frequently and truly said of particular mathematicians that they are lacking in the physical instinct; and on the other hand a certain lack of sympathy is frequently manifested on the part of physicists for the aims and ideals of the mathematician. The habits of mind and the ideals of the mathematician and of the physicist cannot be of an identical character. The concepts of the mathematician necessarily lack, in their pure form, just that element of concreteness which is an essential condition of the success of the physicist, but which to the mathematician would often only obscure those aspects of things which it is his province to study. The abstract mathematical standard of exactitude is one of which the physicist can make no direct use. The calculations in Mathematics are directed towards ideal precision, those in Physics consist of approximations within assigned limits of error. The physicist can, for example, make no direct use of such an object as an irrational number; in any given case a properly chosen rational number approximating to the irrational one is sufficient for his purpose. Such a notion as continuity, as it occurs in Mathematics, is, in its purity, unknown to the physicist, who can make use only of sensible continuity. The physical counterpart of mathematical discontinuity is very rapid change through a thin layer of transition, or during a very short time. Much of the skill of the true mathematical physicist and of the mathematical astronomer consists in the power of adapting methods and results carried out on an exact mathematical basis to obtain approximations sufficient for the purposes of physical measurement. It might perhaps be thought that a scheme of Mathematics on a frankly approximative basis would be sufficient for all the practical purposes of application in Physics, Engineering Science, and Astronomy; and no doubt it would be possible to develop, to some extent at least, a species of Mathematics on these lines. Such a system would, however, involve an intolerable awkwardness and prolixity in the statement of results, especially in view of the fact that the degrees of approximation necessary for various purposes are very different, and thus that unassigned grades of approximation would have to be provided for. Moreover the mathematician working on these lines would be cut off from his chief sources of inspiration, the ideals of exactitude and logical rigour, as well as from one of his most indispensable guides to discovery, symmetry and permanence of mathematical form. The

history of the actual movements of mathematical thought through the centuries shows that these ideals are the very life-blood of the science, and warrants the conclusion that a constant striving towards their attainment is an absolutely essential condition of vigorous growth. These ideals have their roots in irresistible impulses and deep-seated needs of the human mind, manifested in its efforts to introduce intelligibility into certain great domains of the world of thought.

There exists a widespread impression amongst physicists, engineers, and other men of science that the effect of recent developments of Pure Mathematics, by making it more abstract than formerly, has been to remove it further from the order of ideas of those who are primarily concerned with the physical world. The prejudice that Pure Mathematics has its sole *raison d'être* in its function of providing useful tools for application in the physical sciences, a prejudice which did much to retard the due development of Pure Mathematics in this country during the nineteenth century, is by no means extinct. It is not infrequently said that the present devotion of many mathematicians to the interminable discussion of purely abstract questions relating to modern developments of the notions of number and function, and to theories of algebraic form, serves only the purpose of deflecting them from their proper work into paths which lead nowhere. It is considered that mathematicians are apt to occupy themselves too exclusively with ideas too remote from the physical order in which Mathematics had its origin and in which it should still find its proper applications. A direct answer to the question *cui bono?* when it is raised in respect of a department of study such as Pure Mathematics, seldom carries conviction, in default of a standard of values common to those who ask and to those who answer the question. To appreciate the importance of a sphere of mental activity different from our own always requires some effort of the sympathetic imagination, some recognition of the fact that the absolute value of interests and ideals of a particular class may be much greater than the value which our own mentality inclines us to attach to them. If a defence is needed of the expenditure of time and energy on the abstract problems of Pure Mathematics, that defence must be of a cumulative character. The fact that abstract mathematical thinking is one of the normal forms of activity of the human mind, a fact which the general history of thought fully establishes, will appeal to some minds as a ground of decisive weight. A great department of thought must have its own inner life, however transcendent may be the importance of its relations to the outside. No department of science, least of all one requiring so high a degree of mental concentration as Mathematics, can be developed entirely, or even mainly, with a view to applications outside its own range. The increased complexity and specialisation of all branches of knowledge makes it true in the present, however it may have been in former times, that important advances in such a department as Mathematics can be expected only from men who are interested in the subject for its own sake, and who, whilst keeping an open mind for suggestions from outside, allow their thought to range freely in those lines of advance which are indicated by the present state of their subject, untrammelled by any preoccupation as to applications to other departments of science. Even with a view to applications, if Mathematics is to be adequately equipped for the purpose of coping with the intricate problems which will be presented to it in the future by Physics, Chemistry, and other branches of physical science, many of these problems probably of a character which we cannot at present forecast, it is essential that Mathematics should be allowed to develop itself freely on its own lines. Even if much of our present mathematical theorising turns out to be useless for external purposes, it is wiser, for a well-known reason, to allow the wheat and the tares to grow together. It would be easy to establish in detail that many of the applications which have been actually made of Mathematics were wholly unforeseen by those who first developed the methods and ideas on which they rest. Recently, the more refined mathematical methods which have been applied to gravitational Astronomy by Delaunay, G. W. Hill, Poincaré, E. W. Brown, and others, have thrown much light on questions relating to the solar system, and have much increased the accuracy of our knowledge of the motions of the moon and the planets. Who knows what weapons forged by the theories of functions, of differential equations, or of groups, may be required when the time comes for such an empirical law as Mendeléeff's periodic law of

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the elements to receive its dynamical explanation by means of an analysis of the detailed possibilities of relatively stable types of motion, the general schematic character of which will have been indicated by the physicist? It is undoubtedly true that the cleft between Pure Mathematics and Physical Science is at the present time wider than formerly. That is, however, a result of the natural development, on their own lines, of both subjects. In the classical period of the eighteenth century, the time of Lagrange and Laplace, the nature of the physical investigations, consisting largely of the detailed working out of problems of gravitational Astronomy in accordance with Newton's law, was such that the passage was easy from the concrete problems to the corresponding abstract mathematical ones. Later on, mathematical physicists were much occupied with problems which lent themselves readily to treatment by means of continuous analysis. In our own time the effect of recent developments of Physics has been to present problems of molecular and sub-molecular Mechanics to which continuous analysis is not at least directly applicable, and can only be made applicable by a process of averaging the effects of great swarms of discrete entities. The speculative and incomplete character of our conceptions of the structure of the objects of investigation has made the applications of Dynamics to their detailed elucidation tentative and partial. The generalised dynamical scheme developed by Lagrange and Hamilton, with its power of dealing with systems, the detailed structure of which is partially unknown, has however proved a powerful weapon of attack, and affords a striking instance of the deep-rooted significance of mathematical form. The wonderful and perhaps unprecedentedly rapid discoveries in Physics which have been made in the last two decades have given rise to many questions which are as yet hardly sufficiently definite in form to be ripe for mathematical treatment; a necessary condition of which treatment consists in a certain kind of precision in the data of the problems to be solved.

The difficulty of obtaining an adequate notion of the general scope and aims of Mathematics, or even of special branches of it, is perhaps greater than in the case of any other science. Many persons, even such as have made a serious and prolonged study of the subject, feel the difficulty of seeing the wood for trees. The severe demands made upon students by the labour of acquiring a difficult technique largely accounts for this; but teachers might do much to facilitate the attainment of a wider outlook by directing the attention of their students to the more general and less technical aspects of the various parts of the subject, and especially by the introduction into the courses of instruction of more of the historical element than has hitherto been usual.

All attempts to characterise the domain of Mathematics by means of a formal definition which shall not only be complete, but which shall also rigidly mark off that domain from the adjacent provinces of Formal Logic on the one side and of Physical Science on the other side, are almost certain to meet with but doubtful success; such success as they may attain will probably be only transient, in view of the power which the science has always shown of constantly extending its borders in unforeseen directions. Such definitions, many of which have been advanced, are apt to err by excess or defect, and often contain distinct traces of the personal predilections of those who formulate them. There was a time when it would have been a tolerably sufficient description of Pure Mathematics to say that its subject matter consisted of magnitude and geometrical form. Such a description of it would be wholly inadequate at the present day. Some of the most important branches of modern Mathematics, such as the theory of groups, and Universal Algebra, are concerned, in their abstract forms, neither with magnitude nor with number, nor with geometrical form. That great modern development, Projective Geometry, has been so formulated as to be independent of all metric considerations. Indeed the tendency of mathematicians under the influence of the movement known as the Arithmetisation of Analysis, a movement which has become a dominant one in the last few decades, is to banish altogether the notion of measurable quantity as a conception necessary to Pure Mathematics; Number, in the extended meaning it has attained, taking its place. Measurement is regarded as one of the applications, but as no part of the basis, of mathematical analysis. Perhaps the least inadequate description of the general scope of modern Pure Mathematics—I will not call it a definition—would be to say that it deals with *form*, in a very general sense of

the term; this would include algebraic form, geometrical form, functional relationship, the relations of order in any ordered set of entities such as numbers, and the analysis of the peculiarities of form of groups of operations. A strong tendency is manifested in many of the recent definitions to break down the line of demarcation which was formerly supposed to separate Mathematics from formal logic; the rise and development of symbolic logic has no doubt emphasised this tendency. Thus Mathematics has been described by the eminent American mathematician and logician B. Pierce as 'the Science which draws necessary conclusions,' a pretty complete identification of Mathematics with logical procedure in general. A definition which appears to identify all Mathematics with the Mengenlehre, or Theory of Aggregates, has been given by E. Papperitz: 'The subject-matter of Pure Mathematics consists of the relations that can be established between any objects of thought when we regard those objects as contained in an ordered manifold; the law of order of this manifold must be subject to our choice.' The form of definition which illustrates most strikingly the tendencies of the modern school of logic is one given by Mr. Bertrand Russell. I reproduce it here, in order to show how wide is the chasm between the modes of expression of adherents of this school and those of mathematicians under the influence of the ordinary traditions of the science. Mr. Russell writes:¹ 'Pure Mathematics is the class of all propositions of the form " p implies q ," where p and q are propositions containing one or more variables, the same in the two propositions, and neither p nor q contains any constants except logical constants. And logical constants are all notions definable in terms of the following: Implication, the relation of a term to a class of which it is a member, the notion of *such that*, the notion of relation, and such further notions as may be involved in the general notion of propositions of the above form. In addition to these, Mathematics uses a notion which is not a constituent of the propositions which it considers—namely, the notion of truth.'

The belief is very general amongst instructed persons that the truths of Mathematics have absolute certainty, or at least that there appertains to them the highest degree of certainty of which the human mind is capable. It is thought that a valid mathematical theorem is necessarily of such a character as to compel belief in any mind capable of following the steps of the demonstration. Any considerations tending to weaken this belief would be disconcerting and would cause some degree of astonishment. At the risk of this, I must here mention two facts which are of considerable importance as regards an estimation of the precise character of mathematical knowledge. In the first place, it is a fact that frequently, and at various times, differences of opinion have existed among mathematicians, giving rise to controversies as to the validity of whole lines of reasoning, and affecting the results of such reasoning; a considerable amount of difference of opinion of this character exists among mathematicians at the present time. In the second place, the accepted standard of rigour, that is, the standard of what is deemed necessary to constitute a valid demonstration, has undergone change in the course of time. Much of the reasoning which was formerly regarded as satisfactory and irrefutable is now regarded as insufficient to establish the results which it was employed to demonstrate. It has even been shown that results which were once supposed to have been fully established by demonstrations are, in point of fact, affected with error. I propose here to explain in general terms how these phenomena are possible.

In every subject of study, if one probes deep enough, there are found to be points in which that subject comes in contact with general philosophy, and where differences of philosophical view will have a greater or less influence on the attitude of the mind towards the principles of the particular subject. This is not surprising when we reflect that there is but one universe of thought, that no department of knowledge can be absolutely isolated, and that metaphysical and psychological implications are a necessary element in all the activities of the mind. A particular department, such as Mathematics, is compelled to set up a more or less artificial frontier, which marks it off from general philosophy. This frontier consists of a set of regulative ideas in the form of indefinables and axioms, partly ontological assumptions, and partly postulations of a logical

¹ *Principles of Mathematics*, p. 1.

character. To go behind these, to attempt to analyse their nature and origin, and to justify their validity, is to go outside the special department and to touch on the domains of the metaphysician and the psychologist. Whether they are regarded as possessing apodictic certainty or as purely hypothetical in character, these ideas represent the data or premises of the science, and the whole of its edifice is dependent upon them. They serve as the foundation on which all is built, as well as the frontier on the side of philosophy and psychology. A set of data ideally perfect in respect of precision and permanence is unattainable—or at least has not yet been attained; and the adjustment of frontiers is one of the most frequent causes of strife. As a matter of fact, variations of opinion have at various times arisen within the ranks of the mathematicians as to the nature, scope, and proper formulation of the principles which form the foundations of the science, and the views of mathematicians in this regard have always necessarily been largely affected by the conscious or unconscious attitude of particular minds towards questions of general philosophy. It is in this region, I think, that the source is to be found of those remarkable differences of opinion amongst mathematicians which have come into prominence at various times, and have given rise to much controversy as to fundamentals. Since the time of Newton and Leibnitz there has been almost unceasing discussion as to the proper foundations for the so-called infinitesimal calculus. More recently, questions relating to the foundations of geometry and rational mechanics have much occupied the attention of mathematicians. The very great change which has taken place during the last half century in the dominant view of the foundations of mathematical analysis—a change which has exercised a great influence extending through the whole detailed treatment of that subject—although critical in its origin, has been constructive in its results. The *Mengenlehre*, or theory of aggregates, had its origin in the critical study of the foundations of analysis, but has already become a great constructive scheme, is indispensable as a method in the investigations of analysis, provides the language requisite for the statement in precise form of analytical theorems of a general character, and, moreover, has already found important applications in geometry. In connection with the *Mengenlehre* there has arisen a controversy amongst mathematicians which is at the present time far from having reached a decisive issue. The exact point at issue is one which may be described as a matter of mathematical ontology; it turns upon the question of what constitutes a valid definition of a mathematical object. The school known as mathematical ‘idealists’ admit, as valid objects of mathematical discussion, entities which the rival ‘empiricist’ school regard as non-existent for mathematical thought, because insufficiently defined. It is clear that the idealist may build whole superstructures on a foundation which the empiricist regards as made of sand, and this is what has actually happened in some of the recent developments of what has come to be known as Cantorism. The difference of view of these rival schools, depending as it does on deep-seated differences of philosophical outlook, is thought by some to be essentially irreconcilable. This controversy was due to the fact that certain processes of reasoning, of very considerable plausibility, which had been employed by G. Cantor, the founder of the *Mengenlehre*, had led to results which contained flat contradictions. The efforts made to remove these contradictions, and to trace their source, led to the discussion, disclosing much difference of opinion, of the proper definitions and principles on which the subject should be based.

The proposition $7+5=12$, taken as typical of the propositions expressing the results of the elementary operations of arithmetic, has since the time of Kant given rise to very voluminous discussion amongst philosophers, in relation to the precise meaning and implication of the operation and the terms. It will, however, be maintained, probably by the majority of mankind, that the theorem retains its validity as stating a practically certain and useful fact, whatever view philosophers may choose to take of its precise nature—as, for example, whether it represents, in the language of Kant, a synthetic or an analytic judgment. It may, I think, be admitted that there is much cogency in this view; and, were Mathematics concerned with the elementary operations of arithmetic alone, it could fairly be held that the mathematician, like the practical man of the world, might without much risk shut his eyes and ears to the discussions of the philosophers on such points. The exactitude of such a proposition, in a suffi-

ciently definite sense for practical purposes, is empirically verifiable by sensuous intuition, whatever meaning the metaphysician may attach to it. But Mathematics cannot be built up from the operations of elementary arithmetic without the introduction of further conceptual elements. Except in certain very simple cases no process of measurement, such as the determination of an area or a volume, can be carried out with exactitude by a finite number of applications of the operations of arithmetic. The result to be obtained appears in the form of a limit, corresponding to an interminable sequence of arithmetical operations. The notion of 'limit,' in the definite form given to it by Cauchy and his followers, together with the closely related theory of the arithmetic continuum, and the notions of continuity and functionality, lie at the very heart of modern analysis. Essentially bound up with this central doctrine of limits is the concept of a non-finite set of entities, a concept which is not directly derivable from sensuous intuition, but which is nevertheless a necessary postulation in mathematical analysis. The conception of the infinite, in some form, is thus indispensable in Mathematics; and this conception requires precise characterisation by a scheme of exact definitions, prior to all the processes of deduction required in obtaining the detailed results of analysis. The formulation of this precise scheme gives an opening to differences of philosophical opinion which has led to a variety of views as to the proper character of those definitions which involve the concept of the infinite. Here is the point of divergence of opinion among mathematicians to which I have alluded above. Under what conditions is a non-finite aggregate of entities a properly defined object of mathematical thought, of such a character that no contradictions will arise in the theories based upon it? That is the question to which varying answers have been offered by different mathematical thinkers. No one answer of a completely general character has as yet met with universal acceptance. Physical intuition offers no answer to such a question; it is one which abstract thought alone can settle. It cannot be altogether avoided, because, without the notion of the infinite, at least in connection with the central conception of the 'limit,' mathematical analysis as a coherent body of thought falls to the ground.

Both in geometry and in analysis our standard of what constitutes a rigorous demonstration has in the course of the nineteenth century undergone an almost revolutionary change. That oldest text-book of science in the world, 'Euclid's Elements of Geometry,' has been popularly held for centuries to be the very model of deductive logical demonstration. Criticism has, however, largely invalidated this view. It appears that, at a large number of points, assumptions not included in the preliminary axioms and postulates are made use of. The fact that these assumptions usually escape notice is due to their nature and origin. Derived as they are from our spatial intuition, their very self-evidence has allowed them to be ignored, although their truth is not more obvious empirically than that of other assumptions derived from the same source which are included in the axioms and postulates explicitly stated as part of the foundation of Euclid's treatment of the subject. The method of superimposition, employed by Euclid with obvious reluctance, but forming an essential part of his treatment of geometry, is, when regarded from his point of view, open to most serious objections as regards its logical coherence. In analysis, as in geometry, the older methods of treatment consisted of processes of deduction eked out by the more or less surreptitious introduction, at numerous points in the subject, of assumptions only justifiable by spatial intuition. The result of this deviation from the purely deductive method was more disastrous in the case of analysis than in geometry, because it led to much actual error in the theory. For example, it was held until comparatively recently that a continuous function necessarily possesses a differential coefficient, on the ground that a curve always has a tangent. This we now know to be quite erroneous, when any reasonable definition of continuity is employed. The first step in the discovery of this error was made when it occurred to Ampère that the existence of the differential coefficient could only be asserted as a theorem requiring proof; and he himself published an attempt at such proof. The erroneous character of the former belief on this matter was most strikingly exhibited when Weierstrass produced a function which is everywhere continuous, but which nowhere possesses a differential coefficient; such functions can now be constructed *ad libitum*. It is not too much to say that no one of the general theorems of analysis is true without the introduction of limitations and conditions which were

entirely unknown to the discoverers of those theorems. It has been the task of mathematicians under the lead of such men as Cauchy, Riemann, Weierstrass, and G. Cantor, to carry out the work of reconstruction of mathematical analysis, to render explicit all the limitations of the truth of the general theorems, and to lay down the conditions of validity of the ordinary analytical operations. Physicists and others often maintain that this modern extreme precision amounts to an unnecessary and pedantic purism, because in all practical applications of Mathematics only such functions are of importance as exclude the remoter possibilities contemplated by theorists. Such objections leave the true mathematician unmoved; to him it is an intolerable defect that, in an order of ideas in which absolute exactitude is the guiding ideal, statements should be made, and processes employed, both of which are subject to unexpressed qualifications, as conditions of their truth or validity. The pure mathematician has developed a specialised conscience, extremely sensitive as regards sins against logical precision. The physicist, with his conscience hardened in this respect by the rough-and-tumble work of investigating the physical world, is apt to regard the more tender organ of the mathematician with that feeling of impatience, not unmingled with contempt, which the man of the world manifests for what he considers to be over-scrupulosity and impracticality.

It is true that we cannot conceive how such a science as Mathematics could have come into existence apart from physical experience. But it is also true that physical precepts, as given directly in unanalysed experience, are wholly unfitted to form the basis of an exact science. Moreover, physical intuition fails altogether to afford any trustworthy guidance in connection with the concept of the infinite, which, as we have seen, is in some form indispensable in the formation of a coherent system of mathematical analysis. The hasty and uncritical extension to the region of the infinite, of results which are true and often obvious in the region of the finite, has been a fruitful source of error in the past, and remains as a pitfall for the unwary student in the present. The notions derived from physical intuition must be transformed into a scheme of exact definitions and axioms before they are available for the mathematician, the necessary precision being contributed by the mind itself. A very remarkable fact in connection with this process of refinement of the rough data of experience is that it contains an element of arbitrariness, so that the result of the process is not necessarily unique. The most striking example of this want of uniqueness in the conceptual scheme so obtained is the case of geometry, in which it has been shown to be possible to set up various sets of axioms, each set self-consistent, but inconsistent with any other of the sets, and yet such that each set of axioms, at least under suitable limitations, leads to results consistent with our perception of actual space-relations. Allusion is here made, in particular, to the well-known geometries of Lobatchewsky and of Riemann, which differ from the geometry of Euclid in respect of the axiom of parallels, in place of which axioms inconsistent with that of Euclid and with one another are substituted. It is a matter of demonstration that any inconsistency which might be supposed to exist in the scheme known as hyperbolic geometry, or in that known as elliptic geometry, would necessarily entail the existence of a corresponding inconsistency in Euclid's set of axioms. The three geometries therefore, from the logical point of view, are completely on a par with one another. An interesting mathematical result is that all efforts to prove Euclid's axiom of parallels, i.e., to deduce it from his other axioms, are doomed to necessary failure; this is of importance in view of the many efforts that have been made to obtain the proof referred to. When the question is raised which of these geometries is the true one, the kind of answer that will be given depends a good deal on the view taken of the relation of conceptual schemes in general to actual experience. It is maintained by M. Poincaré, for example, that the question which is the true scheme has no meaning; that it is, in fact, entirely a matter of convention and convenience which of these geometries is actually employed in connection with spatial measurements. To decide between them by a crucial test is impossible, because our space perceptions are not sufficiently exact in the mathematical sense to enable us to decide between the various axioms of parallels. Whatever views are taken as to the difficult questions that arise in this connection, the contemplation and study of schemes of geometry wider than that of Euclid, and some of them including Euclid's geometry as a special

case, is of great interest not only from the purely mathematical point of view, but also in relation to the general theory of knowledge, in that, owing to the results of this study, some change is necessitated in the views which have been held by philosophers as to what is known as Kant's space-problem.

The school of thought which has most emphasised the purely logical aspect of Mathematics is that which is represented in this country by Mr. Bertrand Russell and Dr. Whitehead, and which has distinguished adherents both in Europe and in America. The ideal of this school is a presentation of the whole of Mathematics as a deductive scheme in which are employed a certain limited number of indefinables and unprovable axioms, by means of a procedure in which all possibility of the illicit intrusion of extraneous elements into the deduction is excluded by the employment of a symbolism in which each symbol expresses a certain logical relation. This school receives its inspiration from a peculiar form of philosophic realism which, in its revolt from idealism, produces in the adherents of the school a strong tendency to ignore altogether the psychological implications in the movements of mathematical thought. This is carried so far that in their writings no explicit recognition is made of any psychological factors in the selection of the indefinables and in the formulation of the axioms upon which the whole structure of Mathematics is to be based. The actually worked-out part of their scheme has as yet reached only the mere fringe of modern Mathematics as a great detailed body of doctrine; but to any objection to the method on the ground of the prolixity of the treatment which would be necessary to carry it out far enough to enable it to embrace the various branches of Mathematics in all the wealth of their present development, it would probably be replied that the main point of interest is to establish in principle the possibility only of subsuming Pure Mathematics under a scheme of logic. It is quite impossible for me here to attempt to discuss, even in outline, the tenets of this school, or even to deal with the interesting question of the possibility of setting up a final system of indefinables and axioms which shall suffice for all present and future developments of Mathematics.

I am very far from wishing to minimise the high philosophic interest of the attempt made by the Peano-Russell school to exhibit Mathematics as a scheme of deductive logic. I have myself emphasised above the necessity and importance of fitting the results of mathematical research in their final form into a framework of deduction, for the purpose of ensuring the complete precision and the verification of the various mathematical theories. At the same time it must be recognised that the purely deductive method is wholly inadequate as an instrument of research. Whatever view may be held as regards the place of psychological implications in a completed body of mathematical doctrine, in research the psychological factor is of paramount importance. The slightest acquaintance with the history of Mathematics establishes the fact that discoveries have seldom, or never, been made by purely deductive processes. The results are thrown into a purely deductive form after, and often long after, their discovery. In many cases the purely deductive form, in the full sense, is quite modern. The possession of a body of indefinables, axioms, or postulates, and symbols denoting logical relation, would, taken by itself, be wholly insufficient for the development of a mathematical theory. With these alone the mathematician would be unable to move a step. In face of an unlimited number of possible combinations a principle of selection of such as are of interest, a purposive element, and a perceptive faculty are essential for the development of anything new. In the process of discovery the chains in a sequence of logical deduction do not at first arise in their final order in the mind of the mathematical discoverer. He divines the results before they are established; he has an intuitive grasp of the general line of a demonstration long before he has filled in the details. A developed theory, or even a demonstration of a single theorem, is no more identical with a mere complex of syllogisms than a melody is identical with the mere sum of the musical notes employed in its composition. In each case the whole is something more than merely the sum of its parts; it has a unity of its own, and that unity must be, in some measure at least, discerned by its creator before the parts fall completely into their places. Logic is, so to speak, the grammar of Mathematics; but a knowledge of the rules of grammar and the letters of the alphabet would not be sufficient equipment to enable a man to write a book. There is much room for

individuality in the modes of mathematical discovery. Some great mathematicians have employed largely images derived from spatial intuition as a guide to their results; others appear wholly to have discarded such aids, and were led by a fine feeling for algebraic and other species of mathematical form. A certain tentative process is common, in which, by the aid of results known or obtained in special cases, generalisations are perceived and afterwards established, which take up into themselves all the special cases so employed. Most mathematicians leave some traces, in the final presentation of their work, of the scaffolding they have employed in building their edifices: some much more than others.

The difference between a mathematical theory in the making and as a finished product is, perhaps, most strikingly illustrated by the case of geometry, as presented in its most approved modern shape. It is not too much to say that geometry, reduced to a purely deductive form—as presented, for example, by Hilbert, or by some of the modern Italian school—has no necessary connection with space. The words ‘point,’ ‘line,’ ‘plane’ are employed to denote any entities whatever which satisfy certain prescribed conditions of relationship. Various premises are postulated that would appear to be of a perfectly arbitrary nature, if we did not know how they had been suggested. In that division of the subject known as metric geometry, for example, axioms of congruency are assumed which, by their purely abstract character, avoid the very real difficulties that arise in this regard in reducing perceptual space-relations of measurements to a purely conceptual form. Such schemes, triumphs of constructive thought at its highest and most abstract level as they are, could never have been constructed apart from the space-perceptions that suggested them, although the concepts of spatial origin are transformed almost out of recognition. But what I want to call attention to here is that, apart from the basis of this geometry, mathematicians would never have been able to find their way through the details of the deductions without having continual recourse to the guidance given them by spatial intuition. If one attempts to follow one of the demonstrations of a particular theorem in the work of writers of this school, one would find it quite impossible to retain the steps of the process long enough to master the whole, without the aid of the very spatial suggestions which have been abstracted. This is perhaps sufficiently warranted by the fact that writers of this school find it necessary to provide their readers with figures, in order to avoid complete bewilderment in following the demonstrations, although the processes, being purely logical deductions from premises of the nature I have described, deal only with entities which have no necessary similarity to anything indicated by the figures.

A most interesting account has been written by one of the greatest mathematicians of our time, M. Henri Poincaré, of the way in which he was led to some of his most important mathematical discoveries.¹ He describes the process of discovery as consisting of three stages: the first of these consists of a long effort of concentrated attention upon the problem in hand in all its bearings; during the second stage he is not consciously occupied with the subject at all, but at some quite unexpected moment the central idea which enables him to surmount the difficulties, the nature of which he had made clear to himself during the first stage, flashes suddenly into his consciousness. The third stage consists of the work of carrying out in detail and reducing to a connected form the results to which he is led by the light of his central idea; this stage, like the first, is one requiring conscious effort. This is, I think, clearly not a description of a purely deductive process; it is assuredly more interesting to the psychologist than to the logician. We have here the account of a complex of mental processes in which it is certain that the reduction to a scheme of precise logical deduction is the latest stage. After all, a mathematician is a human being, not a logic-engine. Who that has studied the works of such men as Euler, Lagrange, Cauchy, Riemann, Sophus Lie, and Weierstrass, can doubt that a great mathematician is a great artist? The faculties possessed by such men, varying greatly in kind and degree with the individual, are analogous to those requisite for constructive art. Not every great mathematician possesses in a specially high degree that critical faculty which finds its employment in the perfection of form, in conformity with the

¹ See the ‘Revue du Mois’ for 1908.

ideal of logical completeness; but every great mathematician possesses the rarer faculty of constructive imagination.

The actual evolution of mathematical theories proceeds by a process of induction strictly analogous to the method of induction employed in building up the physical sciences; observation, comparison, classification, trial, and generalisation are essential in both cases. Not only are special results, obtained independently of one another, frequently seen to be really included in some generalisation, but branches of the subject which have been developed quite independently of one another are sometimes found to have connections which enable them to be synthesised in one single body of doctrine. The essential nature of mathematical thought manifests itself in the discernment of fundamental identity in the mathematical aspects of what are superficially very different domains. A striking example of this species of immanent identity of mathematical form was exhibited by the discovery of that distinguished mathematician, our General Secretary, Major Macmahon, that all possible Latin squares are capable of enumeration by the consideration of certain differential operators. Here we have a case in which an enumeration, which appears to be not amenable to direct treatment, can actually be carried out in a simple manner when the underlying identity of the operation is recognised with that involved in certain operations due to differential operators, the calculus of which belongs superficially to a wholly different region of thought from that relating to Latin squares. The modern abstract theory of groups affords a very important illustration of this point; all sets of operations, whatever be their concrete character, which have the same group, are from the point of view of the abstract theory identical, and an analysis of the properties of the abstract group gives results which are applicable to all the actual sets of operations, however diverse their character, which are dominated by the one group. The characteristic feature of any special geometrical scheme is known when the group of transformations which leave unaltered certain relations of figures has been assigned. Two schemes in which the space elements may be quite different have this fundamental identity, provided they have the same group; every special theorem is then capable of interpretation as a property of figures either in the one or in the other geometry. The mathematical physicist is familiar with the fact that a single mathematical theory is often capable of interpretation in relation to a variety of physical phenomena. In some instances a mathematical formulation, as in some fashion representing observed facts, has survived the physical theory it was originally devised to represent. In the case of electromagnetic and optical theory, there appears to be reason for trusting the equations, even when the proper physical interpretation of some of the vectors appearing in them is a matter of uncertainty and gives rise to much difference of opinion; another instance of the fundamental nature of mathematical form.

One of the most general mathematical conceptions is that of functional relationship, or 'functionality.' Starting originally from simple cases such as a function represented by a power of a variable, this conception has, under the pressure of the needs of expanding mathematical theories, gradually attained the completeness of generality which it possesses at the present time. The opinion appears to be gaining ground that this very general conception of functionality, born on mathematical ground, is destined to supersede the narrower notion of causation, traditional in connection with the natural sciences. As an abstract formulation of the idea of determination in its most general sense, the notion of functionality includes and transcends the more special notion of causation as a one-sided determination of future phenomena by means of present conditions; it can be used to express the fact of the subsumption under a general law of past, present, and future alike, in a sequence of phenomena. From this point of view the remark of Huxley that Mathematics 'knows nothing of causation' could only be taken to express the whole truth, if by the term 'causation' is understood 'efficient causation.' The latter notion has, however, in recent times been to an increasing extent regarded as just as irrelevant in the natural sciences as it is in Mathematics; the idea of thorough-going determinancy, in accordance with formal law, being thought to be alone significant in either domain.

The observations I have made in the present address have, in the main, had reference to Mathematics as a living and growing science related to and permeating other great departments of knowledge. The small remaining space at

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my disposal I propose to devote to a few words about some matters connected with the teaching of the more elementary parts of Mathematics. Of late years a new spirit has come over the mathematical teaching in many of our institutions, due in no small measure to the reforming zeal of our General Treasurer, Professor John Perry. The changes that have been made followed a recognition of the fact that the abstract mode of treatment of the subject that had been traditional was not only wholly unsuitable as a training for physicists and engineers, but was also to a large extent a failure in relation to general education, because it neglected to bring out clearly the bearing of the subject on the concrete side of things. With the general principle that a much less abstract mode of treatment than was formerly customary is desirable for a variety of reasons, I am in complete accord. It is a sound educational principle that instruction should begin with the concrete side, and should only gradually introduce the more general and abstract aspects of the subject; an abstract treatment on a purely logical basis being reserved only for that highest and latest stage which will be reached only by a small minority of students. At the same time I think there are some serious dangers connected with the movement towards making the teaching of Mathematics more practical than formerly, and I do not think that, in making the recent changes in the modes of teaching, these dangers have always been successfully avoided.

Geometry and mechanics are both subjects with two sides : on the one side, the observational, they are physical sciences; on the other side, the abstract and deductive, they are branches of Pure Mathematics. The older traditional treatment of these subjects has been of a mixed character, in which deduction and induction occurred side by side throughout, but far too much stress was laid upon the deductive side, especially in the earlier stages of instruction. It is the proportion of the two elements in the mixture that has been altered by the changed methods of instruction of the newer school of teachers. In the earliest teaching of the subjects they should, I believe, be treated wholly as observational studies. At a later stage a mixed treatment must be employed, observation and deduction going hand in hand, more stress being, however, laid on the observational side than was formerly customary. This mixed treatment leaves much opening for variety of method; its character must depend to a large extent on the age and general mental development of the pupils; it should allow free scope for the individual methods of various teachers as suggested to those teachers by experience. Attempts to fix too rigidly any particular order of treatment of these subjects are much to be deprecated, and, unfortunately, such attempts are now being made. To have escaped from the thralldom of Euclid will avail little if the study of geometry in all the schools is to fall under the domination of some other rigidly prescribed scheme.

There are at the present time some signs of reaction against the recent movement of reform in the teaching of geometry. It is found that the lack of a regular order in the sequence of propositions increases the difficulty of the examiner in appraising the performance of the candidates, and in standardising the results of examinations. That this is true may well be believed, and it was indeed foreseen by many of those who took part in bringing about the dethronement of Euclid as a text-book. From the point of view of the examiner it is without doubt an enormous simplification if all the students have learned the subject in the same order, and have studied the same text-book. But, admitting this fact, ought decisive weight to be allowed to it? I am decidedly of opinion that it ought not. I think the convenience of the examiner, and even precision in the results of examinations, ought unhesitatingly to be sacrificed when they are in conflict—as I believe they are in this case—with the vastly more important interests of education. Of the many evils which our examination system has inflicted upon us, the central one has consisted in forcing our school and university teaching into moulds determined not by the true interests of education, but by the mechanical exigencies of the examination syllabus. The examiner has thus exercised a potent influence in discouraging initiative and individuality of method on the part of the teacher; he has robbed the teacher of that freedom which is essential for any high degree of efficiency. An objection of a different character to the newer modes of teaching geometry has been frequently made of late. It is said that the students are induced to accept and reproduce, as proofs of theorems, arguments which are not really proofs, and thus that the logical

training which should be imparted by a study of geometry is vitiated. If this objection really implies a demand for a purely deductive treatment of the subject, I think some of those who raise it hardly realise all that would be involved in the complete satisfaction of their requirement. I have already remarked that Euclid's treatment of the subject is not rigorous as regards logic. Owing to the recent exploration of the foundations of geometry we possess at the present time tolerably satisfactory methods of purely deductive treatment of the subject; in regard to mechanics, notwithstanding the valuable work of Mach, Herz, and others, this is not yet the case. But, in the schemes of purely deductive geometry, the systems of axioms and postulates are far from being of a very simple character; their real nature, and the necessity for many of them, can only be appreciated at a much later stage in mathematical education than the one of which I am speaking. A purely logical treatment is the highest stage in the training of the mathematician, and is wholly unsuitable—and, indeed, quite impossible—in those stages beyond which the great majority of students never pass. It can then, in the case of all students, except a few advanced ones in the universities, only be a question of degree how far the purely logical factor in the proofs of propositions shall be modified by the introduction of elements derived from observation or spatial intuition. If the freedom of teaching which I have advocated be allowed, it will be open to those teachers who find it advisable in the interests of their students to emphasise the logical side of their teaching to do so; and it is certainly of value in all cases to draw the attention of students to those points in a proof where the intuitional element enters. I draw, then, the conclusion that a mixed treatment of geometry, as of mechanics, must prevail in the future, as it has done in the past, but that the proportion of the observational or intuitional factor to the logical one must vary in accordance with the needs and intellectual attainments of the students, and that a large measure of freedom of judgment in this regard should be left to the teacher.

The great and increasing importance of a knowledge of the differential and integral calculus for students of engineering and other branches of physical science has led to the publication during the last few years of a considerable number of text-books on this subject intended for the use of such students. Some of these text-books are excellent, and their authors, by a skilful insistence on the principles of the subject, have done their utmost to guard against the very real dangers which attend attempts to adapt such a subject to the practical needs of engineers and others. It is quite true that a great mass of detail which has gradually come to form part—often much too large a part—of the material of the student of Mathematics, may with great advantage be ignored by those whose main study is to be engineering science or physics. Yet it cannot be too strongly insisted on that a firm grasp of the principles, as distinct from the mere processes of calculation, is essential if Mathematics is to be a tool really useful to the engineer and the physicist. There is a danger, which experience has shown to be only too real, that such students may learn to regard Mathematics as consisting merely of formulæ and of rules which provide the means of performing the numerical computations necessary for solving certain categories of problems which occur in the practical sciences. Apart from the deplorable effect, on the educational side, of degrading Mathematics to this level, the practical effect of reducing it to a number of rule-of-thumb processes can only be to make those who learn it in so unintelligent a manner incapable of applying mathematical methods to any practical problem in which the data differ even slightly from those in the model problems which they have studied. Only a firm grasp of the principles will give the necessary freedom in handling the methods of Mathematics required for the various practical problems in the solution of which they are essential.

British Association for the Advancement of Science.

SHEFFIELD, 1910.

ADDRESS TO THE CHEMICAL SECTION

BY

J. E. STEAD, F.R.S., F.I.C., F.C.S.,

PRESIDENT OF THE SECTION.

[WITH 4 PLATES.]

It was with considerable diffidence that I accepted the position of President of this section. The long list of illustrious and eminent chemists who have occupied the chair in the past, scientists of the highest attainments, and usually professors of our educational institutions, is indicative of the very high standard to be followed. As, however, it was urged that a President with experience in the metallurgy of iron and steel was desired, I bowed to the decision of the Council, concluding that even as a mere layman I might, in this address, discuss one or more subjects to which prominent metallurgists have for the past thirty years directed their earnest attention, both in Europe and America. I refer to some of the underlying phenomena connected with the effect of sulphur and silicon on the carbon condition of commercial cast iron.

The effect of sulphur and silicon on cast iron has received the attention of Karsten, Percy, Weston, Howe, Keep, West, Dillner, Bachman, Summershach, Wüst, Johnson, Stoughton, Hailstone, Longmuir, Adamson, Turner and Schuler, Levy, and many others. They all agree in concluding that sulphur tends to make iron white by retaining the carbon in the combined state, and that silicon tends in the opposite direction. Professor Howe and Dr. Wüst have endeavoured to arrive at the exact quantitative effect of sulphur and silicon in preventing or facilitating the decomposition of the carbides.

Howe recognised that the data available are insufficient on which to make any final conclusion.

Wüst found, by a series of trials, that in pigs containing 3.15 per cent. carbon and about 1 per cent. silicon, on an average 0.01 per cent. sulphur prevented the separation of 0.02 per cent. graphite, but that with 2 per cent. silicon its effect was much less.

It is the general experience, that the effect of sulphur depends on the proportion, not only of silicon, but of the total carbon and manganese, and of the temperature at which the iron is cast, and the size and temperature of the mould into which the metal is run. Under some critical conditions 0.1 per cent. sulphur may prevent the separation of 3 per cent. graphite.

Howe's discovery—that the tendency of silicon, in increasing the decomposition of the carbides, is rapid at first, especially as the silicon rises from zero to 0.75

per cent., and then slower and slower with each further increase—is very important; so also is the generalisation of Messrs. Charpy and Grenet—that the separation of graphite on annealing iron which is initially white, containing the whole of the carbon in the combined condition, begins at a temperature which is the lower the greater the percentage of the associated silicon, and that the separation of graphite, once begun, continues at even lower temperatures than that at which it started.

The evidence advanced by Phillips, Prost, Campredon, Schulte, and others—that, on dissolving sulphurous irons in hydrochloric acid, all the sulphur is not given off as H_2S , and that a part either passes off as $\text{S}(\text{CH}_3)_2$ or remains behind with the solution as some organic product—was tentatively believed as indicative that the sulphur is chemically associated with the carbon and the iron.

Levy,¹ who has done much good work in the endeavour to determine the relations which exist between iron, carbon, and sulphur, in the alloys of these elements, states, as the result of his research, that there is no conclusive evidence of any chemical union.

In his tabulated results showing the amount of sulphur evolved presumably as $\text{S}(\text{CH}_3)_2$ on dissolving iron, carbon and sulphur alloys, the maximum is 0·06 per cent., but the average is very much less.

Schulte, on the other hand, had found that from 1 per cent. to 12 per cent. of the total sulphur is evolved as an organic sulphur compound; and Bischoff found an even greater quantity.

The results are apparently conflicting, and it is evidently obvious that more research is required in this direction.

It has been shown by Arnold and McWilliam, and confirmed by others, that carbide of iron does not decompose into graphite and iron during the annealing of steel until it segregates into relatively large masses. Taking this as a basis Mr. Levy has advanced an explanatory hypothesis as to how it is that sulphide of iron prevents the decomposition of carbides in white irons. He had found that during the solidification of irons free from silicon and manganese, but rich in sulphur, 'the sulphide separates at a temperature in the neighbourhood of 1130° C., together with, and as a component of, the austenite-cementite eutectic, forming a triple austenite-cementite-sulphide eutectic, the cementite component of which is interstratified with a jointed pearlite (by decomposition of austenite) sulphide one.' He stated that 'The presence of iron sulphide in the eutectic introduces intervening layers, which may partly ball up on annealing, but even then leave sulphide films between the cementite crystals; these act almost as emulsifiers, preventing the coalescence of the cementite portion, which is apparently a necessary preliminary to its decomposition into free carbon and iron. These layers and films are so persistent, even on slow cooling, as to retain their position between the cementite crystals, until the metal has cooled well below the temperature of decomposition, so that an iron which might otherwise become grey is retained, even on very protracted cooling, in the white form, by sulphur as sulphide; 0·25 per cent. sulphur being sufficient for this purpose under the moderately protracted cooling conditions of the research. It is not improbable that the mechanical force exerted by sulphide, on separation and cooling, may also prevent the physical conditions necessary for carbide decomposition, which, as is well known, is accompanied by considerable expansion.'

It is to be noted that Mr. Levy's argument is based on the effect of the sulphide films in the eutectic, preventing the segregation of the cementite into relatively large masses, which, as he expresses it, 'is apparently a necessary preliminary to its decomposition.'

His conclusions were based on the examination of hypo-eutectic alloys containing not more than 2·75 per cent. carbon and free from massive plates of cementite.

Whilst admitting that his conclusions may be correct, as applied to the eutectic, some other explanation would be necessary if decomposition did not occur when a considerable quantity of massive cementite initially were to form in the alloy.

That stable massive cementite can be so obtained in iron sulphide alloys I shall presently show.

¹ *Journal of the Iron and Steel Institute*, No. 2, 1908.

If it could be shown that sulphur in some form of combination with the iron and carbon does crystallise with the carbides, and that such mixture or solid solution is stable and not readily decomposed, it would be reasonable to conclude that the sulphur is responsible for the stability.

It has been suggested that silicon in iron decomposes the carbides according to the following chemical reaction: $3\text{Si} - {}^2\text{Fe}_3\text{C} = {}^2\text{Fe}_3\text{Si} - 2\text{C}$. The only objection to this explanation is that the silicon is not free in cast iron, as was proved by Turner, and, moreover, as will be shown presently, it is combined with iron in solid solution before the carbide is decomposed.

Gontermann¹ found that on adding pure silicon to molten iron, the iron and silicon combined with considerable rise in temperature, and I have noticed the same thing even when adding it to carburised iron.

The same authority, who has made a most careful study of the ternary alloys of the iron-carbon-silicon series, has shown that the eutectic freezing-point rises with the silicon from 1130° when silicon is absent, to about 1150° when it reaches 10 per cent., and to 1175° when it is about 17 per cent., and that the carbon in the eutectic of the alloys containing between 0 per cent. and 10 per cent. silicon, falls as the silicon rises by about 0.3 per cent. for each unit of silicon.

The same author proved that the pearlite reversion point in these alloys rises with the silicon on an average of about 30°C . for each unit of silicon in the alloys containing between 0 and 6 per cent. silicon. He concluded, but did not actually prove, that in the region of the curve of unvarying equilibrium two cementites crystallise; one a solid solution of the carbide and silicide of iron; and a second, a mixture of this with another ternary iron-silicon-carbon solid solution.

If the composition of the alloy lies between the curve of saturated silico-austenite and the curve of non-varying equilibrium, saturated silico-austenite primarily forms; and following this a secondary crystallisation of a binary eutectic consisting of this saturated austenite and silico-cementite.

In the year 1901 I described certain unique idiomorphic crystals which had been found in the hearth of a disused blast furnace at Blaina. The crystals were more or less oxidised on their exterior surfaces.

The analysis was as follows:—

	After deducting the Oxygen, &c. Per cent.
Manganese	. 54.56
Iron	. 37.71
Carbon	. 3.91
Silicon	. 3.82
	100.00

A micro-examination proved the crystals to be quite homogeneous mixtures, or solid solutions. It was difficult to assign to them any definite chemical constitution. They may be considered as silico-carbides of manganese and iron, and, as will be shown presently, bear a close relation to similar crystals which primarily form during the freezing of iron-carbon-silicon alloys.

Having briefly referred to the work of a number of authorities, I now propose to describe my attempts to supplement our knowledge in this direction by a purely micro-chemical research.

In order to understand the remarks which follow, it is necessary to briefly describe the changes which occur when pure iron-iron carbide alloys pass from the liquid to the solid state as are indicated by the researches of Osmond, Roberts, Austin, Stansfield, and of Carpenter and Keeling.

In the iron alloys containing less than the eutectic proportion of 4.3 per cent. carbon, described as hypo-eutectic alloys, austenite octahedral crystallites of the fir-tree type first fall out of solution, and these continue to grow till the liquid is so impoverished of iron and enriched in carbon that when the eutectic

¹ *Anorganische Chemie*, Bd. 59, 1908.

proportion of 4·3 per cent. carbon is reached, the liquid solidifies and breaks up into carbide of iron and austenite.

The hypereutectic alloys, containing more than the eutectic proportion of carbon, on cooling, first yield carbide of iron crystals, and these continue to grow till, by removal of the excess carbon, the eutectic proportions of iron and carbon are reached. The eutectic in its turn then freezes.

For the purpose of my research it was necessary to select pig metals, grey and high in silicon and white with high sulphur. These were kindly supplied by Messrs. Wilson, Pease and Co. and Messrs. Cochrane and Co., Middlesbrough. They were made from Cleveland ironstone and contained :—

	White	Grey Glazed Iron	
		No. 1	No. 2
	Per cent.	Per cent.	Per cent.
Combined carbon	2·98	nil	Trace
Graphite	traces	2·65	3·300
	0·29	0·72	0·676
Silicon	1·89	5·21	4·321
Sulphur	0·27	0·03	0·025
Phosphorus	1·62		1·660

It may be accepted that the sulphur in the white iron undoubtedly is the cause of the whiteness of the iron, whilst the excessively high silicon and low sulphur are equally responsible for the graphitic condition of the carbon in the grey irons.

The micro-structure of the high silicon metal was characteristic of all phosphoretic, high-silicon, carbon alloys. Curved plates of graphite cut the mass in many directions, whilst the binary eutectic of phosphorus and iron remained in irregular patches, generally midway between the graphite plates. The ground mass occupying the space between the eutectic and graphite plates consisted of silico-ferrite.

The interesting feature about the structure of the white iron is that there was no iron-iron-carbide eutectic. This had been replaced by the ternary eutectic of iron-phosphorus and carbon, which, according to Dr. Wüst, contains about :—

	Per cent.
Iron	91
Phosphorus	7
Carbon	2
	<hr/>
	100

There was evidence that the primary crystals of austenite of the octohedral skeleton type had been the first to fall out of solution, that the second crystal to form consisted of short plates of carbide of iron (cementite); whilst the ternary eutectic of phosphorus, carbon, and iron was the last to freeze and occupied spaces between the cementite plates and the primary crystals.

Dr. Carpenter and his assistant, Mr. Edwards, of Victoria University, Manchester, kindly obtained, for the purpose of this address, the cooling curves of these two typical metals. These were as follows :—

Grey Iron.

The long arrest at 1118° indicates a change of state, but is also coincident with important chemical changes. The second long arrest at 945° is due to freezing of the iron phosphorus carbon eutectic. The arrest at 850° indicates the formation of pearlite, and corresponds closely with the arrest in a similar alloy examined by Gontermann. The arrest at 690° is probably due to the formation of pearlite in the eutectic of iron and phosphorus, and is of great interest, for it points to the conclusion that silicon is not a constituent of the austenite of the ternary eutectic.

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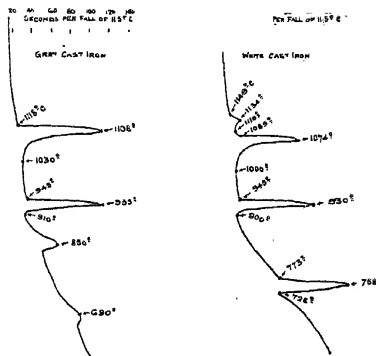


Diagram showing arrest in cooling; grey iron No. 2 on left, white iron on right.

White Iron.

The micro-structure and analysis help more fully to explain the arrests on cooling this alloy.

The first arrest at 1149° C. is where the primary austenite crystallises with the silicon, as will be shown presently.

The second arrest is where the primary cementite plates freeze.

The third arrest at 945° is the freezing point of the ternary eutectic, and is identical with that of the corresponding long arrest of the grey iron.

The fourth arrest at 770° is coincident with the formation of pearlite.

Bearing in mind that the manganese in the white iron was insufficient to combine with the whole of the sulphur present to form manganese sulphide, it is obvious that some other compound or compounds of sulphur existed. The microscope clearly revealed the presence of manganese sulphide and traces of free iron sulphide.

The carbide plates were quite free from striations of sulphide, such as had been noticed by Mr. Levy in the eutectic of high sulphur irons.

But for the sulphur present, the silicon would have been sufficient to effect a decomposition of the carbides, and the metal in absence of the sulphur would have given a grey instead of a white fracture. In view of this conclusion it appeared to be probable that if manganese were to be melted with the metal, it would combine with the sulphur associated with iron, etc., and crystallise as MnS, previous to the solidification of the carbide, or independently, and that the metal would then become grey on cooling.

In order to test this, a portion of the metal was melted in a clay pot with a little pure manganese, free from carbon—sufficient to give 1 per cent. of manganese, which was more than sufficient to combine with the whole of the sulphur. As soon as the mass was melted it was at once poured into a sand mould and allowed to set. When cold, it broke with a grey fracture corresponding to what is known as hard forge, and the combined carbon instead of being about 3 per cent. was reduced to 0.6 per cent., a result proving the correctness of the hypothesis.

It is well known that when manganese or chromium and some other metals are present in large quantities in pig irons, these metals, as carbides, crystallise with the carbide of iron forming double carbides, and these are much more stable than the massive pure iron carbide. It appeared reasonable to believe that if sulphide of iron, or some iron-sulpho-carbon compound, were to crystallise with the carbides it would have a similar effect.

Remembering that the conclusions on this question, as to whether sulphur does or does not crystallise with the carbides, are conflicting, it is evident that the only possible way to find out whether sulphur does so crystallise is to separate the carbide from the iron and test it for sulphur. With this object, a considerable quantity of the original Cleveland white metal was crushed to the very finest powder. It was then treated with a 10 per cent. solution of hydrochloric acid in water in large excess, and the action of the acid was allowed to continue until evolution of gas ceased. The insoluble matters, consisting mainly of carbides and phosphides, were filtered off, washed and dried, and were ground down in an agate mortar to a still finer powder, so as to liberate any mechanically entangled sulphides. The powder so dealt with was again treated with acid as before, after which the residue was filtered off, thoroughly washed with water, was transferred to a separate vessel, and was boiled with strong caustic-potash to dissolve any decomposition products.

The residue was again filtered off, was washed and dried, and submitted to analysis. The residue when dried weighed about 45 per cent. of the original metal, and contained as follows :—

	Per cent.
Iron	92.43
Carbon	6.06
Silicon	0.12
Sulphur	0.12
Phosphorus	0.97 (6.2 per cent. phosphide of iron)
Water, &c.	0.30

100.00

A second trial was made with the same metal; but, in this case, repounding and acid treatment were repeated three times, so as to eliminate the possibility of mechanical inclusion of sulphide or iron. The sulphur found in the remaining carbides was 0.1 per cent.

As the manganese in this metal was not sufficient to form manganese sulphide with the sulphur, it seemed desirable to determine whether or not when the manganese is in sufficient quantity sulphur would crystallise with the carbide. For this purpose the white chilled part of a crushing roll was experimented upon. The centre part was open grey iron, and contained 3.1 per cent. of the carbon as graphite.

The white chilled portion contained :—

	Per cent.
Combined carbon	3.75
Graphitic carbon	Trace
Manganese	0.65
Silicon	0.70
Sulphur	0.10
Phosphorus	0.23

It was crushed to powder and treated exactly in the same way as previously described for the separation of carbide. The residue contained by analysis :—

	Per cent.
Silicon	0.016
Sulphur	

a result showing that only a minute quantity of sulphur was crystallised with the carbide. Whether a different result would follow if both sulphur and manganese were greatly increased has yet to be determined.

Having proved that sulphur in some undetermined state of chemical combination does crystallise with carbide of iron, an attempt was made to determine the maximum amount of that element the carbide will retain under the most favourable conditions. With this object in view a considerable quantity of very pure white iron, containing only traces of silicon, sulphur, and phosphorus, and 3.5 per cent. of carbon, was melted in a plumbago crucible; and when in a molten condition sticks of roll sulphur were forced under the surface of the metal, and

afterwards the mixture was briskly shaken up with the sulphur which had liquefied on the surface.

Precisely the same result was obtained as described by Karsten, who had made a similar experiment. A metal was produced having a white fracture and large cleavage faces. The micro-structure was similar to that of hypereutectic iron carbon alloys. Large plates of carbide cut the metal in many directions, whilst between the carbide plates was located the triple carbide-sulphide-pearlite eutectic, so accurately described by Mr. Donald Levy.

The carbide plates themselves were peculiar in having circular prismatic inclusions of sulphide of iron symmetrically arranged at right angles to the sides of the plates. In horizontal sections of these plates they appeared as circular dots, sometimes arranged in continuous lines, suggesting that the sulphide had been actually in solution with the carbide when the metal was liquid, that they fell out of solution together, the sulphide separating and segregating along the cleavages of the carbide.

A portion of this sulphurous material was remelted and treated with a second quantity of sulphur. This time in addition to sulphide of iron a considerable quantity of the soot-like substance described by Karsten floated to the surface, and free graphite separated and stuck to the sides of the crucible.

The analyses of these metals are as follows :—

	After the first addition of Sulphur. Per cent.	After the second treatment with Sulphur. Per cent.
Carbon	4.37	4.39
Sulphur	about 1.00	1.00
Silicon	0.03	0.05

From which we may conclude that the maximum degree to which the carbon can be concentrated by this method is about 4.4 per cent. In these trials the carbide certainly had sufficient opportunity to become saturated with sulphur in each case. Both of the metals were crushed to exceedingly fine powder, and were treated with acid to decompose the free sulphides. The residues were repounded and treated with acid a second time, and afterwards with strong potash solution. After this treatment, analyses of the insoluble residues indicated, in one case 0.09 per cent. sulphur, and in the other 0.08 per cent. *From this it would appear that carbides will not carry in solid solution more than about 0.1 per cent. of sulphur.*

The metal containing 4.37 per cent. carbon and 1 per cent. sulphur, even on prolonged annealing, did not become graphite, a proof that the massive carbides present were quite stable.

The microscope reveals the fact that in almost all commercial white irons containing much sulphur the greater part of the sulphur is combined with either manganese or iron, and that the sulphides mainly exist as independent inclusions. It appears reasonable to assume that the manganese sulphide is without influence on the carbon condition, and that, although iron sulphide may have some influence, in the way suggested by Mr. Levy on the eutectic, *it is the sulphur that crystallises with the carbide which is mainly responsible in preventing the separation of graphite by making the carbide more stable.*

If it is assumed that the stability of the carbide depends on the quantity of sulphur which crystallises with it, and not on the total amount present in the metal carrying the carbides, it is clear that a great field of research is now open, the borders of which I have barely touched to co-relate their stability and sulphur contents.

The microscope does not show in what constituent the silicon crystallises. It is known that in grey irons it is associated with the ferrite and pearlite. but grey iron is the final result of the decomposition of carbide of iron and possibly silico-carbides, which primarily form during solidification, and although the silicon in the decomposed product may be entirely associated with the iron it is no proof that initially some of it may not have crystallised with the carbides.

In the white Cleveland iron, previously referred to, it is probable that the several constituents are present in the following proportions :—

TRANSACTIONS OF SECTION B.

	Per cent.
Silico-pearlite, the residue of the original austenite octa-	
hedral crystallites	42.50
Iron carbide in plates	33.66
Iron, phospho-carbide eutectic	23.10
Manganese sulphide	0.38
Iron sulphide	0.36
	<hr/>
	100.00

When fractionally dissolving the powdered metal in acid, it was the iron and associated silicon of the pearlite which passed into solution, and the carbide and phosphide which remained insoluble, and as these contained only 0.12 per cent. silicon, or about 0.06 per cent. on 100 parts of the original metal, it is evident that the pearlite must have contained $1.89 - 0.06 = 1.83$ per cent. of the silicon, or on 100 parts of it $\frac{100 \times 1.83}{42.5} = 4.3$ per cent., and that about 97 per cent. of the total silicon had crystallised with the austenite.

A little reflection will lead to the conclusion that if the carbon in the Cleveland white iron were to be gradually increased, the proportion of primary austenite crystallites would decrease, there would be less and less of them to carry the silicon, and this element would be concentrated in the diminishing solid austenite. It also follows, that if the carbon were to be so increased that no primary austenite would form, the silicon would have to crystallise in some other constituent.

In the example, referred to above, of the chilled casting, the carbides contained only 0.028 per cent. silicon, or 0.016 per cent. on the original metal. In this case, therefore, about 98 per cent. had crystallised with the primary austenite.

The question as to what amount of silicon will crystallise with the austenite so as to saturate it is probably variable with other variables. To determine this by chemical analysis would involve an exceedingly tedious research.

It is probable that as it increases, and as the austenite approaches more and more nearly to the saturation point, a gradually increasing proportion of the silicon will crystallise with the carbides.

It is well known that molten low silicon grey irons, in the absence of any appreciable quantity of sulphur, gives a white fracture when slightly chilled. Irons with above 5 per cent. silicon, when similarly treated, are supposed not to behave in the same manner; and this is quite true when any ordinary method of chilling is adopted. For instance, when the liquid silicious glazed metal No. 1 was run into water, the chilled iron contained graphite; but when a large drop was suddenly pressed into a sheet as thin as paper between cold plates of iron, the chilled metal was quite white and no graphite could be detected on dissolving it in nitric acid. The metal so chilled was difficult to dissolve in acid, and the silica produced, instead of forming a gelatinous bulky residue, remained in a close dense condition—indeed the thin chilled sheet, after all soluble matter had been removed, remained a rigid sheet of dense coherent silica, whereas the same metal allowed to cool slowly from the liquid state in a sand mould yielded to acid gelatinous silica.

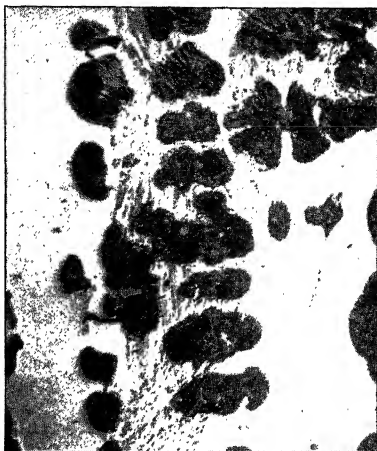
The different behaviour to acid treatment of the chilled as contrasted with that of the slowly-cooled metal indicates that the condition of the silicon in rapidly chilled metal is different from its condition in the same metal slowly-cooled.

In 1895 Mr. T. W. Hogg, of Newburn Steel Works, published an account of a very interesting observation, in which he showed the difference in the silicon solubility in different parts of the same pig iron—a portion of which was white and a portion grey. The iron referred to contained:—

	White part. Per cent.	Grey part. Per cent.
Combined carbon		0.98
Graphitic carbon	0.45 }	3.68 }
Silicon . . .	0.65	0.85
Manganese . .	1.63	1.60

No. 1.

Cleveland White Iron.



White=massive plates of Fe_3C .

Dark=pearlite, the decomposed austenite.

White and half-tone=ternary Fe-C-P eutectic.

No. 2.

Cleveland Glazed Iron.



Ground mass=silico-ferrite.

White complex=iron-iron phosphide eutectic.

Straight dark lines=graphite.

Illustrating the Presidential Address to Section B.

No. 3.

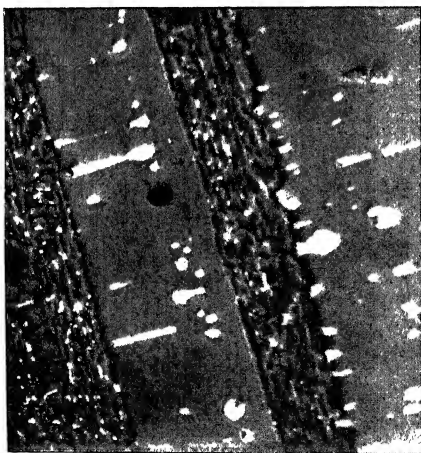
Iron-Carbon-Sulphur Alloy (4.37 per cent. Carbon).



White thick bands = massive carbide of iron.
 Complex structure = iron—iron-carbide-sulphide-pearlite eutectic.

No. 4.

Same as No. 2, heat-tinted and more highly magnified.



Broad bands = massive carbide of iron with inclusion of sulphide of iron.
 Complex structure = jointed eutectic of Fe-Fe₃C-FeS.
 The white specks are all FeS.

No. 5.

Same as No. 4. Section cut parallel to the surface of a massive carbide plate.



The ground mass is carbide of iron.
The white dots are sulphide of iron.

No. 6.

Glazed Cleveland Iron after melting with a little Sulphide of Iron.



White crystals=primary carbo-silicide of iron.
Dark==the second cementite.
Complex structure=iron-carbon-phosphorus eutectic.

No. 7.

An Iron-Carbon-Silicon Alloy, free from Phosphorus, made more stable by Sulphur.



Broken-up structure in the centre = the eutectic of two cementites, silico-carbide and carbide.

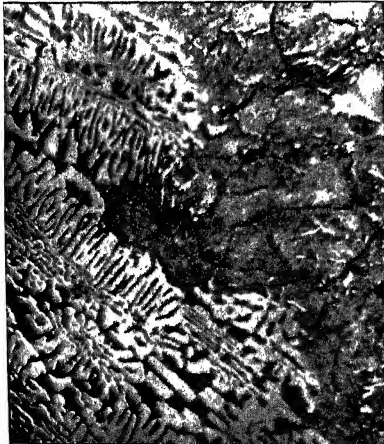
Half-tone = the carbide cementite.

Dark area = decomposed eutectic.

Light portion at right lower corner = crystallite of silico-pearlite.

No. 8.

Pure Iron-Iron Carbide Eutectic, the cooling of which was arrested before the complete decomposition of the Carbides into Austenite and Graphite.



White = carbide of iron.

Black lines = graphite.

Half-tone = pearlite.

He determined the solubility in dilute acid of the silicon in each portion, and found that the silicon soluble in hydrochloric acid was, in the grey part=about 81 per cent. and in the white part=about 48 per cent.

He found also that the silica left on treating the two varieties of metal in acid differed in character—that from the white portion was dense, whilst that from the grey metal was much more voluminous. The white metal contained the eutectic proportion of carbon, and therefore it could not contain any austenite crystallites; indeed with the silicon 0.60 per cent. also present it must be regarded as a hypereutectic alloy, and on that account we are forced to conclude that the silicon must have crystallised with the carbide.

It has long been known that on dissolving grey ferro-silicon containing even 6 per cent. silicon the silica gelatinises, whereas when the silicon approaches 10 per cent. much of the silica remains in a dense form. It is almost certain that during the solidification of the grey part of Mr. Hogg's pig iron a rich silicon cementite must have primarily formed, for the high carbon would not allow the formation of any primary silico-austenite; when this cementite decomposed the silicide part of it would become diluted with the iron of the decomposed carbide. It was, no doubt, this diluted solid solution in the cold grey metal which yielded the gelatinous silica.

That silicon does diffuse into iron, even at relatively low temperature, was proved by Lebeau. He found that free silicon and iron, when heated together in vacuo at 960° C., chemically combine, a fact I have fully confirmed, although it is impossible to get silicon to combine with iron on heating them together in a cementation furnace where oxidising gases have access to the silicon.

To determine whether silicide of iron would diffuse into and precipitate the graphite in white iron, a sample of crushed white iron free from impurities, containing 3.5 per cent. of carbon, was mixed with 10 per cent. by weight of a silicon alloy containing 20 per cent. of silicon (=Fe₃Si) also in powder. The mixture after compression in a short piece of iron tube was heated for two hours at 1000° C., in an atmosphere of hydrogen gas, and was then removed and cooled in air.

For comparison a portion of the crushed white iron was treated in the same way.

The combined carbon in the metals before and after heating were as follows :—

	Before. Per cent.	After. Per cent
In white metal alone .	3.5	3.44
„ „ and silicide	3.20	0.60

Not only does this trial prove that silicide does diffuse into carbide of iron and precipitate graphite, it has also an important bearing on the question as to why silicon in pig-iron, even in small quantities, causes the carbide to be decomposed. In the experiments with the chilled part of a casting containing only 0.7 per cent. silicon and 3.75 per cent. carbon it was shown that the carbide contained only 0.028 per cent. silicon, and that 98 per cent. of the total silicon was concentrated in the pearlite; yet this white iron on heating to 1000° C. became quite grey. *Are we not justified in concluding that it was the diffusion of silicide of iron from the silico-austenite into the carbides which caused the separation of graphite?*

As I had proved, first that sulphur crystallises with and makes the carbide of iron more stable, and second that in the presence of a fusible mother liquor rich in phosphorus, after the austenite crystallisation is complete, the carbide crystallises out in plates and not as iron carbide eutectic, it appeared probable that if, as Gontermann premised, two kinds of cementite actually form during the solidification of iron-carbide-silicon alloys, it might be possible to obtain them in a separate state by melting the rich silicon alloys with a little sulphur.

In order to test this a portion of the No. 1 grey glazed metal was melted, and when fluid a little sulphide of iron was mixed with it. The mixture was then cast in sand. Owing to the rapidity of the melting some of the graphite escaped and floated on the surface of the metal.

When cold it was found that the lower part of the small casting gave a white fractured surface, whilst the upper part was close grey.

The analyses were as follows :—

	White part. Per cent.	Grey part. Per cent.
Combined Carbon	2.06	1.46
Graphite	Trace	0.60
Manganese	0.03	0.03
Silicon	5.41	5.40
Sulphur	0.88	0.91
Phosphorus	1.50	1.50

The grey part, although slowly attacked by cold acid, did dissolve, yielding much voluminous silica. The white part was almost inert and only dissolved in strong hydrochloric acid with difficulty, and when the iron was dissolved out the remaining silica was of the dense variety, from which it would appear that the effect of the sulphide is akin to that of sudden quenching.

It was the micro-structure of the white portion, however, which was of unique interest. On 'heat-tinting' two kinds of hard crystals appeared, one more readily coloured by heating than the other. The more resistant crystals were idiomorphic, and were furnished with their terminal angles, but as they were embedded in the surrounding metal it was impossible to form any exact idea of the crystalline system to which they belonged.

The second order of crystals had evidently solidified at a later period, as their forms were interfered with by those of the idiomorphic type; they were much like ordinary plates of carbide of iron. The ground mass contained indications of octahedral on fir-tree crystallites and a well-developed phosphorus iron eutectic of the honeycomb type. This eutectic was the last to freeze, as it filled the spaces between the plates of the hard crystals. There was no pearlite excepting in the eutectic of phosphorus and iron. We can only tentatively conclude that of the two cementites the idiomorphic crystals contained the greater part of the silicon because of their greater resistance to oxidation and probably consisted of carbosilicide of iron, with sufficient sulphur in them to make them stable; also that the second crystals were carbide of iron, possibly containing a lesser quantity or no silicide in solid solution.

A further series of experiments was made on a portion of the same metal. In this case the molten iron was mixed and agitated with free sulphur instead of sulphide of iron, and the metal was at once poured into a sand mould in a thin layer. When cold it was quite white in fracture and had large brilliant cleavage faces.

It had the following composition :—

	Per cent.
Combined Carbon	2.60
Manganese	Trace
Silicon	6.65
Sulphur	0.93
Phosphorus	2.08

The sulphur had evidently effected concentration of the silicon phosphorus and carbon by removing some of the iron, as sulphide of iron was actually formed and floated on the surface of the iron. It was fractionally dissolved as described in previous cases, and the residue (72 per cent. of the weight of the original metal) was tested and found to contain :—

	Per cent.
Carbon	2.92
Manganese	Trace
Silicon	6.70
Sulphur	0.062
Phosphorus	1.410

This insoluble fraction evidently consisted of both classes of crystals, together with some phosphide of iron. Efforts were made to separate the crystals by chemical means, but without success.

On the long and continued action of strong hydrochloric acid a residue was

obtained containing a little less carbon and more silicon than were present in the mixture, an indication that the less soluble portion is different from that more soluble.

The micro-structure was similar to that of the metals of the previous trial, but as the carbon and silicon were higher the carbo-silicide was in greater quantity. It crystallised in long flat plates and not in relatively short idiomorphic crystals.

It is probable exception may be taken, with some justification, that the sulphur does not simply arrest the decomposition of the cementites, which I have premised primarily form, but may act in some other unknown way. An attempt was therefore made to find out whether they could be obtained by some other method without the aid of sulphur. As it is known that the ternary eutectic of iron, phosphorus, and carbon melts at about 945° C., it appeared probable that if silicon in small quantity were to be melted with an iron-carbon-phosphorus alloy very rich in phosphorus the two kinds of cementites would fall out of solution at a lower temperature, and would probably not decompose into graphite and silico-austenite in cooling down after their formation. To ascertain whether or not this would be the case, a fusible iron-phosphorus-carbon alloy containing more than the eutectic proportion of carbon was made. It had the following composition :—

	Per cent.
Iron	91.89
Phosphorus	5.37
Carbon	2.62
Silicon, &c.	0.10
Sulphur	0.02
	100.00

Four hundred grams were melted with sufficient silicon alloy to yield in the mixture :—

	Per cent.
Carbon	2.4
Phosphorus	5.0
Silicon	2.90
Sulphur	0.02

When melted a portion of it was cast in a sand mould, the remainder was allowed to cool in the crucible.

When cold, that cooled in the crucible was quite grey, whilst the portion cooled in sand was white at the lower part and grey on the top part of the casting, results which proved that the alloy was very unstable and that decomposition of the lower part of the casting was arrested by the slight chilling effect of the cold sand.

On microscopic examination of the white portion the ground mass was found to consist of the binary phosphorus iron eutectic, whilst two different cementites were embedded in it, one much more rapidly coloured on 'heat-tinting' than the other. The colours of the constituents of the properly heated and polished metal were as follows :—

Cementite (a)	White
" (b)	Red
Phosphide of iron	Purple
Iron pearlite crystallites	Grey

The part which broke with a grey fracture consisted of octahedral crystallites of silico-pearlite, the binary phosphorus iron eutectic, and undecomposed (red) cementite crystals, but there was a complete absence of the (white) cementite crystals. Graphite was also present in exceedingly fine plates, resembling what is known as temper graphite.

The evidence here is conclusive that even in the absence of sulphur :—

1st. Two cementites had formed.

2nd. That one cementite is much more unstable than the other variety, and decomposes in advance into silico-austenite and graphite.

Having proved that two different kinds of cementite do actually form and crystallise in the phosphorus eutectic it remained to ascertain in what way these crystallise in the absence of the phosphorus eutectic.

For this purpose two hypo-eutectic alloys were prepared without any phosphorus, but with sufficient sulphide of iron to check the decomposition of the carbides.

They contained :—

	1	2
	Per cent.	Per cent.
Carbon	2.40	2.10
Silicon	3.17	7.10
Sulphur	1.21	0.82
Phosphorus	0.02	0.02

These when cold after casting in sand broke with white fractures.

The carbides separated in the manner previously described contained :—

	1	2
	Per cent.	Per cent.
Carbon	6.16	3.00
Sulphur	0.09	0.08
Silicon	0.97	7.93

Percentage of carbides insoluble in acid	27.5	50.00
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The repeated acid treatment in this, as in all previous cases, no doubt dissolved a portion of the carbides, and what was actually weighed represented only a part of those actually present in the alloys.

In No. 2 alloy, after polishing and 'heat-tinting,' the microscope proved the presence of a few fir-tree crystallites embedded in a ground mass of cementite and a eutectic containing the two kinds of cementite, the No. 1 specimen containing a much smaller proportion of the cementite rich in silicon than No. 2.

As the metals had been somewhat rapidly cooled the alloy No. 2 was remelted, and was then allowed to cool in the crucible, so as to obtain a more coarsely crystallised eutectic. When cold, on polishing and 'heat-tinting,' the eutectic was clearly seen. There were the remains of large primary silico-austenite crystallites, plates of the red-coloured cementite, and a well-developed eutectic consisting of the (red) coloured and (white) cementites.

The cooling having been slow, this compound constituent had suffered partial decomposition in isolated patches into graphite and silico-ferrite, whilst the cementite coloured red remained intact.

There can be little doubt that the residue left insoluble in acid consisted of the two cementites, but in what proportion it is impossible to tell, as a method for isolating them has yet to be found.

Had the alloy contained a greater proportion of carbon the amount of cementite rich in silicon would have been in much greater proportion.

The trials, incomplete and necessarily imperfect as they are, go far to prove, just as Gontermann premised, that during the solidification of high silicon pig-irons two cementites fall out of solution together as a eutectic mixture.

They also have proved that the carbo-silicides are exceedingly unstable, breaking up into silico-austenite almost as soon as formed. *It is the instability of these silico-carbides which is mainly responsible for the graphitic character of grey irons rich in silicon and low in sulphur.*

Summary and Conclusions.

1. The experimental results advanced show proof that carbide of iron in presence of iron sulphide crystallises with a minute quantity of sulphur not exceeding about one-thousandth's part of the weight of the carbide, but the nature of the iron-carbon-sulphur compound has not yet been determined.

2. It seems almost, if not absolutely, certain that it is the sulphur crystallised with the carbide which makes the latter stable.

3. The evidence appears to support the view, long held by some and more recently accepted by others, that during the freezing of iron-carbon-hypo-eutectic alloys after the crystallisation of the primary austenite, and in the eutectic and hypereutectic alloys, it is the carbide and not graphite which primarily forms and that the carbide afterwards decomposes into graphite and austenite.

4. It has been proved by chemical methods that when the hypo-eutectic alloys, low in silicon, freeze, nearly all the silicon crystallises out with the primary austenite; and it follows that on gradually increasing the carbon so as to reduce the quantity of primary austenite, the silicon remaining constant, the austenite which does form must be as gradually enriched in silicon up to saturation-point; and, when that point is reached, the excess silicon crystallises out with a portion of the carbide of iron to form carbo-silicide of iron. Other elements remaining constant, the same result must follow on gradually increasing the silicon.

5. In the alloys of eutectic proportion and in the hypereutectic alloys, as no primary austenite can form, the silicon crystallises primarily with the carbide.

6. In Cleveland pig-iron containing about 1.5 per cent. phosphorus, a ternary eutectic of iron-carbon-phosphorus takes the place of the iron-iron-carbide eutectic. In white irons containing 3 per cent. carbon and under 2 per cent. silicon, after the primary austenite has fallen out of solution carrying practically all the silicon, it is not iron-iron-carbide which forms, but independent plates of cementite, or carbide of iron, and after these have crystallised and the residual mother liquor has arrived at the composition of the ternary iron-carbon-phosphorus eutectic, the latter solidifies at 945°C .

7. In Cleveland irons which become grey on cooling, and in which there is no primary austenite, the same iron-carbon-phosphorus eutectic is the only eutectic to form during cooling, and, instead of a ternary iron-carbon-silicon eutectic, two independent cementites crystallise—one a silico-carbide, and the other carbide of iron possibly containing a little silicide in solid solution. The micro-examination of the cold alloys, to which a little sulphur had previously been added when the metals were melted, led to the conclusion that it is the carbo-silico-cementite which primarily crystallises.

8. There is evidence that the primary carbo-silicides are exceedingly unstable and are the first to decompose into graphite and silico-austenite.

9. In the absence of any sensible quantity of phosphorus, two cementites form—one the silico-carbide cementite, the other the carbide cementite—and these crystallise together as a eutectic mixture.

10. The exact composition of the two cementites has not yet been determined, as no chemical method has been found for their isolation.

11. It is evident that it is the exceedingly unstable character of the silico-carbides which is responsible for the greyness of commercial metals rich in silicon and low in sulphur.

12. Silicide of iron when heated at 1000°C . with pure white iron free from silicon effects the decomposition of the carbide of the white iron. Based on this observation the hypothesis seems justifiable, in cases where all the silicon present in hypo-eutectic alloys crystallises out with the primary austenite, that after the carbide has solidified diffusion of the silicide follows, and this leads to the decomposition of the carbide of iron into graphite of iron.

13. Many of the results arrived at by chemical analysis support the hypothetical conclusions of Gontermann, who depended mainly on data obtained by thermal methods of treatment.

In conclusion, it will be clear from what I have stated that there are many gaps yet to be filled. I hope that the knowledge of this fact will lead others to follow up the research, which, in its present stage, is far from complete.

Proof.

British Association for the Advancement of Science.

SHEFFIELD, 1910.

ADDRESS TO THE AGRICULTURAL SUB-SECTION

BY

A. D. HALL, M.A., F.R.S.,

CHAIRMAN OF THE SUB-SECTION.

I BELIEVE it is customary for anyone who has the honour of presiding over a section of the British Association to provide in his presidential address either a review of the current progress of his subject or an account of some large piece of investigation by which he himself has illuminated it. I wish I had anything of the latter kind which I could consider worthy to occupy your attention for the time at my disposal; and as to a review of the subject, I am not without hopes that the sectional meetings themselves will provide all that is necessary in the way of a general review of what is going forward in our department of science. I have, therefore, chosen instead to deal from an historic point of view with the opinions which have prevailed about one central fact, and I propose to set before you this morning an account of the ebb and flow of ideas as to the causes of the fertility of the soil, a question which has naturally occupied the attention of everyone who has exercised his reason upon matters connected with agriculture. The fertility of the soil is perhaps a vague title, but by it I intend to signify the greater or less power which a piece of land possesses of producing crops under cultivation, or, again, the causes which make one piece of land yield large crops when another piece alongside only yields small ones, differences which are so real that a farmer will pay three or even four pounds an acre rent for some land, whereas he will regard other as dear at ten shillings an acre.

If we go back to the seventeenth century, which we may take as the beginning of organised science, we shall find that men were concerned with two aspects of the question—how the plant itself gains its increase in size, and, secondly, what the soil does towards supplying the material constituting the plant. The first experiment we have recorded is that of Van Helmont, who placed 200 lb. of dried earth in a tub, and planted therein a willow tree weighing 5 lb. After five years the willow tree weighed 169 lb. 3 oz., whereas the soil when redried had lost but 2 oz., though the surface had been carefully protected meantime with a cover of tin. Van Helmont concluded that he had demonstrated a transformation of water into the material of the tree. Boyle repeated these experiments, growing pumpkins and cucumbers in weighed earth and obtaining similar results, except when his gardener lost the figures, an experience that has been repeated. Boyle also distilled his pumpkins, &c., and obtained therefrom various tars and oils, charcoal and ash, from which he concluded that a real transmutation had been effected, 'that salt, spirit, earth, and even oil (though that be thought of all bodies the most opposite to water) may be produced out of water.'

There were not, however, wanting among Boyle's contemporaries men who pointed out that spring water used for the growing plants in these experiments contained abundance of dissolved material, but in the then state of chemistry the

discussion as to the origin of the carbonaceous material in the plant could only be verbal. Boyle himself does not appear to have given any consideration to the part played by the soil in the nutrition of plants, but among his contemporaries experiment was not lacking. Some instinct seems to have led them to regard nitre as one of the sources of fertility, and we find that Sir Kenelm Digby, at Gresham College in 1660, at a meeting of the Society for Promoting Philosophical Knowledge by Experiment, in a lecture on the vegetation of plants, describes an experiment in which he watered young barley plants with a weak solution of nitre and found how their growth was promoted thereby; and John Mayow, that brilliant Oxford man whose early death cost so much to the young science of chemistry, went even further, for, after discussing the growth of nitre in soils, he pointed out that it must be this salt which feeds the plant, because none is to be extracted from soils in which plants are growing. So general has this association of nitre with the fertility of soils become that in 1675 John Evelyn writes: 'I firmly believe that where saltpetre can be obtained in plenty we should not need to find other composts to ameliorate our ground'; and Henshaw, of University College, one of the first members of the Royal Society, also writes about saltpetre: 'I am convinced indeed that the salt which is found in vegetables and animals is but the nitre which is so universally diffused through all the elements (and must therefore make the chief ingredient in their nutriment, and by consequence all their generation), a little altered from its first complexion.'

But these promising beginnings of the theory of plant nutrition came to no fruition; the Oxford movement in the seventeenth century was but the false dawn of science. At its close the human mind, which had looked out of doors for some relief from the fierce religious controversy with which it had been so long engrossed, turned indoors again and went to sleep for another century. Mayow's work was forgotten, and it was not until Priestly and Lavoisier, De Saussure, and others, about the beginning of the nineteenth century, arrived at a sound idea of what the air is and does that it became possible to build afresh a sound theory of the nutrition of the plant. At this time the attention of those who thought about the soil was chiefly fixed upon the humus. It was obvious that any rich soils, such as old gardens and the valuable alluvial lands, contained large quantities of organic matter, and it became somewhat natural to associate the excellence of these fat, unctuous soils with the organic matter they contained. It was recognised that the main part of a plant consisted of carbon, so that the deduction seemed obvious that the soils rich in carbon yielded those fatty, oily substances which we now call humus to the plant, and that their richness depended upon how much of such material they had at their disposal. But by about 1840 it had been definitely settled what the plant is composed of and whence it derives its nutriment: the carbon compounds which constitute nine-tenths of the dry weight from the air, the nitrogen, and the ash from the soil. Little as he had contributed to the discovery, Liebig's brilliant expositions and the weight of his authority had driven this broad theory of plant nutrition home to men's minds: a science of agricultural chemistry had been founded, and such questions as the function of the soil with regard to the plant could be studied with some prospect of success. By this time also methods of analysis had been so far improved that some quantitative idea could be obtained as to what is present in soil and plant, and, naturally enough, the first theory to be framed was that the soil's fertility was determined by its content of those materials which are taken from it by the crop. As the supply of air from which the plant derives its carbonaceous substance is unlimited, the extent of growth would seem to depend upon the supply available of the other constituents which have to be provided by the soil. It was Daubeny, Professor of Botany and Rural Economy at Oxford, and the real founder of a science of agriculture in this country, who first pointed out the enormous difference between the amount of plant food in the soil and that taken out by the crop. In a paper published in the 'Philosophical Transactions' in 1845, being the Bakerian Lecture for that year, Daubeny described a long series of experiments that he had carried out in the Botanic Garden, wherein he cultivated various plants, some grown continuously on the same plot and others in a rotation. Afterwards he compared the amount of plant food removed by the

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crops with that remaining in the soil. Daubeny obtained the results with which we are now familiar, that any normal soil contains the material for from fifty to a hundred field crops. If, then, the growth of the plant depends upon the amount of this material it can get from the soil, why is that growth so limited, and why should it be increased by the supply of manure, which only adds a trifle to the vast stores of plant food already in the soil? For example, a turnip crop will only take away about 30 lb. per acre of phosphoric acid from a soil which may contain about 3,000 lb. an acre; yet, unless to the soil about 50 lb. of phosphoric acid in the shape of manure is added, hardly any turnips at all will be grown. Daubeny then arrived at the idea of a distinction between the active and dormant plant food in the soil. The chief stock of these materials, he concluded, was combined in the soil in some form that kept it from the plant, and only a small proportion from time to time became soluble and available for food. He took a further step and attempted to determine the proportion of the plant food which can be regarded as active. He argued that since plants only take in materials in a dissolved form, and as the great natural solvent is water percolating through the soil more or less charged with carbon dioxide, therefore in water charged with carbon dioxide he would find a solvent which would extract out of a soil just that material which can be regarded as active and available for the plant. In this way he attacked his Botanic Garden soils and compared the materials so dissolved with the amount taken away by his crops. The results, however, were inconclusive and did not hold out much hope that the fertility of the soil can be measured by the amount of available plant food so determined. Daubeny's paper was forgotten, but exactly the same line of argument was revived again about twenty years ago, and all over the world investigators began to try to measure the fertility of the soil by determining as 'available' plant food the phosphoric acid and potash that could be extracted by some weak acid. A large number of different acids were tried, and although a dilute solution of citric acid is at present the most generally accepted solvent I am still of opinion that we shall come back to the water charged with carbon dioxide as the only solvent of its kind for which any justification can be found. Whatever solvent, however, is employed to extract from the soil its available plant food, the results fail to determine the fertility of the soil, because we are measuring but one of the factors in plant production, and that often a comparatively minor one. In fact, some investigators—Whitney and his colleagues in the American Department of Agriculture—have gone so far as to suppose that the actual amount of plant food in the soil is a matter of indifference. They argue that as a plant feeds upon the soil water, and as that soil water must be equally saturated with, say, phosphoric acid, whether the soil contains 1,000 or 3,000 lb. per acre of the comparatively insoluble calcium and iron salts of phosphoric acid which occur in the soil, the plant must be under equal conditions as regards phosphoric acid, whatever the soil in which it may be grown. This argument is, however, a little more suited to controversy than to real life; it is too fiercely logical for the things themselves and depends upon various assumptions holding rigorously, whereas we have more reason to believe that they are only imperfect approximations to the truth. Still this view does merit our careful attention, because it insists that the chief factor in plant production must be the supply of water to the plant, and that soils differ from one another far more in their ability to maintain a good supply of water than in the amount of plant food they contain. Even in a climate like our own, which the textbooks describe as 'humid' and we are apt to call 'wet,' the magnitude of our crops is more often limited by want of water than by any other single factor. The same American investigators have more recently engrafted on to their theory another supposition, that the fertility of soil is often determined by excretions from the plants themselves, which thereby poison the land for a renewed growth of the same crop, though the toxin may be harmless to a different plant which follows it in the rotation. This theory had also been examined by Daubeny, and the arguments he advanced against it in 1845 are valid to this day. Schreiner has indeed isolated a number of organic substances from soils—di-hydroxystearic acid and picoline-carboxylic acid were the first examples—which he claims to be the products of plant growth and toxic to the further growth of the same plants. The evidence of toxicity as

determined by water-cultures requires, however, the greatest care in interpretation, and it is very doubtful how far it can be applied to soils with their great power of precipitating or otherwise putting out of action soluble substances with which they may be supplied. Moreover, there are as yet no data to show whether these so-called toxic substances are not normal products of bacterial action upon organic residues in the soil, and as such just as abundant in fertile soils rich in organic matter as in the supposed sterile soils from which they were extracted.

As then we have failed to base a theory of fertility on the plant food that we can trace in the soil by analysis let us come back to Mayow and Digby and consider again the nitre in the soil, how it is formed and how renewed. Their views of the value of nitrates to the plant were justified when the systematic study of plant-nutrition began, and demonstrated that plants can only obtain their supply of the indispensable element nitrogen when it is presented in the form of a nitrate, but it was not until within the last thirty years that we obtained an idea as to how the nitre came to be found. The oxidation of ammonia and other organic compounds of nitrogen to the state of nitrate was one of the first actions in the soil which was proved to be brought about by bacteria, and by the work of Schloesing and Müntz, Warington and Winogradsky we learnt that in all cultivated soils two groups of bacteria exist which successively oxidise ammonia to nitrites and nitrates, in which latter state the nitrogen is available for the plant. These same investigators showed that the rate at which nitrification takes place is largely dependent upon operations under the control of the farmer: the more thorough the cultivation, the better the drainage and aeration, and the higher the temperature of the soil the more rapidly will the nitrates be produced. As it was then considered that the plant could only assimilate nitrogen in the form of nitrates, and as nitrogen is the prime element necessary to nutrition, it was then an easy step to regard the fertility of the soil as determined by the rate at which it would give rise to nitrates. Thus the bacteria of nitrification became regarded as a factor, and a very large factor, in fertility. This new view of the importance of the living organisms contained in the soil further explained the value of the surface soil, and demolished the fallacy which leads people instinctively to regard the good soil as lying deep and requiring to be brought to the surface by the labour of the cultivator. This confusion between mining and agriculture probably originated in the quasi-moral idea that the more work you do the better the result will be; but its application to practice with the aid of a steam plough in the days before bacteria were thought of ruined many of the clay soils of the Midlands for the next half-century. Not only is the subsoil deficient in humus, which is the accumulated *débris* of previous applications of manure and vegetation, but the humus is the home of the bacteria which have so much to do with fertility.

The discovery of nitrification was only the first step in the elucidation of many actions in the soil depending upon bacteria—for example, the fixation of nitrogen itself. A supply of combined nitrogen in some form or other is absolutely indispensable to plants and, in their turn, to animals; yet, though we live in contact with a vast reservoir of free nitrogen gas in the shape of the atmosphere, until comparatively recently we knew of no natural process except the lightning flash which would bring such nitrogen into combination. Plants take combined nitrogen from the soil, and either give it back again or pass it on to animals. The process, however, is only a cyclic one, and neither plants nor animals are able to bring in fresh material into the account. As the world must have started with all its nitrogen in the form of gas it was difficult to see how the initial stock of combined nitrogen could have arisen; for that reason many of the earlier investigators laboured to demonstrate that plants themselves were capable of fixing and bringing into combination the free gas in the atmosphere. In this demonstration they failed, though they brought to light a number of facts which were impossible to explain and only became cleared up when, in 1886, Hellreigel and Wilfarth showed that certain bacteria, which exist upon the roots of leguminous plants, like clover and beans, are capable of drawing nitrogen from the atmosphere. Thus they not only feed the plant on which they live, but they actually enrich the soil for future crops by the nitrogen they leave behind in the roots and

stubble of the leguminous crop. Long before this discovery experience had taught farmers the very special value of these leguminous crops; the Roman farmer was well aware of their enriching action, which is enshrined in the well-known words in the *Georgics* beginning, 'Aut ibi flava seres,' where Virgil says that the wheat grows best where before the bean, the slender vetch, or the bitter lupin had been most luxuriant. Since the discovery of the nitrogen-fixing organisms associated with leguminous plants other species have been found resident in the soil which are capable of gathering combined nitrogen without the assistance of any host plant, provided only they are supplied with carbonaceous material as a source of energy whereby to effect the combination of the nitrogen. To one of these organisms we may with some confidence attribute the accumulation of the vast stores of combined nitrogen contained in the black virgin soils of places like Manitoba and the Russian steppes. At Rothamsted we have found that the plot on the permanent wheat field which never receives any manure has been losing nitrogen at a rate which almost exactly represents the differences between the annual removal of the crop and the receipts of combined nitrogen in the rain. We can further postulate only a very small fixation of nitrogen to balance the other comparatively small losses in the drainage water or in the weeds that are removed; but on a neighbouring plot which has been left waste for the last quarter of a century, so that the annual vegetation of grass and other herbage falls back to the soil, there has been an accumulation of nitrogen representing the annual fixation of nearly a hundred pounds per acre. The fixation has been possible by the *azotobacter* on this plot, because there alone does the soil receive a supply of carbohydrate, by the combustion in which the *azotobacter* obtained the energy necessary to bring the nitrogen into combination. On the unmanured plot the crop is so largely removed that the little root and stubble remaining does not provide material for much fixation.

Though numerous attempts have been made to correlate the fertility of the soil with the numbers of this or that bacterium existing therein, no general success has been attained, because probably we measure a factor which is only on occasion the determining factor in the production of the crop. Meantime our sense of the complexity of the actions going on in the soil has been sharpened by the discovery of another factor, affecting in the first place the bacterial flora in the soil and, as a consequence, its fertility. Ever since the existence of bacteria has been recognised attempts have been made to obtain soils in a sterile condition, and observations have been from time to time recorded to the effect that soil which has been heated to the temperature of boiling water, in order to destroy any bacteria it may contain, had thereby gained greatly in fertility, as though some large addition of fertiliser had been made to it. Though these observations have been repeated in various times and places they were generally ignored, because of the difficulty of forming any explanation: a fact is not a fact until it fits into a theory. Not only is sterilisation by heating thus effective, but other antiseptics, like chloroform and carbon bisulphide vapour, give rise to a similar result. For example, you will remember how the vineyards of Europe were devastated some thirty years ago by the attacks of phylloxera, and though in a general way the disease has been conquered by the introduction of a hardy American vine stock which resists the attack of the insect, in many of the finest vineyards the owners have feared to risk any possible change in the quality of the grape through the introduction of the new stock, and have resorted instead to a system of killing the parasite by injecting carbon bisulphide into the soil. An Alsatian vine-grower who had treated his vineyards by this method observed that an increase of crop followed the treatment even in cases where no attack of phylloxera was in question. Other observations of a similar character were also reported, and within the last five years the subject has received some considerable attention until the facts became established beyond question. Approximately the crop becomes doubled if the soil has first been heated to a temperature of 70° to 100° for two hours, while treatment for forty-eight hours with the vapour of toluene, chloroform, &c., followed by a complete volatilisation of the antiseptic, brings about an increase of 30 per cent. or so. Moreover, when the material so grown is analysed, the plants are found to have taken very much larger quantities of nitrogen and other plant foods from the treated soil; hence the increase of

growth must be due to larger nutriment and not to mere stimulus. The explanation, however, remained in doubt until it has been recently cleared up by Drs. Russell and Hutchinson, working in the Rothamsted laboratory. In the first place; they found that the soil which had been put through the treatment was chemically characterised by an exceptional accumulation of ammonia, to an extent that would account for the increased fertility. At the same time it was found that the treatment did not effect complete sterilisation of the soil, though it caused at the outset a great reduction in the numbers of bacteria present. This reduction was only temporary, for as soon as the soil was watered and left to itself the bacteria increased to a degree that is never attained under normal conditions. For example, one of the Rothamsted soils employed contains normally about seven million bacteria per gram—a number which remains comparatively constant under ordinary conditions. Heating reduced the numbers to 400 per gram, but four days later they had risen to six million, after which they increased to over forty million per gram. When the soil was treated with toluene a similar variation in the number of bacteria was observed. The accumulation of ammonia in the treated soils was accounted for by this increase in the number of bacteria, because the two processes went on at about the same rate. Some rearrangements were effected also in the nature of the bacterial flora; for example, the group causing nitrification was eliminated, though no substantial change was effected in the distribution of the other types. The bacteria which remained were chiefly of the class which split up organic nitrogen compounds into ammonia, and as the nitrate-making organisms which normally transform ammonia in the soil as fast as it is produced had been killed off by the treatment, it was possible for the ammonia to accumulate. The question now remaining was, What had given this tremendous stimulus to the multiplication of the ammonia-making bacteria? and by various steps, which need not here be enumerated, the two investigators reached the conclusion that the cause was not to be sought in any stimulus supplied by the heating process, but that the normal soil contained some negative factor which limited the multiplication of the bacteria therein. Examination along these lines then showed that all soils contain unsuspected groups of large organisms of the protozoa class, which feed upon living bacteria. These are killed off by heating or treatment by antiseptics, and on their removal the bacteria, which partially escape the treatment and are now relieved from attack, increase to the enormous degree that we have specified. According to this theory the fertility of a soil containing a given store of nitrogen compounds is limited by the rate at which these nitrogen compounds can be converted into ammonia, which, in its turn, depends upon the number of bacteria present effecting the change, and these numbers are kept down by the larger organisms preying upon the bacteria. The larger organisms can be removed by suitable treatment, whereupon a new level of ammonia-production, and therefore of fertility, is rapidly attained. Curiously enough one of the most striking of the larger organisms is an amoeba akin to the white corpuscles of the blood—the phagocytes, which, according to Metchnikoff's theory, preserve us from fever and inflammation by devouring such intrusive bacteria as find entrance in the blood. The two cases are, however, reversed: in the blood the bacteria are deadly, and the amoeba therefore beneficial, whereas in the soil the bacteria are indispensable and the amoeba become noxious beasts of prey.

Since the publication of these views of the functions of protozoa in the soil confirmatory evidence has been derived from various sources. For example, men who grow cucumbers, tomatoes, and other plants under glass are accustomed to make up extremely rich soils for the intensive culture they practise, but, despite the enormous amount of manure they employ, they find it impossible to use the same soil for more than two years. Then they are compelled to introduce soil newly taken from a field and enriched with fresh manure. Several of these growers here have observed that a good baking of this used soil restores its value again; in fact, it becomes too rich and begins to supply the plant with an excessive amount of nitrogen. It has also been pointed out that it was the custom of certain of the Bombay tribes to burn vegetable rubbish mixed as far as possible with the surface soil before sowing their crop, and the value of this practice in European agriculture, though forgotten, is still on record in the books on Roman agriculture. We can go back to the Georgics again, and there find an account of

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a method of heating the soil before sowing, which has only received its explanation within the last year, but which in some form or other has got to find its way back again into the routine of agriculture. Indeed, I am informed that one of the early mysteries, many of which we know to be bound up with the practices of agriculture, culminated in a process of firing the soil, preparatory to sowing the crop.

My time has run out, and I fear that the longer I go on the less you will feel that I am presenting you with any solution of the problem with which we set out—'What is the cause of the fertility of the soil?' Evidently there is no simple solution; there is no single factor to which we can point as *the* cause; instead we have indicated a number of factors any one of which may at a given time become a limiting factor and determine the growth of the plant. All that science can do as yet is to ascertain the existence of these factors one by one and bring them successively under control; but, though we have been able to increase production in various directions, we are still far from being able to disentangle all the interacting forces whose resultant is represented by the crop.

One other point, I trust, my sketch may have suggested to you: when science, a child of barely a century's growth, comes to deal with a fundamental art like agriculture, which goes back to the dawn of the race, it should begin humbly by accepting and trying to interpret the long chain of tradition. It is unsafe for science to be dogmatic; the principles upon which it relies for its conclusions are often no more than first approximations to the truth, and the want of parallelism, which can be neglected in the laboratory, gives rise to wide divergencies when produced into the regions of practice. The method of science is, after all, only an extension of experience. What I have endeavoured to show in my discourse is the continuous thread which links the traditional practices of agriculture with the most modern developments of science.

British Association for the Advancement of Science.

SHEFFIELD, 1910.

ADDRESS TO THE GEOLOGICAL SECTION

BY

PROFESSOR A. P. COLEMAN, M.A., Ph.D., F.R.S.,

PRESIDENT OF THE SECTION.

The History of the 'Canadian Shield.'

CAN there be any greater contrast than Pleistocene boulder clay resting on Archæan gneiss, the latest of rocks covering the earliest, with almost the whole known history of the world in the interval between? It is a fascinating occupation for a geological dreamer to sit on some hillside in Scotland or Finland or Northern Canada, where the schists and gneisses rise in rounded ridges or bosses through boulder clay, and ponder on all the strange happenings that separate the clay from the rock beneath.

The clay melting from its enclosed boulders under the frosts and rain seems the very emblem of the fleeting things of yesterday; while the Archæan gneiss and greenstones are the type of the solid, imperishable framework of the earth, on which all the later rocks rest.

The boulder clay recalls the white surface of a Continental ice-sheet with summer blizzards sweeping across it like those of the Antarctic tableland; while the gneiss beneath tells of a molten magma cooling during millions of years beneath miles of overlying rock.

It is the meeting-place of the geological extremes, and their contact marks the greatest of all discordances.

One thing the clay and the gneiss have in common—both were long neglected by geology; the Pleistocene beds because they were not rocks, but only 'drifts,' confused and troublesome things, hiding the real rocks, the orderly stratified formations; the 'basal complex' because its schists and gneisses were fossil-less, complex, and mysterious products of the dim beginnings of a world still 'without form and void.' The molten sphere, with its slowly consolidating crust, belonged rather to the astronomer than the geologist.

Geology has, of course, long lost that attitude, and now finds some of its most seductive problems in these once neglected extremes of the earth's history. Those who distrust the 'glacial nightmare' are now very few in number; but there are still revered veterans, like Professor Rosenbusch, who speak of the Archæan gneisses as parts of the earth's *Erstarrungskruste*, and who frame theories of the earth's cooling and wrinkling in its hot and furious youth.

Over more than half of Canada the field geologist is forced to occupy himself with both the Pleistocene and the Archæan, since the two are almost everywhere together, while the fossil-bearing beds of the vast intervening time are absent. The seemingly unnatural conjunction is not entirely without advantages; for the Pleistocene has furnished the clue to certain very puzzling problems of the Archæan, as will be shown later.

The geologists of the world have long known the broad outlines of the Canadian Archean or Pre-Cambrian area through Suess's masterly portrayal of the 'Canadian Shield,' and through Dana's account of the 'V Formation,' about which the North American Continent was built up.

It must be remembered, however, that, though most of the territory has been roughly traversed by Bell, Tyrrell, Low, and other explorers, only a few districts in the south have had their geology worked out in detail, because of their valuable deposits of silver, nickel, and iron ores. It is only in these districts and comparatively recently that the succession of Pre-Cambrian formations has been determined with certainty. In the wide spaces of the north only the most general relationships are known.

It is intended to bring together here our knowledge of the most ancient chapters in the history of North America as disclosed by recent field work.

Physiographic Features.

In its physiography the Canadian Shield shows the features that might be expected from one of the oldest and most stable land areas of the world. It was reduced in very early times to a peneplain, but later was elevated, permitting the rivers to begin a process of dissection. This process had a recent interruption by the Pleistocene Ice Age, which blocked many of the valleys with moraines and gave rise to the most extensive tangle of lakes in the world. Physiographically as well as geologically, the region shows a dramatic mingling of extreme youth with extreme old age.

The best account of this rejuvenated peneplain has been given by Dr. A. W. G. Wilson,¹ who shows that the gradients are very gentle, and suggests that two or more facets can be distinguished as having slightly different inclinations and as having been carved at different times. Here it will be unnecessary to take the matter up except in a general way.

The peneplain has been *unequally* elevated, parts standing 3,000 or 4,000 feet above the sea, and other parts sinking beneath its surface. Only at two marginal points can the Archean surface be said to rise as mountains: in the Adirondacks, projecting south-east into the State of New York; and in the Nachvak peninsula, just east of Ungava Bay.

To the south-west and south the shield sinks, almost imperceptibly in many places, beneath the older Paleozoic rocks, and the same is true around the central depression of Hudson Bay. Toward the south-east the shield breaks off suddenly along the great fault of the Lower St. Lawrence, and apparently the precipitous north-east shore of Labrador indicates faulting on even a larger scale. It has been suggested that Greenland, the Highlands of Scotland, Scandinavia, and Finland may have been parts of a single great shield, now separated through the settling down of the sea-bottoms.

In detail the region is full of variety of hill and valley, waterfall, river, and lake; but, on the whole, it is monotonous to the ordinary traveller from the constant repetition of similar forms, since there are no real mountain ranges and few outstanding 'monadnock' hills to break the sky line. The sweep of horizon from every hilltop seems horizontal, the summits around seldom rising more than 200 or 300 feet above the valleys, and all reaching nearly the same elevation.

The geologist finds, however, that this impression of general flatness is deceptive. In reality the rock structures are usually more nearly vertical than horizontal, as in most Archean regions. The schistose rocks, which form so much of the surface, commonly show dips of more than 60°, so that it is clearly a mountain region planed down to its foundations. The arrangement of valleys, ridges, and hills generally follows more or less closely these ancient rock forms.

Geological Structure.

Until recently most of the geological work done in this northern territory has been track surveys following Indian canoe routes. Here and there moraines or old lake deposits hide the rocks for a space, but usually the geology is admirably displayed as one's canoe threads the intricate waterways of sprawling lakes

¹ 'The Laurentian Peneplain': *Jour. Geol.*, vol. xl. No. 7, pp. 615-659.

spilling over from one irregular basin into another. On entering a new district there seems a hopeless confusion of pinkish gneiss and grey-green schist, but presently orderly forms take shape upon the map as the numberless bays and islands are explored, and the ground plan of vanished mountain ranges begins to show itself. Dr. Andrew C. Lawson, in his brilliant study of the Lake-of-the-Woods and Rainy Lake regions in 1884 to 1888, first brought out distinctly the relationships; and later work has added greatly to our knowledge of these ancient structures.

The typical arrangement is that of rounded or oval batholiths of gneiss, or of granite merging at the edges into gneiss, with schists dipping steeply away from them on all sides. Where the batholiths approach one another the green schists occupy narrow troughs between. As shown by Lawson, they are evidently the bottoms of synclines nipped in by the rising areas of granite and gneiss. Round these eruptive masses the schists have a strike parallel to the edge of the gneiss, so that they do not form ordinary synclines, but widen and narrow and swing in curves to adjust themselves to the varying relations of the batholiths. The meshes of green schist are often not complete, the curving ends feathering out to a point. In such places erosion has eaten the surface down below the bottom of the syncline.

The batholiths in Western Ontario are of all sizes, from a mile to 60 miles or more in diameter, and they are commonly somewhat elongated from west to east or from south-west to north-east. They do not always follow one another in orderly succession, but may lie scattered irregularly, almost like bubbles on foamy water. Yet on the large scale one can recognise a general trend in the direction of the longest axes of the batholiths, and the average strike of the schist in the various regions lies between 50° and 80° east of north, conforming to the same direction. This general east-north-east trend of the basement structures doubtless reveals the axial relations of the Archaean mountain ranges.

It is sometimes stated that the so-called V formation of North America was made up of two ranges converging toward the south, the easterly arm of the V parallel to the Appalachian mountains and the westerly one to the Rocky Mountains. The structural arrangement just outlined does not confirm this view, but suggests irregularly parallel chains, cutting the direction of the Rockies about at right-angles and that of the Appalachians at an acute angle.

Of what kind were the mountains erected on these bubble-like foundations of gneiss, set in meshes of schist? In many places they do not seem to have formed continuous ranges such as those of the Rockies, but rather groups of domes of various sizes. Some of them were comparatively low; others seem to have been lofty, though broad. Of the low ones the best known is that of the Grande Presqu' Isle in the Lake-of-the-Woods, an oval of gneiss 18 by 32 miles in dimensions. Here the up-swelling could not have been great, since the schists dip away from the gneiss at low angles all round, and patches of green schist, remnants of the roof, or perhaps of unusually large blocks stopped from above, are found here and there in the interior.

On the other hand, the Rainy Lake batholith, 30 by 50 miles in dimensions, must have risen as a lofty dome, since the surrounding schists dip away at high angles (60° to 90°). The arch of which they were the bases must have swung thousands of feet above the present surface of the batholith. Passing inwards from the Keewatin one finds at first immense slabs of the schist shifted a little and enclosed in gneiss, then bands of green material with softened edges, and finally darker cloudy streaks in the gneiss representing more perfectly digested bands. As Lawson has shown, the outer edge of the batholith is of greyish hornblende syenite gneiss or hornblende granite gneiss, while the interior is of ordinary mica granite gneiss. The outer part has absorbed a certain amount of basic Keewatin material.

One cannot doubt that this zone of green schist fragments, followed by greyish hornblende rock, originally extended over the dome as well as round its edges. In the middle there is now a width of 10 or 12 miles of the ordinary Laurentian gneiss. This implies, of course, that the upper part of the dome, afterwards removed, was several miles in thickness, and that the mountain mass rose correspondingly above the synclinal valleys. It must not be assumed that the dome had a regular surface, nor that it was unbroken. Such a batholith as

that of Rainy Lake was not made by a single sudden up-welling of granite, but by a long succession of slow inflows from various quarters. Meantime the rocks above must have been stretched and fractured during the long ages of elevation, and must have been exposed to the usual destructive forces, which may even have kept pace with the elevation during its late stages when differences of level became pronounced.

The coarse-textured granitoid gneiss making up the batholith must have cooled at great depths and exceedingly slowly.

The Raising of the Domes.

Some curious dynamical problems are involved in the raising of the domed mountains. It is conceivable that fluid lava could be forced by the unequal pressure of shifting mountain blocks through a suitable system of pipes into cisterns, so as to form laccolithic domes, but no such mechanism seems possible with batholiths. The granite of the batholiths was plastic rather than fluid, as shown by its having been dragged into the gneissoid structure. The areas affected covered sometimes 1,000 square miles. We know of no system of dykes to serve as pipes or passages, of no solid floor beneath, of no faulted blocks to provide the pressure. It is generally assumed that the protaxial granites and gneisses in great mountain ranges have risen because of the relief from pressure beneath anticlines due to lateral thrust. It is doubtful if these irregularly scattered ovals, sometimes 30 miles across, can be adjusted to any system of anticlines.

Some years ago I ventured another explanation. Granite is specifically lighter than most of the greenstones and schists of the Keewatin; and molten granite, even if not at a very high temperature, is lighter than the relatively cold rocks above it. If the rocks above were unequally thick, so that some areas were less burdened than others, it is conceivable that these differences in gravity might cause the granite to creep slowly up beneath the parts with the lightest loads, while the overlying rocks sagged into synclines in the heavily loaded parts.¹

Whatever their cause, these oval batholiths enclosed by meshes of schist are the most constant feature of the Canadian Archaean, though in many places erosion has cut so deeply that the meshes have all but disappeared, leaving only straight or curving bands of hornblende schist enclosed in the Laurentian gneiss. Very similar batholithic relations of the Laurentian with the Grenville series of Eastern Ontario are described by Drs. Adams and Barlow, though the batholiths are generally much smaller. Batholithic mountains were typical of the Archaean in North America, and, at least in some cases, also of Archaean regions in other parts of the world.

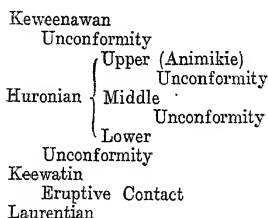
Subdivisions of the Canadian Pre-Cambrian.

Until recently the rocks of the Canadian Shield were usually divided into three parts—the Laurentian, the Huronian, and the Animikie and Keweenawau—the last two being only doubtfully included in the pre-Cambrian. These three divisions are still the only ones shown on the latest general map prepared by the Geological Survey. Lawson's separation of the Keewatin as a lower group than the Huronian was generally recognised as valid, but in practice the subdivision of the two in mapping was difficult, and was only carried out in detailed surveys. His proof that the Laurentian was eruptive and later than the Keewatin was accepted.

As the classification adopted by the American geologists in the Lake Superior region differed from that used in Canada, a Correlation Committee was appointed five or six years ago to draft a compromise, which runs as follows:—

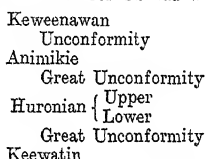
¹ Bull. *Geol. Soc. Am.*, vol. ix. pp. 223–238.

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This compromise system is now generally in use in Canada, though if Canadian relationships alone were considered the Animikie would be separated from the Huronian and placed closer to the Keweenawan, and the Laurentian would be treated as consisting of eruptive rocks frequently later in age than the Lower Huronian.

The most natural classification for Canada would be as follows :—



Laurentian = Post-Keewatin or Post-Huronian granite and gneiss.

The laccolithic domes described on previous pages were formed partly in the interval between the Keewatin and the Lower Huronian, but mostly later than the Lower Huronian. Over much of the shield, however, our knowledge of the relations is not sufficient to separate the mountain structures of the two ages.

Let us now consider the history of the region during the successive periods suggested above.

Conditions during the Keewatin.

One naturally asks what the conditions were in Keewatin times before the earliest known laccolithic mountains were raised. The granitic texture of the eruptives implies very slow cooling under great pressure. The old interpretation of these rocks, following the usual conception of the nebular hypothesis, made them parts of the earth's original crust, which cooled under the tremendous weight of an atmosphere including everything volatile at red heat, an atmosphere 200 or more times heavier than at present. We know, however, that this cannot apply to the Laurentian gneisses of Canada, since they push up eruptively through great thicknesses of older rocks—the Keewatin in the north and west, and the Grenville series in the east, including large amounts of water-formed deposits. Though these older rocks are now found only on edge in synclines protected on each side by domes of gneiss, there can be no doubt that they once spread out wide and flat on the surface of the earth.

The eruptives of the Keewatin have received most attention, but sedimentary rocks occur in it at all levels and with thicknesses of hundreds or thousands of feet. They include Lawson's Couchiching, with its great areas of mica schist and gneiss formed from what were originally muddy and sandy sediments. In other places quartzites and arkoses, slates and phyllites, represent less metamorphosed clastic materials. The slate is often black with carbon. In the north-west there is little limestone or dolomite, but the Grenville and Hastings series of the east, which are probably in part of Keewatin age, contain thousands of feet of limestone. All the ordinary types of sedimentary rocks were being deposited on the Keewatin sea bottoms, and one type unlike modern sediments—the banded silica and magnetite or hematite of the 'iron formation.' The rock last mentioned belongs to the top of the Keewatin, and is very widespread. Its

crumpled jaspers have attracted much attention because of their association with iron ore, but in reality the other varieties of sedimentary rocks are present in far greater amount both as to thickness and extent.

In almost every part of the western region there are associated with the sediments great sheets of basic lavas, agglomerates, and ash rocks, as well as smaller amounts of quartz porphyry, &c., showing that the Keewatin was one of the periods of great volcanic activity in the world's history. It is somewhat puzzling to find these predominantly basic volcanics in the Keewatin, while all the underlying eruptives of the Laurentian are decidedly acid, chiefly granite or syenite in composition.

The extensive sedimentary and eruptive rocks of this earliest formation imply that the ordinary geological processes were at work at the very beginning of known geological time, before the Archaean mountains came into existence. There must have been broad land areas where rocks like granite or gneiss weathered to mud and sand, probably under a cool climate, for the greenish arkoses and slates charged with carbon suggest cold rather than heat.

In the north-west volcanoes were active, but the east was comparatively free from eruptions. Both volcanic ash and ordinary clay and sand seem to have been spread out on the sea bottom in the Lake Superior region, and probably seaweeds throve in the mud. In the Grenville region the waters seem to have been clearer, and limestones were deposited on a very large scale, sometimes pure, but often muddy and mixed with a good deal of carbon, so that furoids probably flourished here also.

If we reconstruct the conditions of the Keewatin we must then assume conditions which have entirely vanished, on which weather, rain, and rivers worked, sweeping sediments down to the shallow or deeper seas to be spread out on a bottom which has also disappeared. The sediments and lavas and tuffs may be said to rest on nothing, for the once fluid or plastic Laurentian gneiss, cradling their synclines and pushing up from beneath them, could not have been the foundation on which they were laid down. Though the floor on which they once rested has nowhere been found, one may be certain that its materials included silica, alumina, and alkalis in the right proportions to fuse into a granitic magma, and this is practically all that is known of the pre-Keewatin world in Canada.

Rise and Fall of the Early Laurentian Mountains.

After the work of the volcanoes, of rain and frost and rivers, of winds and tides and currents, had piled up miles of rock in Keewatin times there came a great upheaval of mountains over thousands of square miles of the early Archaean surface. Possibly the earth was already shrinking through loss of volcanic material and of the steam and gases that exhale in eruptions. The Atlantic floor may have been settling down, thrusting inwards from the south-east, pushing up the weakened earth's crust beneath the shield into mountain rows; or it may be that some other cause must be sought for the somewhat haphazard domes which arose over such wide areas.

It may be suggested that the many thousands of feet of lava and stratified materials had so blanketed the lower-lying rocks that the heat from beneath crept up into them, softening and semi-fusing them, until in the slow lapse of time they began to flow sluggishly, ascending to form the wide-based domes of the Laurentian mountains. The source of the internal heat need not be discussed here. Uranium, with its various progeny, may have been as active then as now, or a more rapid axial rotation may have kneaded the discrete particles of a mass of planetesimals, and so warmed them up to the heat of fusion.

Then followed the deliberate and almost complete destruction of the great mountain system during a long period of time which has left no known Canadian record. The sediments derived from this destruction may have been piled on the bed of the Atlantic as it sank. It is possible that Sederholm's Bottnian in Finland may partially fill the gap.

Whatever disposal was made of the *débris*, several thousands of feet must have been carved from the mountains and swept out of view during the immense interval which separates the Keewatin and early Laurentian from the Lower

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Huronian, for the next series of rocks rests with a great discordance on the upturned edges of the synclinally disposed Keewatin schists and the truncated domes of Laurentian gneiss.

The Huronian.

The Lower Huronian has very different relationships from the Keewatin. Where least disturbed, as north of Lake Huron and in the Cobalt region, the floor beneath it is often well preserved. Dr. Miller has shown that at Cobalt the surface of Keewatin and Laurentian was hilly or hummocky before the basal conglomerate of the Lower Huronian was deposited; and Professor Brock, in describing the Larder Lake district to the north, refers to 'the clean-swept and often rounded surface of the older rocks on which it is frequently laid down.'¹

The basal conglomerate of the Lower Huronian contains pebbles and boulders of all the Keewatin and Laurentian rocks that went before, and among them are found beautifully striated stones. It is the oldest known boulder clay or tillite. The vast period of subaerial destruction that carved away the early Laurentian mountains ended in a glacial period, whose ice-sheets covered many thousands of square miles of North America, just as the last great period of peneplanation ended with the Pleistocene ice-sheets.

It is not a little impressive to see modern till resting on the Huronian tillite and including fragments of it as boulders. It is possible to break out from the modern glaciated surface stones whose underside received their polish and striæ in the Lower Huronian, while their upper surface has been smoothed and scratched by Pleistocene ice movements.

At Cobalt the tillite is accompanied by slate, which may be compared in all essential characters except hardness with the stratified clay of adjoining lake deposits of Pleistocene Age. The most recent and unconsolidated beds make clear the origin of some of the most ancient and, in appearance, most different rocks in the world.

In the Lake Huron region the action of ice was probably followed by an invasion of the sea, for the tillite is succeeded by thousands of feet of quartzite, arkose, and conglomerate, and by a few hundred feet of limestone. Possibly much or all of the limestones of the Grenville and Hastings series, which Dr. Adams reckons among the great limestone formations of the world, were formed at about the same time.

The Middle Huronian (Logan's Upper Huronian) is separated by a basal conglomerate, possibly glacial, from the Lower Huronian; but the break does not seem very profound, and the rocks do not differ much from those just described.

The least changed parts of the Huronian extend as a wide band for 200 or 300 miles north-east of Lake Huron, and in this area the uneven surface of Laurentian and Keewatin beneath the Lower Huronian boulder clay preserves for us a portion of the earliest dry land, the earliest peneplain known in America, and possibly in the world. This band has remained comparatively stable, while, so far as our information goes, all other parts of the Canadian Shield have undergone violent changes.

Rise of the Late Laurentian Mountains.

The Lower Huronian tillite has been found in many places throughout the Archæan region, over a stretch of 1,000 miles from east to west, and 700 miles from north to south; so that in all probability deposits like the Pleistocene till covered most of the surface.

Everywhere, however, except in the band extending north-east from Lake Huron, it seems to have been involved in later mountain building, and has been so sharply folded in with the Keewatin as to destroy the appearance of unconformity. It is instructive to note that so long and momentous an interval was entirely overlooked by geologists or treated as of small importance until a few years ago. There is usually no angular discordance to be observed, and the secondary schistose structures of Keewatin and Huronian are similar and parallel. The Huronian boulder conglomerate has often been rolled out to a schist in which only the harder boulders can be recognised as lenses; and sometimes even they are lost entirely, so that no evidence of discordance remains.

¹ *Bur. Mines, Ont., 1905, p. 31.*

It is evident that the invasion of the later Laurentian granites and gneisses was accompanied by very important dynamic and metamorphic effects. Most of the batholithic domes of North-western Ontario are post-Lower Huronian, and date perhaps from the Middle Huronian or the interval between it and the Upper Huronian (Animikie).

The granites and gneisses of this second time of mountain building have not been distinguished in mapping from those of the first in most places; and as they are both of precisely the same habit it will probably never be possible to separate them completely. Thus far both have been included under the name Laurentian, which must be considered as representing a lithological facies rather than a geological period. It may be, however, that the formation of batholithic mountains never really ceased from the end of the Keewatin to the end of the Lower Huronian. As the rocks called Laurentian are entirely eruptive, they should not be limited to a definite time, but only to a definite set of conditions as to composition, rate of cooling, and amount of pressure.

As in the earlier cycle, the period of mountain-building was followed by a period of destruction, ending in a peneplain of very wide extent.

The Animikie or Upper Huronian.

The interval between the lower formations and the Animikie is of great magnitude, perhaps even greater than that between the Keewatin and the Lower Huronian; and Lawson has suggested for it the name of the Epparchean interval. The Animikie has not been found resting on the Middle Huronian in Canada, so that this formation may partly bridge the chasm. Unless the Middle Huronian quartzites include part of the products of erosion we have no evidence as to the disposal of the many thousands of cubic miles of materials removed from the later Laurentian mountains.

The Animikie begins in most places with a thin basal conglomerate lying almost horizontally on the upturned edges of the previous schists and gneisses. Above this come chert, black slate, and other sediments, sometimes to the extent of 8,000 or 10,000 feet. The slate often contains carbon enough to make an important coal region if collected in definite beds.

The whole no doubt implies a transgressing sea, which ultimately must have covered a very large part of the Canadian Shield, since rocks of this age are found over wide surfaces north-west of Lake Superior, near Lake Mistassini, in the heart of Labrador, on the east side of Hudson Bay, and near Great Bear and Dubaut lakes. These rocks are found in Labrador up to 1,575 feet above the sea. This level, if extended in all directions, would submerge three-fourths of the Archaean peneplain.

At present these areas, though large, are widely separated; and it may be rash to assume that even soft, easily weathered rocks, like the Animikie slate, could have been completely removed from the intervening spaces. It is probable, however, that less than half of the Archaean then remained as dry land.

The Keweenawan.

There is an interval marked by a small discordance and a basal conglomerate between the Animikie and the Keweenawan, but the break in time was apparently not great. The two groups of rocks often occur together, though in many places the Keweenawan sediments overlap on to the Archaean, as in the neighbourhood of Lake Nipigon. Most of the Keweenawan sedimentary rocks are of shallow-water varieties, such as sandstone and conglomerate. At various places on the north-east shore of Lake Superior a coarse basal conglomerate is found as remnants preserved in small valleys or ravines in the granite. The ancient surface is now in process of resurrection by erosion, and the boulders once rolled on a Keweenawan shore are being freed from their matrix and once more set in motion by the waves of Lake Superior.

The Keweenawan, like the Keewatin, was a time of vigorous volcanic activity, and in most places the lava-sheets and laccolithic sills of diabase connected with their eruption far surpass the sediments in amount. The volcanic rocks are generally basic in character, and probably most of the diabase dykes widely found in almost all parts of the Canadian Archaean are of this age. The important

deposits of copper, nickel, and silver in Northern Canada are closely bound up with the Keweenaw basic volcanic rocks or with deeper-seated diabases probably of the same origin.

Here, as in the Keewatin, we are confronted with floods of basic lava coming up from unknown sources through the acid Laurentian gneiss. Do these basic lavas represent heavier segregations settling to the bottom during the slow movements of the granitic magma as it climbed into the Archæan batholiths? One might imagine these heavier and more liquid parts sinking beneath the lighter, more viscid, magmas of the domes, and remaining fluid until the mountain masses above had become completely solid. The supposed thrust from the Atlantic basin to the south-east might then bring strains to bear on the solid crust, more or less shattering and shifting its masses, squeezing up the still molten diabase through all the fractures and pores.

Several remarkable basins were formed in the Archæan peneplain by the ascent of these lavas, permitting the massive roof which formerly covered them to collapse by block faulting or by the formation of an irregular syncline. The basin of Superior seems to be of this nature. It is still rimmed by the Keweenaw lavas, sometimes accumulated to the thickness of 50,000 feet. Just to the north is the smaller basin of Lake Nipigon, with its edges and islands of diabase sheets; and to the east, near Sudbury, is the extraordinary synclinal basin, with which the great nickel mines are connected. These basins seem to have resulted from the collapse of the solid crust because of the removal of support when basic eruptives ascended from beneath.

Palæozoic History.

The exact relation of the Keweenaw to the Cambrian is somewhat in doubt, though most geologists make it pre-Cambrian. The St. Mary's, or Lake Superior, sandstone, which rests upon the Keweenaw with a slight discordance and overlaps upon the Archæan, is generally called Cambrian; it contains no fossils and occurs only along the shores of Lake Superior and St. Mary's River, so that its position in time is uncertain.

Potsdam sandstone, either Upper Cambrian or Lower Ordovician, rests upon the planed-down Archæan surface at the Thousand Islands and other points in Eastern Canada, often with a conglomerate at its base; and undoubted Ordovician limestones feather out upon the Laurentian all the way from Saskatchewan and Manitoba on the north-west through Ontario to the city of Quebec on the east. These limestones represent an important transgression of the sea upon the Canadian Shield. Apparently the old hummocky surface was often pretty cleanly swept, so that limestone with very little fragmental material, rests immediately upon the gneiss, but in other cases there is arkose or a basal conglomerate of Laurentian materials.

Occasionally Archæan hills rise island-like through the shaly limestone, which tilts away quaquaversally, as if the hill had protruded through the sediments. This appearance is probably due to the settling and shrinking of the mud in its consolidation to rock. Drill-holes east of Lake Ontario show that there were valleys hundreds of feet deep between these Archæan hills, so that in this region the peneplain was far from complete. These inequalities may be considered foothills of the Adirondack mountains farther east.

There is reason to believe that before the close of the Ordovician the sea crossed from the region of Lake Winnipeg to Hudson Bay, flooding all the lower parts of the shield; but probably most of Labrador and part of Franklin, north-west of Hudson Bay, remained as dry land.

The Silurian follows on the Ordovician without a discordance, and at this time the sea probably submerged an even larger part of the shield, since the Silurian limestone of James's Bay is only 250 miles from that south of the Great Lakes, and there are two outliers between—on Lakes Nipissing and Temiscaming. It may be added that the highland of Silurian limestone crossing Southern Ontario, with a bold escarpment facing north-east, rises hundreds of feet higher than the watershed towards Hudson Bay. The escarpment facing the Archæan 'old land' corresponds to the Scandinavian 'glint,' and has a similar relation to the lakes of the Archæan border.

The Devonian Sea also encroached south of James Bay and along the south-west side of the shield from Clear Lake, in Saskatchewan, to Great Bear Lake.

What took place on the Archaean continent while the coal forests flourished on the lowlands to the south and to the far north is unknown, since no carboniferous rock has been found on its surface.

Mesozoic and Cenozoic History.

Early Mesozoic times are a blank, but a few small outcrops of Cretaceous rocks resting on the Archaean toward the south-west show that portions of its rim were once more under water. Dr. Wilson believes that an important facet of the peneplain should be dated from the Cretaceous, since planation was going on in parts of the United States at this time; but no positive evidence of this is at hand.

Nor is there any evidence as to its history in the tertiary before the oncoming of the Ice Age of the Pleistocene, when its whole surface was scoured more than once by great glacial sheets. The mantle of decayed rock which must have accumulated during the long dry land stage was almost completely swept away, leaving the rounded surfaces of ancient rock fresh and clean beneath the boulder clay.

In an important inter-glacial interval and in post-glacial times much of the morainic material was assorted in great lakes whose shore and deep water deposits cover large parts of the surface. With the departure of the ice the sea once more transgressed upon the lower parts of the shield, but the land has been rising since, leaving a belt of marine deposits up to about 500 feet around the shores of Hudson Bay, the St. Lawrence, and the Atlantic.

How much of the Shield has been Covered?

It is generally stated that the Canadian Shield has been dry land since the Archaean, and hence that erosion has been taking place ever since that time. This is probably true for part of the north-eastern portion of the shield and perhaps also the north-western, but much of the area, especially toward the south, was buried in early days under Palaeozoic sedimentary rocks, and so protected from further destruction. These sediments are still being slowly stripped from the Archaean in many places.

This may account for the greater proportion of Huronian and Keewatin rocks in the south as compared with the north. It is probable that in the unprotected northern parts weathering agencies have eaten the higher Archaean rocks completely away from the Laurentian gneiss beneath. Before asserting this positively, however, it may be well to await more thorough exploration of the little known north.

It is possible, but not very probable, that the whole area was at one time covered with Ordovician or Silurian shale and limestone. If so, all traces of this capping have been removed from hundreds of thousands of square miles of its surface.

There is one very impressive feature of the Archaean as found beneath the later rocks. The peneplain, with its rounded, hummocky surface, seems exactly the same when one strips from it recent boulder clay, early Palaeozoic shale or sandstone or limestone, Keweenawan eruptives, or even Lower Huronian tillite, where this has remained undisturbed. It is as though all the millions of years of destruction since the Middle Palaeozoic had made only unimportant changes in the pre-Cambrian peneplain. When it is recalled that peneplanation took place twice in the pre-Cambrian, before the Lower Huronian and before the Animikie, one is almost driven to think that pre-Cambrian time is far longer than post-Cambrian.

Relation of the Shield to the Palaeozoic.

Except toward the east, the Canadian Shield sinks gently beneath Palaeozoic beds, in most cases retaining its character as a peneplain. How far does it continue to the south and west beneath the sedimentary rocks, and to what depth does it extend?

The results of drilling at Toronto, 80 miles south of the contact, show gneiss and crystalline limestone at a depth of 1,200 feet below the surface, or 940 feet below sea-level. Near Lake Erie, 130 miles to the south of the contact, the Archæan is reached at a depth of 3,300 feet—2,700 feet below sea-level. Its slope to Toronto is at the rate of 20 feet per mile, and from Toronto to Lake Erie at the rate of 35 feet per mile. This corresponds fairly well with the dip of the overlying Palæozoic rocks.

As the peneplain rises more than 1,300 feet above sea-level at the watershed 300 miles north of Lake Erie, there is a difference of 4,000 feet in a north and south direction; and if comparison is made with the Adirondack Mountains 250 miles to the east the difference even amounts to 6,600 feet. It is probable, however, that the Adirondacks were a residual group of mountains never reduced to the general peneplain level. It is clear that the pre-Palæozoic peneplain has been greatly warped in later ages, perhaps as a result of the increasing load of sediments piled on its southern edge.

One is apt to think of these ancient crystalline rocks as an exceedingly solid and resistant block of the earth's crust, likely to undergo little deformation; so that this evidence of warping or doming of the surface comes as a surprise. In reality shiftings of level under changes of load are normal in every region, and have been going on along the southern border of the Canadian Shield all through Pleistocene times, and perhaps continue now.

The proof of this is to be found in the differential elevation of the shore-lines of the great post-glacial lakes, which ascend with an increasing grade toward the north (N. 20° E.). In the case of Lake Iroquois the difference in level between the two ends of the earliest shore is more than 500 feet, and the grade toward the north even rises to six or seven feet per mile. If we add 230 feet of deformation of the marine beaches, which followed Lake Iroquois toward the north-east after the final melting of the ice, there is a known change of level amounting to 730 feet within late Pleistocene times. There is reason to believe that similar changes of level took place during the inter-glacial period recorded at Toronto and to the north.

The Pleistocene sinkings and risings are naturally accounted for by the piling up and removal of the thousands of feet of ice in the Glacial Periods, though probably isostatic equilibrium was not reached in these movements.

We know that the ice was more than 4,000 feet thick, since it passed over the tops of the Adirondack mountains. This thickness of ice is equal in weight to about 1,600 feet of rock, while the greatest known elevation since the removal of the load is not much more than 700 feet, implying that a weight of 900 feet of rock can be supported by the shield. It may be, however, that in the interior of Labrador, where no beach-lines give evidence as to changes of level, the doming is much greater than the amount suggested.

It is of interest to note that these adjustments to change of load take thousands of years to accomplish. The rise due to the melting of the Labrador ice-sheet may be going on slowly now, 30,000 or 40,000 years after the load was lifted.

These sinkings and risings must be accomplished by plastic flow outwards from beneath the loaded area or inward toward the area relieved of its load.

Instead of a rigid, unyielding shield, we must conceive a stiffly flexible covering over a plastic substratum, where during thousands of years adjustments of level, amounting to hundreds of feet, may take place; and during millions of years of removal of load by erosion, or of piling on of load through sedimentation, changes of level of thousands of feet can be accomplished. Such changes have taken place on the southern and western sides of the shield without any known rupture, while on the east the adjustment has been accomplished in part by great faults.

Has the Archæan, which is supposed to underlie the stratified rocks in all parts of the world, undergone the same vicissitudes?

Summary.

The history of the Canadian Shield begins in pre-Keewatin times, with land surfaces on which weathering took place, and seas in which mud and sand were deposited. If the earth were ever molten, that stage had long been passed before

the Keewatin sediments were laid down, for they include carbon probably derived from fucoids, which could not have lived in a hot sea.

The pre-Keewatin land surfaces and sea bottoms have totally disappeared, so far as known to Canadian geology. Apparently they have been fused and transformed into the gneisses of the Laurentian.

The Keewatin was a time of great volcanic activity, lava streams and ash rocks surpassing in amount the thick sheets of sediments. At the end of the Keewatin the thousands of feet of volcanic and clastic rocks were lifted as domes by the up-welling of batholiths of early Laurentian gneiss.

Then followed a profound gap in the record, during which the mountains were levelled to a hummocky peneplain. This gap represents a very long period of weathering and destruction on a land surface, ending in glacial action on a large scale.

The Lower Huronian begins with the deposit of a thick and widespread boulder clay, followed up by a transgression of the sea in which mud and sand, and also limestone and chert, were deposited.

After a short break similar processes went on in the Middle Huronian. During the Middle Huronian, or in the interval between it and the Upper Huronian (Animikie), mountain-building was renewed on a grand scale, many synclines of Keewatin and Lower Huronian rocks being caught between the rising batholiths of late Laurentian gneiss. A broad central band of the Lower Huronian escaped this process, however, and has preserved its original attitude on a floor of Keewatin and Laurentian.

The Animikie or Upper Huronian sediments which rest on the planed-down floor of upturned Lower Huronian, Laurentian, and Keewatin rocks consist largely of chert and carbonaceous slate or shale, which lie nearly horizontal and have undergone very little change.

The Keweenaw follows the Animikie with only a small break, and includes shallow water-beds of sandstone and conglomerate, accompanied by immense outflows of lava. As a result of the outpouring of lava great basins, like that of Superior, resulted. It is probable that during the Animikie and Keweenaw most or all of the Canadian Shield was covered by the sea.

The Keweenaw is generally held to mark the close of the Archaean (or Algonkian or Proterozoic). Low reports portions of these formations as having been caught in mountain-building of the Laurentian type in Labrador, but commonly they have not been disturbed.

During early Palaeozoic times the Canadian Shield was more than once encroached upon by the sea, though probably much of the peninsula of Labrador, and perhaps a region north-west of Hudson Bay, escaped.

From the Devonian to the Pleistocene the shield seems to have remained dry land, and part of the Ordovician and Silurian capping of sediments was removed during this long period.

The succession of Pleistocene ice-sheets completed the work of denudation, and at the end of the Ice Age many thousands of square miles of the lower portions were once more beneath the sea.

Last of all, the region has been rising at unequal rates in different parts, as shown by the warping of marine and fresh-water beaches.

The surface of low hills and rounded knolls of gneiss and schists beneath the Pleistocene boulder clay resembles in every way that beneath the flat shales and limestones of the early Palaeozoic, or the nearly horizontal sediments of the Animikie, or even the undisturbed parts of the Lower Huronian boulder clay. It may be that much of the surface has been covered with sediments and restored to daylight by subaerial erosion several times in succession. The greater part of the carving-down seems to have been done before the Animikie—i.e., within pre-Cambrian times—and the pre-Huronian surface seems as mature as any of the later ones. The bearing of this on the length of early geological time is evident. Pre-Huronian time includes the laying down of thousands of feet of Keewatin sediments, the elevation of early Laurentian mountains, and the levelling of these mountains to a peneplain. It may be as long as post-Huronian time.

British Association for the Advancement of Science.

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ADDRESS TO THE ZOOLOGICAL SECTION BY Professor G. C. BOURNE, M.A., D.Sc., F.R.S., PRESIDENT OF THE SECTION.

IN choosing a subject for the address with which it is my duty, as President of this Section, to trouble you, I have found myself in no small embarrassment. As one whose business it is to lecture and give instruction in the details of comparative anatomy, and whose published work, *qualecunque sit*, has been indited on typical and, as men would now say, old-fashioned morphological lines, I seem to stand self-condemned as a morphologist. For morphology, if I read the signs of the times aright, is no longer in favour in this country, and among a section of the zoological world has almost fallen into disgrace. At all events, I have been very frankly assured that this is the case by a large proportion of the young gentlemen whom it has been my fate to examine during the past two years; and, as this seems to be the opinion of the rising generation of English zoologists, and as there are evident signs that their opinion is backed by an influential section of their elders, I have thought that it might be of some interest, and perhaps of some use, if I took this opportunity of offering an apology for animal morphology.

It is a sound rule to begin with a definition of terms, so I will first try to give a short answer to the question 'What is morphology?' and, when I have given a somewhat dogmatic answer, I will try to deal in the course of this address with two further questions: What has morphology done for zoological science in the past? What remains for morphology to do in the future?

To begin with, then, what do we include under the term morphology? I must, first of all, protest against the frequent assumption that we are bound by the definitions of C. F. Wolff or Goethe, or even of Haeckel, and that we may not enlarge the limits of morphological study beyond those laid down by the fathers of this branch of our science. We are not—at all events we should not be—bound by authority, and we owe no allegiance other than what reason commends to causes and principles enunciated by our predecessors, however eminent they may have been.

The term morphology, stripped of all the theoretical conceptions that have clustered around it, means nothing more than the study of form, and it is applicable to all branches of zoology in which the relationships of animals are

determined by reference to their form and structure. Morphology, therefore, extends its sway not only over the comparative anatomy of adult and recent animals, but also over palæontology, comparative embryology, systematic zoology and cytology, for all these branches of our science are occupied with the study of form. And in treating of form they have all, since the acceptance of the doctrine of descent with modification, made use of the same guiding principle—namely, that likeness of form is the index to blood-relationship. It was the introduction of this principle that revolutionised the methods of morphology fifty years ago, and stimulated that vast output of morphological work which some persons, erroneously as I think, regard as a departure from the line of progress indicated by Darwin.

We may now ask, what has morphology done for the advancement of zoological science since the publication of the 'Origin of Species'? We need not stop to inquire what facts it has accumulated: it is sufficiently obvious that it has added enormously to our stock of concrete knowledge. We have rather to ask what great general principles has it established on so secure a basis that they meet with universal acceptance at the hands of competent zoologists?

It has doubtless been the object of morphology during the past half-century to illustrate and confirm the Darwinian theory. How far has it been successful? To answer this question we have to be sure of what we mean when we speak of the Darwinian theory. I think that we mean at least two things. (1) That the assemblage of animal forms as we now see them, with all their diversities of form, habit, and structure, is directly descended from a precedent and somewhat different assemblage, and these in turn from a precedent and more different assemblage, and so on down to remote periods of geological time. Further, that throughout all these periods inheritance combined with changeability of structure have been the factors operative in producing the differences between the successive assemblages. (2) That the modifications of form which this theory of evolution implies have been rejected or preserved and accumulated by the action of natural selection.

As regards the first of these propositions, I think there can be no doubt that morphology has done great service in establishing our belief on a secure basis. The transmutation of animal forms in past time cannot be proved directly; it can only be shown that, as a theory, it has a much higher degree of probability than any other that can be brought forward, and in order to establish the highest possible degree of probability, it was necessary to demonstrate that all anatomical, embryological, and palæontological facts were consistent with it. We are apt to forget, nowadays, that there is no *a priori* reason for regarding the resemblances and differences that we observe in organic forms as something different in kind from the analogous series of resemblances and differences that obtain in inanimate objects. This was clearly pointed out by Fleeming Jenkin in a very able and much-referred to article in the *North British Review* for June 1867, and his argument from the *a priori* standpoint has as much force to-day as when it was written forty-three years ago. But it has lost almost all its force through the arguments *a posteriori* supplied by morphological science. Our belief in the transmutation of animal organisation in past time is founded very largely upon our minute and intimate knowledge of the manifold relations of structural form that obtain among adult animals; on our precise knowledge of the steps by which these adult relations are established during the development of different kinds of animals; on our constantly increasing knowledge of the succession of animal forms in past time; and, generally, on the conviction that all the diverse forms of tissues, organs, and entire animals are but the expression of an infinite number of variations of a single theme, that theme being cell-division, multiplication, and differentiation. This conviction grew but slowly in men's minds. It was opposed to the cherished beliefs of centuries, and morphology rendered a necessary service when it spent all those years which have been described as 'years in the wilderness' in accumulating such a mass of circumstantial evidence in favour of an evolutionary explanation of the order of animate nature as to place the doctrine of descent with modification on a secure foundation of fact. I do not believe that this foundation could have been so securely laid in any other way, and I hold that zoologists were actuated by a sound instinct in working so largely on morphological lines for forty years after Darwin wrote. For there was a large mass of fact and theory to be remodelled and brought into harmony with the new

ideas, and a still larger vein of undiscovered fact to explore. The matter was difficult and the pace could not be forced. Morphology, therefore, deserves the credit of having done well in the past: the question remains, What can it do in the future?

It is evident, I think, that it cannot do much in the way of adding new truths and general principles to zoological science, nor even much more that is useful in the verification of established principles, without enlarging its scope and methods. Hitherto—or, at any rate, until very recently—it has accepted certain guiding principles on faith, and, without inquiring too closely into their validity, has occupied itself with showing that, on the assumption that these principles are true, the phenomena of animal structure, development, and succession receive a reasonable explanation.

We have seen that the fundamental principles relied upon during the last fifty years have been inheritance and variation. In every inference drawn from the comparison of one kind of animal structure with another, the morphologist founds himself on the assumption that different degrees of similitude correspond more or less closely to degrees of blood-relationship, and to-day there are probably few persons who doubt that this assumption is valid. But we must not forget that, before the publication of the 'Origin of Species,' it was rejected by the most influential zoologists as an idle speculation, and that it is imperilled by Mendelian experiments showing that characters may be split up and reunited in different combinations in the course of a few generations. We do not doubt the importance of the principle of inheritance, but we are not quite so sure as we were that close resemblances are due to close kinship and remoter resemblances to remoter kinship.

The principle of variation asserts that like does not beget exactly like, but something more or less different. For a long time morphologists did not inquire too closely into the question how these differences arose. They simply accepted it as a fact that they occur, and that they are of sufficient frequency and magnitude, and that a sufficient proportion of them lead in such directions that natural selection can take advantage of them. Difficulties and objections were raised, but morphology on the whole took little heed of them. Remaining steadfast in its adherence to the principles laid down by Darwin, it contented itself with piling up circumstantial evidence, and met objection and criticism with an ingenious apologetic. In brief, its labours have consisted in bringing fresh instances, and especially such instances as seemed uncomfortable, under the rules, and in perfecting a system of classification in illustration of the rules. It is obvious, however, that, although this kind of study is both useful and indispensable at a certain stage of scientific progress, it does not help us to form new rules, and fails altogether if the old rules are seriously called into question.

As a matter of fact, admitting that the old rules are valid, it has become increasingly evident that they are not sufficient. Until a few years ago morphologists were open to the reproach that, while they studied form in all its variety and detail, they occupied themselves too little—if, indeed, they could be said to occupy themselves at all—with the question of how form is produced, and how, when certain forms are established, they are caused to undergo change and give rise to fresh forms. As Klebs has pointed out, the forms of animals and plants were regarded as the expression of their inscrutable inner nature, and the stages passed through in the development of the individual were represented as the outcome of purely internal and hidden laws. This defect seems to have been more distinctly realised by botanical than by zoological morphologists, for Hofmeister, as long ago as 1868, wrote that the most pressing and immediate aim of the investigator was to discover to what extent external forces acting on the organism are of importance in determining its form.

If morphology was to be anything more than a descriptive science, if it was to progress any further in the discovery of the relations of cause and effect, it was clear that it must alter its methods and follow the course indicated by Hofmeister. And I submit that an inquiry into the causes which produce alteration of form is as much the province of, and is as fitly called, morphology as, let us say, a discussion of the significance of the patterns of the molar teeth of mammals or a disputation about the origin of the coelomic cavities of vertebrated and invertebrated animals.

There remains, therefore, a large field for morphology to explore. Exploration

has begun from several sides, and in some quarters has made substantial progress. It will be of interest to consider how much progress has been made along certain lines of research—we cannot now follow all the lines—and to forecast, if possible, the direction that this pioneer work will give to the morphology of the future.

I am not aware that morphologists have, until quite recently, had any very clear concept of what may be expected to underlie form and structure. Dealing, as they have dealt, almost exclusively with things that can be seen or rendered visible by the microscope, they have acquired the habit of thinking of the organism as made up of organs, the organs of tissues, the tissues of cells, and the cells as made up—of what? Of vital units of a lower order, as several very distinguished biologists would have us believe; of physiological units, of micellæ, of determinants and biophors, or of pangenes; all of them essentially morphological conceptions; the products of imagination projected beyond the confines of the visible, yet always restrained by having only one source of experience—namely, the visible. One may give unstinted admiration to the brilliancy, and even set a high value on the usefulness, of these attempts to give formal representations of the genesis of organic structure, and yet recognise that their chief utility has been to make us realise more clearly the problems that have yet to be solved.

Stripped of all the verbiage that has accumulated about them, the simple questions that lie immediately before us are: What are the causes which produce changes in the forms of animals and plants? Are they purely internal, and, if so, are their laws discoverable? Or are they partly or wholly external, and, if so, how far can we find relations of cause and effect between ascertained chemical and physical phenomena and the structural responses of living beings?

As an attempt to answer the last of these questions, we have the recent researches of the experimental morphologists and embryologists directed towards the very aim that Hofmeister proposed. Originally founded by Roux, the school of experimental embryology has outgrown its infancy and has developed into a vigorous youth. It has produced some very remarkable results, which cannot fail to exercise a lasting influence on the course of zoological studies. We have learnt from it a number of positive facts, from which we may draw very important conclusions, subversive of some of the most cherished ideas of whilom morphologists. It has been proved by experiment that very small changes in the chemical and physical environment may and do produce specific form-changes in developing organisms, and in such experiments the consequence follows so regularly on the antecedent that we cannot doubt that we have true relations of cause and effect. It is not the least interesting outcome of these experiments that, as Loeb has remarked, it is as yet impossible to connect in a rational way the effects produced with the causes which produced them, and it is also impossible to define in a simple way the character of the change so produced. For example, there is no obvious connection between the minute quantity of sulphates present in sea-water and the number and position of the characteristic calcareous spicules in the larva of a sea-urchin. Yet Herbst has shown that if the eggs of sea-urchins are reared in sea-water deprived of the needful sulphates (normally '26 per cent. magnesium sulphate and '1 per cent. calcium sulphate), the number and relative positions of these spicules are altered, and, in addition, changes are produced in other organs, such as the gut and the ciliated bands. Again, there is no obvious connection between the presence of a small excess of magnesium chloride in sea-water and the development of the paired optic vesicles. Yet Stockard, by adding magnesium chloride to sea-water in the proportion of 6 grams of the former to 100 c.c. of the latter, has produced specific effects on the eyes of developing embryos of the minnow *Fundulus heteroclitus*: the optic vesicles, instead of being formed as a widely separated pair, were caused to approach the median line, and in about 50 per cent. of the embryos experimented upon the changes were so profound as to give rise to cyclopean monsters. Many other instances might be cited of definite effects of physical and chemical agencies on particular organs, and we are now forced to admit that inherited tendencies may be completely overcome by a minimal change in the environment. The nature of the organism, therefore, is not all important, since it yields readily to influences which at one time we should have thought inadequate to produce perceptible changes in it.

It is open to anyone to argue that, interesting as experiments of this kind may be, they throw no light on the origin of permanent—that is to say, inheritable—modifications of structure. It has for a long time been a matter of common

knowledge that individual plants and animals react to their environment, but the modifications induced by these reactions are somatic; the germ-plasm is not affected, therefore the changes are not inherited, and no permanent effect is produced in the characters of the race or species. It is true that no evidence has yet been produced to show that form-changes as profound as those that I have mentioned are transmitted to the offspring. So far the experimenters have not been able to rear the modified organisms beyond the larval stages, and so there are no offspring to show whether cyclopean eyes or modified forms of spicules are inherited or not. Indeed, it is possible that the balance of organisation of animals thus modified has been upset to such an extent that they are incapable of growing into adults and reproducing their kind.

But evidence is beginning to accumulate which shows that external conditions may produce changes in the germ-cells as well as in the soma, and that such changes may be specific and of the same kind as similarly produced somatic changes. Further, there is evidence that such germinal changes are inherited—and, indeed, we should expect them to be, because they are germinal.

The evidence on this subject is as yet meagre, but it is of good quality and comes from more than one source.

There are the well-known experiments of Weismann, Standfuss, Merrifield, and E. Fischer on the modification of the colour patterns on the wings of various Lepidoptera.

In the more northern forms of the fire-butterfly, *Chrysophanus (Polyommatus) phlaeas*, the upper surfaces of the wings are of a bright red-gold or copper colour with a narrow black margin, but in southern Europe the black tends to extend over the whole surface of the wing and may nearly obliterate the red-gold colour. By exposing pupæ of caterpillars collected at Naples to a temperature of 10° C. Weismann obtained butterflies more golden than the Neapolitan, but blacker than the ordinary German race, and conversely, by exposing pupæ of the German variety to a temperature of about 38° C., butterflies were obtained blacker than the German, but not so black as the Neapolitan variety. Similar deviations from the normal standard have been obtained by like means in various species of *Vanessa* by Standfuss and Merrifield. Standfuss, working with the small tortoise-shell butterfly (*Vanessa urticae*), produced colour aberrations by subjecting the pupæ to cold, and found that some specimens reared under normal conditions from the eggs produced by the aberrant forms exhibited the same aberrations, but in a lesser degree. Weismann obtained similar results with the same species. E. Fischer obtained parallel results with *Arctia caja*, a brightly coloured diurnal moth of the family Bombycidae. Pupæ of this moth were exposed to a temperature of 8° C., and some of the butterflies that emerged were very dark-coloured aberrant forms. A pair of these dark aberrants were mated, and the female produced eggs, and from these larvæ and pupæ were reared at a normal temperature. The progeny was for the most part normal, but some few individuals exhibited the dark colour of the parents, though in a less degree. The simple conclusions to be drawn from the results of these experiments is that a proportion of the germ-cells of the animals experimented upon were affected by the abnormal temperatures, and that the reaction of the germ-cells was of the same kind as the reaction of the somatic cells and produced similar results. As everybody knows, Weismann, while admitting that the germ-cells were affected, would not admit the simple explanation, but gave another complicated and, in my opinion, wholly unsupported explanation of the phenomena.

In any case this series of experiments was on too small a scale, and the separate experiments were not sufficiently carefully planned to exclude the possibility of error. But no objection of this kind can be urged against the careful and prolonged studies of Tower on the evolution of chrysomelid beetles of the genus *Leptinotarsa*—better known, perhaps, by the name *Doryphora*—is the potato-beetle, which has spread from a centre in North Mexico southwards into the isthmus of Panama and northwards over a great part of the United States. It is divisible into a large number of species, some of which are dominant and widely ranging; others are restricted to very small localities. The specific characters relied upon are chiefly referable to the colouration and colour patterns of the epicranium, pronotum, elytra, and underside of the abdominal segments. In some species the specific markings are very constant, in others, particularly in the common and wide-ranging *L. decemlineata*, they vary to an extreme degree.

As the potato-beetle is easily reared and maintained in captivity, and produces two broods every year, it is a particularly favourable subject for experimental investigation. Tower's experiments have extended over a period of eleven years, and he has made a thorough study of the geographical distribution, dispersal, habits, and natural history of the genus. The whole work appears to have been carried out with the most scrupulous regard to scientific accuracy, and the author is unusually cautious in drawing conclusions and chary of offering hypothetical explanations of his results. I have been greatly impressed by the large scale on which the experiments have been conducted, by the methods used, by the care taken to verify every result obtained, and by the great theoretical importance of Tower's conclusions. I can do no more now than allude to some of the most remarkable of them.

After showing that there are good grounds for believing that colour production in insects is dependent on the action of a group of closely related enzymes, of which chitinase, the agent which produces hardening of chitin, is the most important, Tower demonstrates by a series of well-planned experiments that colours are directly modified by the action of external agencies—viz., temperature, humidity, food, altitude, and light. Food chiefly affects the subhypodermal colours of the larvæ, and does not enter much into account; the most important agents affecting the adult colouration being temperature and humidity. A *slight* increase or a *slight* decrease of temperature or humidity was found to stimulate the action of the colour-producing enzymes, giving a tendency to melanism; but a large increase or decrease of temperature or humidity was found to inhibit the action of the enzymes, producing a strong tendency to albinism.

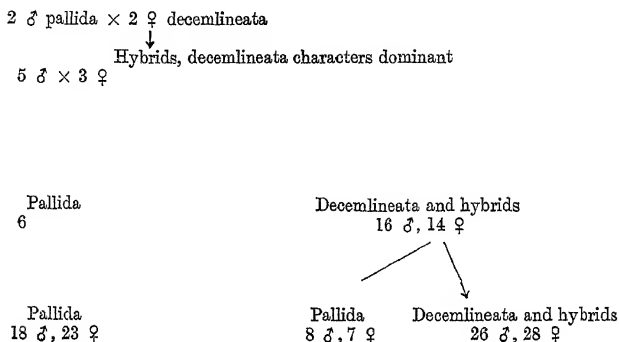
A set of experiments was undertaken to test the question whether colouration changes induced by changed environmental conditions were inherited, increased, or dropped in successive generations. These experiments, carried on for ten lineal generations, showed that the changed conditions immediately produced their maximum effect; that they were purely somatic and were not inherited, the progeny of individuals which had been exposed to changed conditions through several generations promptly reverting when returned to normal conditions of environment. So far the results are confirmatory of the well-established proposition that induced somatic changes are not inheritable.

But it was found necessary to remove the individuals experimented upon from the influence of changed conditions during the periods of growth and maturation of the germ-cells. Potato-beetles emerge from the pupa or from hibernation with the germ-cells in an undeveloped condition, and the ova do not all undergo their development at once, but are matured in batches. The first batch matures during the first few days following emergence, then follows an interval of from four to ten days, after which the next batch of eggs is matured, and so on. This fact made it possible to test the effect of altered conditions on the maturing germ-cells by subjecting its imagoes to experimental conditions during the development of some of the batches of ova and to normal conditions during the development of other batches.

In one of the experiments four male and four female individuals of *T. decemlineata* were subjected to very hot and dry conditions, accompanied by low atmospheric pressure, during the development and fertilisation of the first three batches of eggs. Such conditions had been found productive of albinic deviations in previous experiments. As soon as the eggs were laid they were removed to normal conditions, and the larvæ and pupæ reared from them were kept in normal conditions. Ninety-eight adult beetles were reared from these batches of eggs, of which eighty-two exhibited the characters of an albinic variety found in nature and described as a species under the name *pallida*; two exhibited the characters of another albinic species named *immaculothorax*, and fourteen were unmodified *decemlineatas*. This gave a clear indication that the altered conditions had produced modifications in the germ-cells which were expressed by colour changes in the adult individuals reared from them. To prove that the deviations were not inherent in the germ-plasm of the parents, the latter were kept under normal conditions during the periods of development and fertilisation of the last two batches of eggs; the larvæ and pupæ reared from these eggs were similarly subjected to normal conditions, and gave rise to sixty-one unmodified *decemlineatas*, which, when bred together, came true to type for three generations. The *decemlineata* forms produced under experimental conditions also came true to type when

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bred together. Of the *pallida* forms produced by experimental conditions all but two males were killed by a bacterial disease. These two were crossed with normal *decemlineata* females, and the result was a typical Mendelian segregation, as shown by the following table :—



This is a much more detailed experiment than those of Standfuss, Merrifield, and Fisher, and it shows that the changes produced by the action of altered conditions on the maturing germ-cells were definite and discontinuous, and therefore of the nature of mutations in De Vries' sense.

In another experiment Tower reared three generations of *decemlineata* to test the purity of his stock. He found that they showed no tendency to produce extreme variations under normal conditions. From this pure stock seven males and seven females were chosen and subjected during the maturation periods of the first two batches of ova to hot and dry conditions. Four hundred and nine eggs were laid, from which sixty-nine adults were reared, constituted as follows :

Twenty (12 ♂, 8 ♀)	apparently normal <i>decemlineata</i> .
Twenty-three (10 ♂, 13 ♀)	<i>pallida</i> .
Five (2 ♂, 3 ♀)	<i>immaculothorax</i> .
Sixteen (9 ♂, 7 ♀)	<i>albida</i> .

These constituted lot A.

The same seven pairs of parents subjected during the second half of the reproductive period to normal conditions gave 840 eggs, from which were reared 123 adults, all *decemlineatas*. These constituted lot B. The *decemlineatas* of lot A and lot B were reared side by side under normal and exactly similar conditions. The results were striking. From lot B normal progeny were reared up to the tenth generation, and, as usual in the genus, two generations were produced in each year. The *decemlineatas* of lot A segregated into two lots in the second generation. A¹ were normal in all respects, but A², while retaining the normal appearance of *decemlineata*, went through five generations in a year, and this for three successive years, thus exhibiting a remarkable physiological modification, and one without parallel in nature, for no species of the genus *Leptinotarsa* are known which produce more than two generations in the year. This experiment is a sufficient refutation of Weismann's argument that the inheritance of induced modifications in *Vanessa urticae* is only apparent, the phenomena observed being due to the inheritance of two kinds of determinants—one from dark-coloured forms which are phyletically the oldest, and the other from more gaily coloured forms derived from the darker forms. There is no evidence whatever that there was ever a species or variety of potato-beetle that produced more than two, or at the most, and then as an exception, three broods in a year.

The modified albinic forms in this last experiment of Tower's were weakly ; they were bred through two or three generations and came true to type, but then died out. No hybridisation experiments were made with them, but in other

similar experiments, which I have not time to mention in detail, modified forms produced by the action of changed conditions gave typical Mendelian characters when crossed with unmodified *decemlineatas*, thus proving that the induced characters were constant and heritable according to the regular laws.

I have thought it worth while to relate these experiments at some length, because they seem to me to be very important, and because they do not appear to have attracted the attention in this country that they deserve.

They are confirmed to a very large extent by the experiments of Professor Klebs on plants, the results of which were published this summer in the Croonian Lecture on 'Alterations of the development and forms of plants as a result of environment.' As I have only a short abstract of the Croonian Lecture to refer to, I cannot say much on this subject for fear of misrepresenting the author; but, as far as I can judge, his results are quite consistent with those of Tower. *Sempervivum funckii* and *S. acuminatum* were subjected to altered conditions of light and nutrition, with the result that striking variations, such as the transformation of sepals into petals, of petals into stamens, of stamens into petals and into carpels, were produced. Experiments were made on *Sempervivum acuminatum*, with the view of answering the question whether such alterations of flowers can be transmitted. The answer was in the affirmative. The seeds of flowers artificially altered and self-fertilised gave rise to twenty-one seedlings, among which four showed surprising deviations of floral structure. In two of these seedlings all the flowers were greatly altered, and presented some of the modifications of the mother plant, especially the transformation of stamens into petals. These experiments are still in progress, and it would perhaps be premature to lay too much stress upon them if it were not for the fact that they are so completely confirmatory of the results obtained by similar methods in the animal kingdom.

I submit to you that evidence is forthcoming that external conditions may give rise to inheritable alterations of structure. Not, however, as was once supposed, by producing specific changes in the parental soma, which changes were reflected, so to speak, upon the germ-cells. The new evidence confirms the distinctions drawn by Weismann between somatic and germinal variations. It shows that the former are not inherited, while the latter are; but it indicates that the germ may be caused to vary by the action of external conditions in such a manner as to produce specific changes in the progeny resulting from it. It is no more possible at the present time to connect rationally the action of external conditions on the germ-cells with the specific results produced in the progeny than it is possible to connect cause with effect in the experiments of Herbst and Stockard; but, when we compare these two kinds of experiments, we are no longer able to argue that it is inconceivable that such and such conditions acting on the germ-plasm can produce such and such effects in the next generation of adults. We must accept the evidence that things which appeared inconceivable do in fact happen, and in accepting this we remove a great obstacle from the path of our inquiries, and gain a distinct step in our attempts to discover the laws which determine the production of organic form and structure.

But such experiments as those which I have mentioned only deal with one aspect of the problem. They tell us about external conditions and the effects that they are observed to produce upon the organism. They give us no definite information about the internal changes which, taken together, constitute the response of the organism to external stimuli. As Darwin wrote, there are two factors to be taken into account—the nature of the conditions and the nature of the organism—and the latter is much the more important of the two. More important because the reactions of animals and plants are manifold; but, on the whole, the changes in the conditions are few and small in amount. Morphology has not succeeded in giving us any positive knowledge of the nature of the organism, and in this matter we must turn for guidance to the physiologists, and ask of them how far recent researches have resulted in the discovery of factors competent to account for change of structure. Perhaps the first step in this inquiry is to ask whether there is any evidence of internal chemical changes analogous in their operation to the external physical and chemical changes which we have been dealing with.

There is a great deal of evidence, but it is extremely difficult to bring it to a focus and to show its relevancy to the particular problems that perplex the zoolo-

gist. Moreover, the evidence is of so many different kinds, and each kind is so technical and complex, that it would be absurd to attempt to deal with it at the end of an address that has already been drawn out to sufficient length. But perhaps I may be allowed to allude to one or two generalisations which appear to me to be most suggestive.

We shall all agree that, at the bottom, production and change of form is due to increase or diminution of the activities of groups of cells, and we are aware that in the higher animals change of structure is not altogether a local affair, but carries with it certain consequences in the nature of correlated changes in other parts of the body. If we are to make any progress in the study of morphogeny, we ought to have as exact ideas as possible as to what we mean when we speak of the activities of cells and of correlation. On these subjects physiology supplies us with ideas much more exact than those derived from morphology.

It is, perhaps, too sweeping a generalisation to assert that the life of any given animal is the expression of the sum of the activities of the enzymes contained in it, but it seems well established that the activities of cells are, if not wholly, at all events largely, the result of the actions of the various kinds of enzymes held in combination by their living protoplasm. These enzymes are highly susceptible to the influence of physical and chemical media, and it is because of this susceptibility that the organism responds to changes in the environment, as is clearly illustrated in a particular case by Tower's experiments on the production of colour changes in potato-beetles. Bayliss and Starling have shown that in lower animals, protozoa and sponges, in which no nervous system has been developed, the response of the organism to the environment is effected by purely chemical means. In protozoa, because of their small size, the question of coadaptation of function hardly comes into question; but in sponges, many of which are of large size, the mechanism of coadaptation must also be almost exclusively chemical. Thus we learn that the simplest and, by inference, the phyletically oldest mechanism of reaction and co-ordination is a chemical mechanism. In higher animals the necessity for rapid reaction to external and internal stimuli has led to the development of a central and peripheral nervous system, and as we ascend the scale of organisation, this assumes a greater and greater importance as a co-ordinating bond between the various organs and tissues of the body. But the more primitive chemical bond persists, and is scarcely diminished in importance, but only overshadowed by the more easily recognisable reactions due to the working of the nervous system. In higher animals we may recognise special chemical means whereby chemical coadaptations are established and maintained at a normal level, or under certain circumstances altered. These are the internal secretions produced by sundry organs, whether by typical secretory glands (in which case the internal secretion is something additional and different from the external secretion), or by the so-called ductless glands, such as the thyroid, the thymus, the adrenal bodies, or by organs which cannot strictly be called glands—namely, the ovaries and testes. All these produce chemical substances which, passing into the blood or lymph, are distributed through the system, and have the peculiar property of regulating or exciting the specific functions of other organs. Not, however, of all the organs, for the different internal secretions are more or less limited and local in their effects: one affecting the activity of this and another the activity of that kind of tissue or organ. Starling proposed the name hormones for the internal secretions because of their excitatory properties (*ὁρμῶν*, to stir up, to excite).

Hormones have been studied chiefly from the point of view of their stimulating effect on the metabolism of various organs. From the morphologist's point of view, interest chiefly attaches to the possibility of their regulating and promoting the production of form. It might be expected that they should be efficient agents in regulating form, for, if changes in structure are the result of the activities of groups of cells, and the activities of cells are the results of the activities of the enzymes which they contain, and if the activities of the enzymes are regulated by the hormones, it follows that the last-named must be the ultimate agents in the production of form. It is difficult to obtain distinct evidence of this agency, but in some cases at least the evidence is sufficiently clear. I will confine myself to the effects of the hormones produced by the testes and ovaries. These have been proved to be intimately connected with the development of

secondary sexual characters—such, for instance, as the characteristic shape and size of the horns of the bull; the comb, wattles, spurs, plumage colour, and spurs in poultry; the swelling on the index finger of the male frog; the shape and size of the abdominal segments of crabs. These are essentially morphological characters, the results of increased local activity of cell-growth and differentiation. As they are attributable to the stimulating effect of the hormone produced by the male organ in each species, they afford at least one good instance of the production of a specific change of form as the result of an internal chemical stimulus. We get here a hint as to the nature of the chemical mechanism which excites and correlates form and function in higher organisms; and, from what has just been said, we perceive that this is the most primitive of all the animal mechanisms. I submit that this is a step towards forming a clear and concrete idea of the inner nature of the organism. There is one point, and that a very important one, upon which we are by no means clear. We do not know how far the hormones themselves are liable to change, whether by the action of external conditions or by the reciprocal action of the activities of the organs to which they are related. It is at least conceivable that agencies which produce chemical disturbances in the circulating fluids may alter the chemical constitution of the hormones, and thus produce far-reaching effects. The pathology of the thyroid gland gives some ground for belief that such changes may be produced by the action of external conditions. But, however this may be, the line of reasoning that we have followed raises the expectation that a chemical bond must exist between the functionally active organs of the body and the germ-cells. For if, in the absence of a specialised nervous system, the only possible regulating and coadapting mechanism is a chemical mechanism, and if the specific activities of a cell are dependent on the enzymes which it holds in combination, the germ-cells of any given animal must be the depository of a stock of enzymes sufficient to insure the due succession of all its developmental stages as well as of its adult structure and functions. And as the number of blastomeres increases, and the need for co-ordination of form and function arises, before ever the rudiments of a nervous system are differentiated, it is necessary to assume that there is also a stock of appropriate hormones to supply the chemical nexus between the different parts of the embryo. The only alternative is to suppose that they are synthesised as required in the course of development. There are grave objections to this supposition. All the evidence at our disposal goes to show that the potentialities of germ-cells are determined at the close of the maturation divisions. Following the physiological line of argument, it must be allowed that in this connection 'potentiality' can mean nothing else than chemical constitution. If we admit this, we admit the validity of the theory advanced by more than one physiologist that heritable 'characters' or 'tendencies' must be identified with the enzymes carried in the germ-cells. If this be a true representation of the facts, and if the most fundamental and primitive bond between one part of an organism and another is a chemical bond, it can hardly be the case that germ-cells—which, *inter alia*, are the most primitive, in the sense of being the least differentiated, cells in the body—should be the only cells which are exempt from the chemical influences which go to make up the co-ordinate life of the organism. It would seem, therefore, that there is some theoretical justification for the inheritance of induced modifications, provided that these are of such a kind as to react chemically on the enzymes contained in the germ-cells.

One further idea that suggests itself to me and I have done. Is it possible that different kinds of enzymes exercise an inhibiting influence on one another; that germ-cells are 'undifferentiated' because they contain a large number of enzymes, none of which can show their activities in the presence of others, and that what we call 'differentiation' consists in the segregation of the different kinds into separate cells, or perhaps, prior to cell-formation, into different parts of the fertilised ovum, giving rise to the phenomenon known to us as prelocalisation? The idea is purely speculative; but, if it could be shown to have any warrant, it would go far to assist us in getting an understanding of the laws of the production of form.

I have been wandering in territories outside my own province, and I shall certainly be told that I have lost my way. But my thesis has been that morphology, if it is to make useful progress, must come out of its reserves and explore

new ground. To explore is to tread unknown paths, and one is likely to lose one's way in the unknown. To stay at home in the environment of familiar ideas is no doubt a safe course, but it does not make for advancement. Morphology, I believe, has as great a future before it as it has a past behind it, but it can only realise that future by leaving its old home, with all its comfortable furniture of well-worn rules and methods, and embarking on a journey, the first stages of which will certainly be uncomfortable and the end is far to seek.

British Association for the Advancement of Science.

SHEFFIELD, 1910.

ADDRESS TO THE GEOGRAPHICAL SECTION

BY

A. J. HERBERTSON, M.A., PH.D., Professor of Geography in the
University of Oxford,

PRESIDENT OF THE SECTION.

Geography and some of its more Pressing Needs.

At the close of a reign which has practically coincided with the first decade of a new century, it is natural to look back and summarise the progress of geography during the decade. At the beginning of a new reign it is equally natural to consider the future. Our new sovereign is one of the most travelled of men. No monarch knows the World as he knows it; no monarch has ruled over a larger Empire or seen more of his dominions. His advice has been to wake up, to consider and to act. It will be in consonance with this advice if I pay more attention to the geography of the future than to that of the past, and say more about its applications than about its origins.

Yet I do so with some reluctance, for the last decade has been one of the most active and interesting in the history of our science. The measurement of new and the remeasurement of old arcs will give us better data for determining the size and shape of the Earth. Surveys of all kinds, from the simple route sketches of the traveller to the elaborate cadastral surveys of some of the more populous and settled regions have so extended our knowledge of the surface features of the Earth that a map on the scale of 1,000,000 is not merely planned, but actually partly executed. Such surveys and such maps are the indispensable basis of our science, and I might say much about the need for accurate topographical surveys. This, however, has been done very fully by some of my predecessors in this chair in recent years.

The progress of oceanography has also been great. The soundings of our own and other Admiralties, of scientific oceanographical expeditions, and those made for the purpose of laying cables, have given us much more detailed knowledge of the irregularities of the ocean floor. An international map of oceanic contours due to the inspiration and munificence of the Prince of Oceanographers and of Monaco has been issued during the decade, and so much new material has accumulated that it is now being revised. A comparison of the old and new editions of Krümmel's *Oceanographie* shows us the immense advances in this subject.

Great progress has been made on the geographical side of meteorology and climate. The importance of this knowledge for tropical agriculture and hygiene has led to an increase of meteorological stations all over the hot belt—the results of which will be of immense value to the geographer. Mr. Bartho-

lownew's 'Atlas of Meteorology' appeared at the beginning, and Sir John Eliot's 'Meteorological Atlas of India' at the end of the decade. Dr. Hann's 'Lehrbuch' and the new edition of his 'Climatology,' Messrs. Hildebrandsson and Teisserenc de Bort's great work, 'The Study of the Upper Atmosphere,' are among the landmarks of progress. The record is marred only by the closing of Ben Nevis Observatory. A comparison of the present number and distribution of meteorological stations with those given in Bartholomew's Atlas would show how great the extension of this work is.

I have not time to recapitulate the innumerable studies of geographical value issued by many meteorological services, observatories and observers—public and private—but I may call attention to the improved weather maps and to the excellent pilot charts of the North Atlantic and of the Indian Ocean published monthly by our Meteorological Office.

Lake studies have also been a feature of this decade, and none are so complete or so valuable as the Scottish Lakes Survey—a work of national importance, undertaken by private enthusiasm and generosity. We have to congratulate Sir John Murray and Mr. Pullar on the completion of a great work.

In Geology I might note that we now possess a map of Europe on a scale of 1:1,500,000 prepared by international co-operation and also one of North America on a smaller scale. The thanks and congratulations of all geographers are due to Professor Suess on the conclusion of his classical study of the Face of the Earth, the first comprehensive study of the main divisions and characteristics of its skeleton. English readers are indebted to Professor and Miss Sollas for the brilliant English translation which they have prepared.

A new movement, inspired mainly by Professor Flahault in France, Professor Geddes in this country, Professors Engler, Drude, and Schimper in Germany has arisen among botanists, and at last we have some modern botanical geography which is really valuable to the geographer. I wish we could report similar progress in zoological geography, but that, I trust, will come in the next decade.

I pass over the various expensive arbitrations and commissions to settle boundary disputes which have in many cases been due to geographical ignorance, also the important and fascinating problems of the growth of our knowledge of the distribution of economic products and powers existing and potential, and the new geographical problems for statesmen due to the industrial revolution in Japan and China.

It is quite impossible to deal with the exploration of the decade. Even in the past two years we have had Peary and Shackleton, Stein and Hedin, the Duke of the Abruzzi, and a host of others returning to tell us of unknown or little known parts of the globe. We hope to hear some of the results of latest investigations from Dr. Charcot.

We wish success to Scott and his companions, to Bruce, Amundsen, Filchner, and others, British, American, German, or whatever nationality, who go to the South or North Polar ice worlds, to Longstaff, Bruce, and others exploring the Himalayan regions, and to other geographical expeditions too numerous to mention.

One word of caution may, perhaps, be permitted. There is a tendency on the part of the public to confuse geographical exploration and sport. The newspaper reporter naturally lays stress on the unusual in any expedition, the accidental rather than the essential, and those of us who have to examine the work of expeditions know how some have been unduly boomed because of some adventurous element, while others have not received adequate popular recognition because all went well. The fact that all went well is in itself a proof of competent organisation. There is no excuse for us in this section if we fall into the journalist's mistake, and we shall certainly be acting against the interests of both our science and our section if we do so.

It was not my intention in this address to raise the question of what is Geography, but various circumstances make it desirable to say a few words upon it. We are all the victims of the geographical teaching of our youth, and it is easy to understand how those who have retained unchanged the conceptions of geography they gained at school many years ago cavil at the recognition of geography as a branch of science. Moreover the geography of the schools still

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colours the conceptions of some geographers who have nevertheless done much to make school geography scientific and educational. Many definitions of geography are consequently too much limited by the arbitrary but traditional division of school subjects. In schools tradition and practical convenience have on the whole rightly determined the scope of the different subjects. Geography in schools is best defined as the study of the Earth as the home of Man; its limits should not be too closely scrutinised, and it should be used freely as a co-ordinating subject.

The present division into sections of the British Association is also largely a matter of practical convenience, but we are told that the present illogical arrangement of sections distresses some minds. No doubt there are some curious anomalies. The most glaring, perhaps, is that of combining mathematics with physics—as if mathematical methods were used in no other subjects.

There is a universal tendency to sub-division and an ever-increasing specialism, but there is also an ever-growing interdependence of different parts of science which the British Association is unquestionably bound to take into account. At present this is chiefly done by joint meetings of sections, a wise course, of which this section has been one of the chief promoters. It is possible that some more systematic grouping of sections might be well advised, but such a reform should be systematic and not piecemeal. It is one which raises the whole question of the classification of knowledge. This is so vast a problem and one on which such divergent opinions are held that I must apologise for venturing to put forward some tentative suggestions.

It might be found desirable to take as primary divisions the Mathematical, Physical, Biological, Anthropological, and Geographical groups. Statistics might be regarded as a subdivision of Mathematics or as a field common to Mathematics and any of the other groups. In the second might be the sub-divisions Physics and Chemistry. Each would devote a certain proportion of time to its applied aspects—or there might be sub-sections on Physics, which would include Engineering and Applied Chemistry. In the Biological Group there would be Botany, Zoology, in both cases including Palæontology and Embryology, and Applied Biology, which would be dealt with in one or other of the ways I have suggested, and would include Agriculture, Fisheries, etc. (Medicine we leave out at present.) In the Psychological group there would be a new section on Psychology, with the Education section as the practical appendage. Mathematical applications would be considered in each of the other sections which use mathematical notations. In the Anthropological group there would be the present Anthropology and Theoretical Economics with Applied Economics and Administration. In the Geographical group there would be Geography and Geology, the practical applications of Geography being considered in joint meetings, or sub-sections—for instance, Geography and Physics for questions of Atmospheric and Oceanic Circulation, Geography and Economics for questions of Transportation, etc.

So much, then, for the classification of Geography with reference to the other sciences. I should like to say a few words about geographical classification and geographical terminology.

In the scheme of the Universe it is possible to consider the Earth as a unit, with its own constitution and history. It has an individuality of its own, though for the astronomer it is only one example of a particular type of heavenly bodies. As geographers we take it as our unit individual in the same way that an anatomist takes a man. We see that it is composed of different parts and we try to discover what these are, of what they are composed, what their function is, what has been their history.

The fundamental division is into land, water, and air. Each has its forms and its movements. The forms are more obvious and persistent in the land. They are least so in the atmosphere, though forms exist—some of which are at times made visible by clouds, and many can be clearly discerned on isobaric charts. The land is the temporarily permanent; the water and atmosphere the persistently mobile; the latter more so than the former. The stable forms of the land help to control the distribution and movements of the waters and to a less extent those of the atmosphere. How great the influence of the distribution of land and water is on the atmosphere may be seen in the monsoon region of Eastern Asia.

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We can analyse and classify the sub-divisions of the land, the water, and the atmosphere. Each has given rise to a special branch of study.

Geomorphology deals with the forms of the land and their shaping—Geomorphology, Oceanography, and Climatology. Three things have to be kept clearly in view: (i) The structure, including the composition, of the more permanent substance of the form; (ii) The forces which are modifying it; and (iii) The phase in the cycle of forms characteristic of such structure acted on by such forms. We may say that any form is a function of structure, process, and time. The matter is even more complicated, for we have instances, *v.g.* in antecedent drainage systems, of the conditions of a previous cycle affecting a subsequent one—a kind of heredity of forms which cannot be neglected.

The geomorphologist is seeking for a genetic classification of forms, and in the works of Davis, Penck, Richthofen, and Supan and their pupils are being accumulated the materials for a more complete and systematic classification of forms. As you all know, the question of terms for the manifold land-forms is a difficult one and apt to engender much more controversy than the analysis of the forms themselves. I have long thought that we shall be driven to some notation analogous to that of the chemists. I have not yet had time to work such a notation out in detail, but it might take the form of using different symbols for the three factors noted above—say, letters for different kinds of structure, and, say, Arabic figures for processes, and Roman figures for the stage of a cycle the form has reached.

Take a very simple set of structures and indicate each by a letter:

		Undisturbed	Faulted
Structure . . .	homogeneous	A	A'
	layered { horizontal	B	B'
	{ tilted	C	C'
	{ folded	D	D'
	mixed	E	E'

If pervious or impervious, a *p* or an *i* could be added—*e.g.* a tilted limestone with faults would be C'*p*.

Next indicate the commoner erosion processes by Arabic numerals:

Process .	surface water	1
	ice	2
	wind	3
	sea	4

One process may have followed another, *e.g.* where a long period of ice erosion has been followed by water erosion we might write 21 where these alternate annually, say 21.

The phase of the cycle might be denoted by Roman figures. A scale of V might be adopted and I, III, and V used for youthful, middle-aged and old-aged, as Professor Davis calls them; or early, middle, and late phases, as I shall prefer to term them. II and IV would denote intermediate stages.

A scarped limestone ridge in a relatively mature phase like the Cotswolds would be, if we put the process first—1 C' III. : A highland like the Southern Uplands of Scotland would be denoted by the formula 1. 21 E' III.

This is the roughest suggestion, but it shows how we could label our cases of notes, and pigeon-hole our types of forms—and prevent for the present undue quarrelling over terms. No doubt there would be many discussions about the exact phase of the cycle, for example, whether ice in addition to water has been an agent in shaping this or that form. But, after all, these discussions would be more profitable than quarrels as to which descriptive term, or place name, or local usage should be adopted to distinguish it.

In the case of climatology, there is coming to be a general consensus of opinion as to what are the chief natural divisions, and the use of figures and letters to indicate them has been followed by several authors. This should also be attempted for oceanography.

If any international agreement of symbols and colours could be come to for

such things it would be a great gain, and I hope to bring this matter before the next International Geographical Congress.

We have still to come to Geography itself. What are the units smaller than the whole Earth with which our science has to deal? When we fix our attention on parts of the Earth, and ask what is a natural unit, we are hampered by preconceptions. We recognise species, or genera, families, or races as units—but they are abstract rather than concrete units. Speaking for myself, I should say that every visible concrete natural unit on the Earth's surface consisting of more than one organic individual is a geographical unit. It is a common difficulty not to be able to see the wood for the trees; it is still more difficult to recognise that the wood consists of more than trees, that it is a complex of trees and other vegetation, fixed to a definite part of the solid earth and bathed in air.

The family, the species, and the race are abstract ideas. If we consider them as units, it is because they have a certain historical continuity. They have not an actual physical continuity as the component parts of an individual have. Concrete physical continuity is what differentiates the geographical unit. We may speak of a town or state as composed of people, but a complete conception of either must include the spacial connections which unite its parts. A town is not merely an association of individuals, nor is it simply a piece of land covered with streets and buildings; it is a combination of both.

In determining the greater geographical units, man need not be taken into account. We are too much influenced by the mobility of man, by his power to pass from one region to another, and we are apt to forget that his influence on his environment is negligible except when we are dealing with relatively small units. The geographer will not neglect man; he will merely be careful to prevent himself from being unduly influenced by the human factor in selecting his major units.

Some geographers and many geologists have suggested that land forms alone need be taken into account in determining these geographical units. Every different recognisable land form is undoubtedly a geographical unit. A great mountain system, such as that of Western North America, or a vast lowland such as that which lies to the east of the Rocky Mountains, is undoubtedly a geographical unit of great importance, but its sub-divisions are not wholly orographical. The shores of the Gulf of Mexico cannot be considered as similar geographically to those of the Arctic Ocean, even if they are morphologically homologous. I wish to lay great stress on the significance of vegetation to the geographer for the purposes of regional classification. I do not wish to employ a biological terminology nor to raise false analogies between the individual organism and the larger units of which it is a part, but I think we should do well to consider what may be called the life or movement going on in our units as well as their form. We must consider the seasonal changes of its atmospheric and of its water movements, as well as the parts of the Earth's crust which they move over and even slightly modify. For this purpose a study of climatic regions is as necessary as a study of morphological regions. The lowlands of the Arctic area are very different from those at or near the tropics. The rhythm of their life is different, and this difference is revealed in the differences of vegetation.

By vegetation I mean not the flora, the historically related elements, but the vegetable coating, the space-related elements. Vegetation in this sense is a geographical phenomenon of fundamental importance. It indicates quality—quality of atmosphere and quality of soil. It is a visible synthesis of the climatic and edaphic elements. Hence the vast lowlands of relatively uniform land features are properly divided into regions according to vegetation—tundra, pine forest, deciduous forest, warm evergreen forest, steppe, and scrub. Such differences of vegetation are full of significance even in mountainous areas.

The search after geographical unity—after general features common to recognisable divisions of the Earth's surface, the analysis of these, their classification into types, the comparisons between different examples of the types—seem to me among the first duties of a geographer. Two sets of maps are essential—topographical and vegetational—the first giving the superficial topography and as far as possible its surface irregularities, the latter indicating quality of climate and soil.

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Much has been said in recent years—more particularly from this Presidential chair—on the need for reliable topographical maps. Without such maps no others can be made. But when they are being made it would be very easy to have a general vegetational map compiled. Such maps are even more fundamental than geological maps, and they can be constructed more rapidly and cheaply. Every settled country, and more particularly every partially settled country, will find them invaluable if there is to be any intelligent and systematic utilisation of the products of the country.

The geographer's task I am assuming is to study environments, to examine the forms and qualities of the Earth's surface, and to recognise, define, and classify the different kinds of natural units into which it can be divided. For these we have not as yet even names. It may seem absurd that there should be this want of terms in a subject which is associated in the minds of most people with a superfluity of names. I have elsewhere suggested the use of the terms major natural region, natural region, district, and locality to represent different grades of geographical units, and have also attempted to map the seventy or eighty major natural regions into which the Earth's surface is divided, and to classify them into about twenty types. These tentative divisions will necessarily become more accurate as research proceeds, and the minor natural regions into which each major natural region should be divided will be definitely recognised, described, and classified. Before this can be done, however, the study of geomorphology and of plant formations must be carried far beyond the present limits.

At the opposite end of the scale, that is in the geographical study of localities, good work is beginning to be done. Dr. H. R. Mill, one of the pioneers of geography in this country and one of my most distinguished predecessors in this chair, has given us in his study of south-west Sussex an admirable example of the geographical monograph proper, which takes into account the whole of the geographical factors involved. He has employed quantitative methods as far as these could be applied, and in doing so has made a great step in advance. Quantitative determinations are at least as essential in geographical research as the consideration of the time factor.

The geomorphologist and the sociologist have also busied themselves with particular aspects of selected localities. Professor W. M. Davis, of Harvard, has published geomorphological monographs which are invaluable as models of what such work should be. In a number of cases he has passed beyond mere morphology and has called attention to the organic responses associated with each land form. Some of the monographs published under the supervision of the late Professor Ratzel, of Leipzig, bring out very clearly the relation between organic and inorganic distributions, and some of the monographs of the Le Play school incidentally do the same.

At present there is a double need. Research may take the form, in the first place, of collecting new information, or, in the second place, of working up the material which is continually being accumulated.

The first task—that of collecting new information—is no small one. In many cases it must be undertaken on a scale that can be financed only by Governments. The Ordnance and Geological Surveys of our own and other countries are examples of Government departments carrying on this work. We need more of them. We need urgently a Hydrographical Department, which would co-operate with Dr. Mill's rainfall organisation. It would be one of the tasks of this department to extend and co-ordinate the observations on river and lake discharge, which are so important from an economic or health point of view that various public bodies have had to make such investigations for the drainage areas which they control. Such research work as that done by Dr. Strahan for the Exe and Medway would be of the greatest value to such a department, which ought to prepare a map showing all existing water rights, public and private.

We shall see how serious the absence of such a department is if we consider how our water supply is limited, and how much of it is not used to the best advantage. We must know its average quantity and the extreme variations of supply. We must also know what water is already assigned to the uses of persons and corporations, and what water is still available. We shall have to differentiate between water for the personal use of man and animals, and water for

industrial purposes. The actualities and the potentialities can be ascertained and should be recorded and mapped.

In the second direction of research—that of treating from the geographical standpoint the data accumulated, whether by Government departments or by private initiative—work has as yet hardly been begun.

The topographical work of the Ordnance Survey is the basis of all geographical work in our country. The Survey has issued many excellent maps, none more so than the recently published half-inch contoured and hill-shaded maps with colours 'in layers.' Its maps are not all above criticism; for instance, few can be obtained for the whole kingdom having precisely the same symbols. It has not undertaken some of the work that should have been done by a national cartographic service—for instance, the lake survey. Nor has it yet done what the Geological Survey has done—published descriptive accounts of the facts represented on each sheet of the map. From every point of view this is a great defect; but in making these criticisms we must not forget (i) that the Treasury is not always willing to find the necessary money, and (ii) that the Ordnance Survey was primarily made for military purposes, and that the latest map it has issued has been prepared for military reasons. It has been carried out by men who were soldiers first and topographers after, and did not necessarily possess geographical interests. The ideal geographical map, with its accompanying geographical memoir, can be produced only by those who have had a geographical training. Dr. Mill, in the monograph already referred to, has shown us how to prepare systematised descriptions of the one-inch map sheets issued by the Ordnance Survey.

At Oxford we are continuing Dr. Mill's work. We require our diploma students to select some district shown on a sheet of this map for detailed study by means of map measurements, an examination of statistics and literature which throw light on the geographical conditions, and, above all, by field work in the selected district. Every year we are accumulating more of these district monographs, which ought, in their turn, to be used for compiling regional monographs dealing with the larger natural areas. In recent years excellent examples of such regional monographs have come from France and from Germany.

The preparation of such monographs would seem to fall within the province of the Ordnance Survey. If this is impossible, the American plan might be adopted. There the Geological Survey, which is also a topographical one, is glad to obtain the services of professors and lecturers who are willing to undertake work in the field during vacations. It should not be difficult to arrange similar co-operation between the Universities and the Ordnance Survey in this country. At present the Schools of Geography at Oxford and at the London School of Economics are the only University departments which have paid attention to the preparation of such monographs, but other Universities will probably fall into line. Both the Universities and the Ordnance Survey would gain by such co-operation. The chief obstacle is the expense of publication. This might reasonably be made a charge on the Ordnance Survey, on condition that each monograph published were approved by a small committee on which both the Universities and the Ordnance Survey were represented.

The information which many other Government departments are accumulating would also become much more valuable if it were discussed geographically. Much excellent geographical work is done by the Admiralty and the War Office. The Meteorological Office collects statistics of the weather conditions from a limited number of stations; but its work is supplemented by private societies which are not well enough off to discuss the observations they publish with the detail which these observations deserve. The Board of Agriculture and Fisheries has detailed statistical information as to crops and live stock for the geographer to work up. From the Board of Trade he would obtain industrial and commercial data, and from the Local Government Board vital and other demographic statistics. At present most of the information of these departments is only published in statistical tables.

Statistics are all very well, but they are usually published in a tabular form, which is the least intelligible of all. Statistics should be mapped and not merely be set out in columns of figures. Many dull Blue Books would be more interesting

and more widely used if their facts were properly mapped. I say *properly* mapped because most examples of so-called statistical maps are merely crude diagrams and are often actually misleading. It requires a knowledge of geography in addition to an understanding of statistical methods to prepare intelligible statistical maps. If Mr. Bosse's maps of the population of England and Wales in Bartholomew's Survey Atlas are compared with the ordinary ones the difference between a geographical map and a cartographic diagram will be easily appreciated.

The coming census, and to a certain extent the census of production, and probably the new land valuation, will give more valuable raw material for geographical treatment. If these are published merely in tabular form they will not be studied by any but a few experts. Give a geographer with a proper staff the task of mapping them in a truly geographical way and they will be eagerly examined even by the man in the street, who cannot fail to learn from them. The presentation of the true state of the country in a clear, graphic and intelligible form is a patriotic piece of work which the Government should undertake. It would add relatively little to the cost of the census and it would infinitely increase its value.

The double lack--the lacuna in the information and the absence of adequate geographical treatment of such material as there is--makes the task of studying the huge natural divisions which we call continents a very difficult and unsatisfactory one. For several years in Oxford we have been trying to gather together the material available for the study of the continents and to make as accurate maps as is possible for geographical purposes. We have adopted uniform scales and methods, and by using equal area projections we have obtained comparative graphic representations of the facts. We hope before the end of the year to issue maps of physical features, vegetation, and rainfall of each continent and other maps for the World. These are being measured, and I hope will yield more reliable quantitative information about the World and its continents than we possess at present.

With such quantitative information and with a fuller analysis of the major natural regions it ought to be possible to go a step further and to attempt to map the economic value of different regions at the present day. Such maps would necessarily be only approximations at first. Out of them might grow other maps prophetic of economic possibilities. Prophecy in the scientific sense is an important outcome of geographical as well as of other scientific research. The test of geographical laws as of others is the pragmatic one. Prophecy is commonly but unduly derided. Mendelyeff's period law involved prophecies which have been splendidly verified. We no longer sneer at the weather prophet. Efficient action is based on knowledge of cause and consequence, and proves that a true forecast of the various factors has been made. Is it too much to look forward to the time when the geographical prospector, the geographer who can estimate potential geographical values, will be as common as and more reliable than the mining prospector?

The day will undoubtedly come when every Government will have its geographical-statistical department dealing with its own and other countries--an information bureau for the administration corresponding to the department of special inquiries at the Board of Education. There is no geographical staff to deal geographically with economic matters or with administrative matters. Yet the recognition of and proper estimation of the geographical factor is going to be more and more important as the uttermost ends of the Earth are bound together by visible steel lines and steel vessels or invisible impulses which require no artificial path or vessel as their vehicle.

The development of geographical research along these lines in our own country could give us an Intelligence Department of the kind, which is much needed. If this were also done by other states within the Empire, an Imperial Intelligence Department would gradually develop. Thinking in continents, to borrow an apt phrase from one of my predecessors, might then become part of the necessary equipment of a statesman instead of merely an after-dinner aspiration. The country which first gives this training to its statesmen will have an immeasurable advantage in the struggle for existence.

Our universities will naturally be the places where the men fit to constitute such an Intelligence Department will be trained. It is encouraging, therefore, to

see that they are taking up a new attitude towards geography, and that the Civil Service Commissioners, by making it a subject for the highest Civil Service examinations, are doing much to strengthen the hands of the universities. When the British Association last met in Sheffield geography was the most despised of school subjects, and it was quite unknown in the universities. It owed its first recognition as a subject of university status to the generous financial support of the Royal Geographical Society and the brilliant teaching of Mr. Mackinder at Oxford. Ten years ago Schools of Geography were struggling into existence at Oxford and Cambridge, under the auspices of the Royal Geographical Society. A single decade has seen the example of Oxford and Cambridge followed by nearly every university in Great Britain, the University of Sheffield among them. In Dr. Rudmore Brown it has secured a traveller and explorer of exceptionally wide experience, who will doubtless build up a Department of Geography worthy of this great industrial capital. The difficulty, however, in all universities is to find the funds necessary for the endowment, equipment, and working expenses of a Geographical Department of the first rank. Such a department requires expensive instruments and apparatus, and, since the geographer has to take the whole world as his subject, it must spend largely on collecting, storing, and utilising raw material of the kind I have spoken of. Moreover, a professor of geography should have seen much of the World before he is appointed, and it ought to be an important part of his professional duties to travel frequently and far. I have never been able to settle to my own satisfaction the maximum income which a department of geography might usefully spend, but I have had considerable experience of working a department with an income not very far above the minimum. Till this year the Oxford School of Geography has been obliged to content itself with three rooms and to make these suffice not merely for lecture-rooms and laboratories, but also for housing its large and valuable collection of maps and other materials. This collection is far beyond anything which any other university in this country possesses, but it shrinks into insignificance beside that of a rich and adequately supported Geographical Department like that of the University of Berlin. This fortunate department has an income of about 6,000*l.* a year and an institute built specially for its requirements at a cost of over 150,000*l.*, excluding the site. In Oxford we are only too grateful that the generosity of Mr. Bailey, of Johannesburg, has enabled the School of Geography to add to its accommodation by renting for five years a private house, in which there will temporarily be room for our students and for our collections, but where we can never hope to do what we might if we had a building specially designed for geographical teaching and research. Again, Lord Brassey and Mr. Douglas Freshfield, a former President of this Section, have each generously offered 500*l.* towards the endowment of a professorship if other support is forthcoming. All this is matter for congratulation, but I need hardly point out that a professor with only a precarious income for his department is a person in a far from enviable position. There is at present no permanent working income guaranteed to any Geographical Department in the country, and so long as this is the case the work of all these departments will be hampered and the training of a succession of competent men retarded. I do not think that I can conclude this brief Address better than by appealing to those princes of industry who have made this great city what it is to provide for the Geographical Department of their university on a scale which shall make it at once a model and a stimulus to every other university in the country and to all benefactors of universities.

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ADDRESS

TO THE

ECONOMIC SCIENCE AND STATISTICS SECTION

BY

SIR H. LLEWELLYN SMITH, K.C.B., M.A., B.Sc., F.S.S.,

PRESIDENT OF THE SECTION.

WHAT should be the scope, form, and contents of a presidential address to the Economic Section of the British Association?

If we attempt to solve this question by the historical method we obtain indecisive results, for a hasty glance at the addresses of my predecessors in recent years shows a very wide range of variation, from the detailed examination of some particular point of economic theory or practice to a general survey of the past, present, or future of Economic Science, or a funeral oration over the grave of some obsolete school of thought. Nor does the comparative method—the examination of the prevalent customs in other sections of the British Association—give any clearer guidance.

If we fall back upon the *à priori* or deductive method it may not be difficult to construct some theoretical ideal of what a presidential address should be. Thus it might be suggested that the opening address should be to the general proceedings of the section what an overture is to an opera—introductory, suggestive, occasionally reminiscent of previous works of the series, touching skilfully on the principal ‘motives’ of current discussions without resting too long or too heavily on any. It should doubtless include a systematic and impartial review of the whole range of contemporary economic activity both on the scientific and on the practical sides, with just enough accompaniment of judicious applause or censure, encouragement or warning, to maintain the interest and to afford the necessary light and shade, while avoiding the dangers of polemical controversy which would provoke a desire for retort and refutation.

Unfortunately, *à priori* reasoning notoriously depends upon hypotheses, which rarely correspond accurately with realities, and which in the present case are veritable feet of clay. In particular our theory presupposes that the Council of the British Association have appointed a President who has the necessary leisure of mind to keep himself fully abreast of economic thought and research throughout the world, and who can devote to the pursuit of economic science that unremitting and undistracted attention which the economic man is popularly supposed to devote to the pursuit of wealth. Lastly, he is also assumed to be in a position of untrammelled freedom to tell anything he happens to know. Instead of this the Council have selected a hard-worked official with little leisure for the pursuit of any subject outside the range of his official duties, while as regards

subjects within that range (including almost every branch of applied economics) the very nature of these duties imposes the most stringent reserves on his power of free discussion.

Nor does this disability arise from any formal rule or prohibition or considerations of official etiquette. It is the inevitable result of the working of natural laws which connect causes with their consequences direct and indirect, immediate and remote. Were I to-day to discourse to you freely and without reserve on questions of fiscal policy and commercial treaties, industrial combinations, railway agreements, and shipping rings, patents and copyright, merchandise marks, labour organisation, bankruptcy legislation, and municipal trading, I think that I could probably afford you an hour's entertainment which might be both interesting and instructive, but I should look forward with some misgiving to the reactions of my indiscretions on the future working of the machine of which I am an attendant.

This, then, being the plight in which I find myself, my only course is to do the best I can under rather unpropitious conditions, to put before you without much order or system such thoughts and ideas as occur to me with regard to recent and present economic tendencies, and, if I must be indiscreet, to confine my indiscretions within reasonable limits.

But at the outset I must pause to say a word or two as to our losses during the past year. Death has been unusually busy of late in the ranks of economists, and hardly any important country has escaped its ravages. I can only mention a few of the more important losses.

To take our own country first, we are mourning the loss of the recognised doyen of economic statistics. Sir Robert Giffen was unequalled in his broad grasp, fine sense of proportion, and clear insight in handling masses of common statistics, and in extracting from them their real significance. But he was more than a statistician; his mind was extraordinarily fresh and original, and there were few subjects within the range of practical economics which he touched without illuminating. Of Sir Robert Giffen as an official I speak from personal knowledge, for he was my first chief in the Civil Service, and I shall always remember the ungrudging help and the generous support and appreciation which he bestowed on his staff and colleagues.

Looking across the sea we share the regrets of our American colleagues for the loss of Simon Newcomb, a great astronomer who had also made a name among economists for the freshness, vigour, and originality of his frequent incursions into the domain of our science; and also of Professor Sumner, of Yale, who stood very high indeed among economic teachers.

In Léon Walras, France has lost a distinguished economist whose most important work belongs to a past generation, but whose death severs one more of the few remaining links with the period of the first revolt against the doctrinaire successors of the classical economists. Walras was one of the early rebels, and he shares with Menger and Jevons the honour of being a pioneer of the application of mathematical methods to economic theory.

Another and yet more recent loss sustained by France has been the death of Emile Cheysson, distinguished both as administrator and statistician. His work was of particular importance in those fields of social enterprise in which the mathematical methods of the actuary are needed to place philanthropic effort on a sound basis, and he also devoted much personal energy to the promotion of schemes for social improvement.

The untimely and almost tragic death of Ernst von Halle occurred on June 29, 1909, and therefore falls just outside the period of the last twelve months. But I cannot pass over in silence the loss which Germany has sustained by the death of this brilliant young economist and public servant. Von Halle was probably best known in this country by his study of Trusts, published at a time when industrial combinations were only beginning to attract the attention which has since been so abundantly bestowed on them. But perhaps his most characteristic work was done in connection with the maritime development of Germany, and one of his latest economic writings was the essay which appeared in the 'Economic Journal' eighteen months before his death, in which he endeavoured to combat misapprehensions as to the

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historic basis, the objects, scope, and necessary limitations of a movement which is of such profound interest to the world at the present time.

One of the most notable losses of the year has been that of Nicholaas Gerard Pierson, the economist statesman of Holland, who was not only unquestionably the most distinguished of contemporary Dutch economists, but who for four years (1897-1901) occupied the post of Prime Minister of his country.

Austria mourns the loss, at the age of sixty-one, of Dr. Franz Ritter von Juraschek, head of the Austrian Central Statistical Commission, whose eminent services to statistics were especially fruitful in the domain of international comparisons.

Two other veteran economists have passed away—namely, Dr. Julius Kautz, the Hungarian economist, at the age of eighty; and Professor Aschehoug, of Christiania, at the age of eighty-seven. Kautz is a link with the time of Roscher and the early German historical school. Aschehoug had occupied a professorial position in the university for fifty-six years.

Turning from the review of our losses to the progress of economic science and statistics during the past year we find perhaps little that is sensational to record, but much evidence of quiet, steady, and solid progress along various lines of research. I am now, of course, speaking of the output of new and original economic work, not of the popular discussion of practical economic problems, which has been perhaps more active and persistent, not to say blatant, during the last twelve months than in any corresponding period of recent times. For reasons which I shall indicate presently, I am by no means inclined to take a purely cynical view of the value of these popular discussions even when carried on amid the heat of party politics. Good as well as evil, and often in greater measure than evil, may, and I am convinced does, result in the long run to economic science from popular discussions of economic questions, however superficial they may be, and however distorted by bias and passion. But it is not of this sort of thing that I am now speaking, but of modern economic work and thought properly so-called.

Among the most welcome tendencies of recent economic thought and writing has, I think, been a marked falling-off in the sterile and strident controversy that has so long been carried on between the advocates of different methods of research. We might, I think, be justified in saying that a truce, if not a permanent peace, has been declared between the champions of the so-called historical and abstract or analytic methods, based on the mutual recognition that both methods are indispensable, and are, when rightly used, complementary rather than antagonistic. The metaphor of the two feet which are necessary for walking (or at least for any advance which is other than a series of spasmodic hops) seems to have brought comfort to some who, a short time ago, were at death-grips. This happy cessation of a controversy which, though once big with great issues, has of late years been little but a barren academic wrangle, is pure gain to economic progress; for it may be taken as a general rule that long-continued and acrimonious controversies about scientific method are a sure sign of a low level of scientific achievement. When men of science and action have important work on hand they have no time or use for elaborate polemics and recriminations as to the proper tools and apparatus to employ. Not, of course, that the problems of method are ever unimportant, but in times of real active advance they occupy a secondary place and are naturally and almost unconsciously solved in relation to each positive economic question as it arises. It is only at times of low positive activity that the question of method assumes an independent position as the dominating problem of the day.

If we were to apply both the historical and analytic methods in combination to the elucidation of the controversy between them which is now dying down, it would, I think, appear that the supposed opposition between the two schools of method, so far from being fundamental, arose largely from circumstances which were local and temporary, that the antagonism was for a time both necessary and fruitful, but that it has long ceased to have either of these characters, and has been a real obstruction to advance. No doubt the pursuit of the two branches of research will remain to a large extent distinct and in separate hands, owing to the natural division of labour according to personal

tastes and aptitudes; but the historian and the theorist will each in future be clearly conscious that the work he is doing is only partial and one-sided, and cannot be made complete without the assistance of the other. And both will, I hope, be conscious to an increasing extent of their common dependence on a third line of research, at present only in its infancy—namely, the quantitative investigation of economic phenomena mainly through the scientific study of statistics. I do not know whether statistical investigation has been conceived as forming one or more toes of both or either (and, if so, which) of the two now famous feet on which political economy is said to rest, but it certainly seems to be an indispensable adjunct to both. At a very early stage of abstract analysis it is usually necessary to give due proportion and precision to our ideas by clothing the dry bones with the flesh of concrete facts, and across the flow of economic phenomena which history investigates we need to take frequent sections and soundings if we desire to measure effects as well as to trace causes. I have some doubt if the economic animal is a biped after all. I do not want to make him a centipede, but I think that at least three feet must be postulated—abstract analysis, historical (including comparative) investigation, and concrete statistical measurement.

The reconciliation of the historical and analytic schools of research does not diminish, perhaps it increases, the necessity for perpetual vigilance on the part of investigators of both types against their peculiar besetting sins. The historical inquirer, full of his doctrine of relativity, must beware of supposing that he justifies an institution when he unfolds its origin and shows how it grew naturally out of the conditions and circumstances of its day, or that he conclusively condemns its continuance when he shows that the conditions and circumstances of its origin have passed away. On the other hand, the analytical reasoner needs to be continually on the watch to detect in his assumptions which appear universally valid the elements which in reality are true only for particular times and places.

The next tendency which I notice in recent economic literature is one which can only be welcomed with some qualifications. I refer to the increasingly technical character of the phraseology and methods employed. Economic and statistical science is in the course of elaborating a highly specialised technical terminology of its own, so that no careless reader may be misled by an ambiguous word or phrase. Since the fundamental terms of economic science are words in common use, such as wealth, capital, wages, rent, prices, and the like, each of which is used in everyday conversation in half a dozen different senses, and none of which have any claim to scientific precision, it can be readily understood that the greatest of the classical economists was not always proof against the danger of using his terms in different senses in different portions of his argument, while the pamphleteer successors of the early economists scarcely made a serious attempt to use terms in a consistent way throughout.

Hence the great convenience for the purpose of analytical reasoning of having a purely scientific terminology, free from ambiguity and incapable of being confused with the popular words used in the market-place.

And yet while welcoming the movement which has given us such terms as 'flow,' 'national dividend,' 'consumers' surplus,' 'quasi-rent,' and the like, I venture to sound a mild note of warning. All these special terms and the technical reasonings into which they enter should be restricted so far as possible to purely scientific publications, and even in these their meanings should be carefully explained and not assumed. I do not say this wholly or mainly in the interests of the popular reader, but quite as much, and perhaps more, in the interests of science. Political economy cannot from its nature ever be in the position, say, of mathematical physics, which, though necessarily a closed book by reason of its abstruseness to the vast majority of intelligent persons, nevertheless commands their complete confidence. No one is afraid to trust himself on a bridge because he cannot follow the reasonings which have made the engineers confident in its stability. He accepts the results though he cannot follow the process. But I do not think that the ordinary man will readily take up this attitude towards a science like economics dealing with matters of everyday life which he deems himself fully competent to understand and discuss. If then the language of economic science is to him an unknown language, he is likely to

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pass it by as an affair of a clique having no relation to practical life. This might not perhaps matter much if we could get along without the practical man, but we cannot, because we need his criticism at every point. A vast amount of rubbish is, of course, said and written annually on economic matters by persons who are imperfectly equipped for the task either of discovery, exposition, or criticism; but this should not blind us to the fact that a great deal of very valuable economic criticism comes, and still more ought to come, from men who are not professional economists, and who have no leisure to keep themselves abreast of the newest modes of professorial expression. Now it is of the highest importance that we should do everything to encourage and nothing to deprive ourselves of this criticism, which is essential in order to keep economic thought sane and sound and in touch with realities. On all grounds it would be deplorable if through the obscurity of its language economic science should relapse into the position of an esoteric doctrine confined to a small circle of initiates, only the bare results of which are capable of dogmatic statement to the outside world. Not only is the doctrine unlikely to be accepted on these terms, but, being *ex hypothesi* a doctrine elaborated in the closet by experts without contact with the fresh breezes of everyday business experience, it is quite certain to suffer in balance and proportion and reality, and in all that gives it value to the world.

Of course the vast bulk of public criticisms and suggestions on technical projects or scientific theories are shallow, irrelevant, and futile. But even though there be 99 per cent. of chaff, the odd 1 per cent. of grain may well be priceless. Now I would say very seriously that it is of the highest importance to our science not to be cut off by any barrier of unintelligible phraseology from the advantage of the co-operation, whether by criticism or suggestion, of that great body of persons who, while not professional economists, are practical experts in one or other of the branches of economic knowledge.

What I have said with regard to the use of special technical phraseology in economic reasonings intended for the eye of the general reader applies of course with special force to the use of mathematical symbols and modes of expression. I do not think that either by taste or training I am likely to underrate the value of mathematical methods in elucidating economic problems. The essentially mathematical conception of functions and mutually dependent variables offers incomparably the most powerful and appropriate method of expressing the interrelations among such economic phenomena as rent, interest, price, wages, product, and capital. Moreover, the apparatus of the infinitesimal calculus affords the only satisfactory mode of representing and analysing continuous economic changes. Ordinary verbal argument on complicated questions (say) of international values or of the ultimate incidence of taxation, can hardly go beyond the analysis of certain particular cases of discontinuous changes selected for purposes of illustration.

Nevertheless, I trust that those who recognise with me the value of mathematical modes of expression will be extremely careful to restrict mathematical language to the pages of technical economic journals or the footnotes and appendices of more popular treatises, and to re-state all the conclusions arrived at by this means, with at least an outline of the arguments which lead to them, in ordinary language free from technical symbols.

The last-mentioned condition is, I think, important, not merely for the reasons which I have already given, but for another which applies peculiarly to mathematical arguments. Before starting our mathematical analysis we are bound, of course, to define very precisely the meaning of the quantities which the symbols express, and this in itself is very salutary, for it compels us to recognise frankly the hypotheses on which our argument will depend. But when the mathematical process has once begun we very quickly lose sight of the economic contents of the symbols. As the school-girl in the examination said: 'Algebraical symbols are what you use when you don't know what you are talking about.' Now from the point when we lose sight of the economic significance of our symbols we lose the means of applying the check of common-sense to the intermediate stages of our analysis, and we do not recover this power of criticism until the final results emerge and are interpreted in ordinary language. Between the points at which our economic assumptions are translated into mathematical formulæ and our ultimate results are re-translated into ordinary language, there is nothing to

enable us to take stock of the position and to warn us of the direction in which we may be drifting.

Another tendency which I see in economic research is to attach increasing importance to quantitative measurement with the aid of the separate though auxiliary science of statistics.

This is a tendency which is wholly welcome and which is likely to become of increasing importance in the immediate future. Like all salutary movements it is, of course, not free from incidental dangers. It is clearly possible to lean on the third foot of our tripod more heavily than it will bear, and it is not only possible but probable that the public demand for quantitative information will for some time outrun the available means of supply. Certainly the pressure for more and better statistics is increasing very rapidly at the present time, and not always with due regard to the limitations of what is practicable. In order to form a sound and sober judgment of the true possibilities of advance along this line it is necessary to recognise frankly the chasm which separates the crude and primitive means of measurement, or rather of quantitative estimate, which alone are open to the economist, from the relatively perfect apparatus and methods which are available to the physicist.

But the more imperfect our data and the more primitive our means of enumeration and measurement, the more perfect and complex needs to be our scientific apparatus for criticising the results and enabling positive inferences to be drawn therefrom. In other words our dependence on statistical science is proportionate to the defects of our means of direct and accurate measurement.

Statistics is often classed with economic science (and, indeed, the two names are linked in the title of our Section) as though there were some essential connection between the two. But this, of course, is not the case, statistical methods being used to a greater or less extent in all the branches of science which occupy the other Sections of the British Association. In fact, it is probably in connection with biology rather than economics that the most important original research by statistical methods has been recently carried out.

Quantitative measurement is the backbone of science, and whenever the quantities handled are in any way indeterminate or inexact, either in regard to their definition or enumeration, we need the assistance of scientific rules and criteria to enable us to correct, or neglect, or at least to limit the error introduced into our results by the faulty nature of the data. The mere direct measurement, counting, or weighing of quantities is scarcely worthy of being called a statistical operation: statistical science properly so-called is mainly concerned with establishing the conditions under which approximately true inferences may be drawn from imperfect data. On this problem the modern statistician brings to bear the powerful engine of the mathematical theory of probability.

It is no part of my intention to attempt to discuss the methods of modern statistical science: I only wish to emphasise the close connection between the elaboration of these methods and the imperfection of the data to which they are applied.

Now the data of economic statistics are almost all liable to error either through defects of definition or extension. Either the only data available are not precisely of the nature required for the purpose of the particular investigation, so that we have to do the best we can with second or third best materials, or the data obtainable only cover a comparatively small portion of the total field, and there may be formidable questions as to how far the results based on such limited data are really representative. Sometimes we suffer from both these difficulties, and statistical inquiries oscillate habitually between the two dangers as between Scylla and Charybdis—the danger on the one hand that over-insistence on elaborate precision of data may so narrow the field that the results obtained from the sample may be unrepresentative of the whole, and the danger on the other hand that the ‘common statistics’ which alone cover the whole field are necessarily obtained not only for one but for many diverse purposes, and are therefore unlikely to be entirely appropriate to any particular inquiry. Between these characteristic dangers of the intensive and extensive methods respectively we have to steer our difficult course as best we may.

The peculiar dangers of the intensive method are so obvious as not to need special emphasis. Everyone is aware that better results are obtained from a wide

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than from a narrow range of observations, and indeed I think that the besetting error of the public is to attach too much rather than too little importance to this defect. It requires a trained observer to understand how few samples if honestly chosen at random suffice to give a good approximation to the truth.

But one special difficulty which attends the extensive method often receives, I think, less attention than it is due.

Statistical investigations which cover very large masses of returns and are repeated periodically so as to admit of historical comparisons must from the nature of the case be based on what Sir Robert Giffen used to call 'common statistics.' The term 'common' is not used in any derogatory sense, but in its strict meaning, viz. statistics not designed or compiled specially for one particular purpose, but destined to serve several purposes in common. The obvious reason for this is that human beings are not willing to spend their lives in filling up forms of inquiry to suit the needs of every statistical investigator. There being, therefore, limits to the amount of statistical data which can be extracted from the public, it follows that returns filled up primarily for one purpose have to serve several other purposes as well.

A single example of the multiple use of common statistics will suffice, viz. the statistics of foreign trade.

Primarily the classification of the foreign trade statistics of every country is based on the subdivisions of its Customs tariff, the object being to enable the operation of the tariff to be watched. Of course, in the case of the United Kingdom, where Customs duties are now confined to a small number of articles, the existing tariff classification has but a minor effect on the classification of the trade accounts, though even our trade accounts show abundant traces of the influence of the tariff subdivisions of bygone days. But in protectionist countries the statistics of foreign trade are practically governed by tariff considerations, and international statisticians are fully aware of the difficulty which tariff variations place in the way of the attainment of statistical uniformity among the different commercial countries of the world.

The second purpose the trade accounts have to serve is that of the practical trader, who is concerned not at all with the attainment of statistical uniformity, but very much with the safeguarding of his own particular trade interests, and who therefore wishes for a classification which will furnish him with all the data needed for his business, while suppressing all details that will serve the purpose of his trade rivals and foreign competitors.

When we have reconciled the claims of the Customs authorities and the practical trader we have to meet the insistent demands and criticisms of persons interested in public affairs who wish to learn from the trade accounts what is the true economic state of the nation, in comparison either with its foreign rivals or with some previous period of its own history. But it is very soon discovered that the requirements of these critics, while not always compatible with those of the practical trader or the financial authorities, are not consistent among themselves. So far as they wish to make international comparisons they recommend modifications which would assimilate our classification to that of foreign countries, but so far as they wish to make historical comparisons they deprecate any changes of classification that will impair continuity.

It is not necessary to labour the point further, though illustrations perhaps even of a more striking kind might be afforded by the multiple uses to which the results of the general census are put.

Thus those who collect and compile common statistics have to serve many masters—sometimes with the usual result. If every statistical enthusiast had his way and the declarations required from traders and citizens were adapted to meet the precise requirements of each investigator in turn, the schedule to be filled up would be something from which the practical business man would recoil in horror. Hence, in practice, we are driven to a rough compromise between divergent and conflicting aims, relying on the resources of statistical science to enable us to apply the needful qualifications to the necessarily imperfect results.

So far we have only been dealing with the apparatus and methods of research and exposition, and not at all with the objects to which such research should be applied, still less with the ultimate ends of economic study and conduct.

The next tendency we have to note belongs to quite another region of ideas.

This is the growing emphasis laid on ends as distinguished from means as the subject of economic study.

There used to be some disposition to question whether the economist was at all concerned with ends, whether he had not fully discharged his duty in making a correct analysis of the structure of existing economic society and of the forces acting upon it; and it was rather the fashion to suggest that when this analysis was completed the economist should depart, and leave the practical statesman to collate his report with those of the moralist and the politician, and to draw the necessary inferences as to practical policy from their combined study.

Such a limitation as this would have been quite foreign to the ideas of the early makers of political economy. The mediæval thinkers were frankly concerned with economic conduct and morals; the mercantilists with the very practical question of adapting economic policy to the race for national power; the physiocrats with the freeing of pre-revolution France from the network of vexatious and oppressive State restrictions on industry with a view to giving free play to the natural expansion of manufacture and commerce. Malthus was engaged in combating social utopias, while Adam Smith was concerned, as we have been recently reminded in Professor Nicholson's striking book, with every field of political and moral activity, as well as with that region within which economic science is usually supposed to be confined. The author of the 'Wealth of Nations' would certainly have been astonished at the suggestion that political economy is not concerned with ends. Yet the first step towards at least a temporary divorce between the study of economic ends and means was taken when Adam Smith enunciated his famous conclusion that 'all systems, either of preference or of restraint . . . being . . . completely taken away, the obvious and simple system of natural liberty establishes itself of its own accord.'

I am not concerned to discuss whether this conclusion was an induction from experience or a deduction from moral or theological presuppositions, or how far it is to be qualified by many other passages in the same great work. But in any case the proposition that the natural forces of human desires and aversions, and their mutual reactions, will naturally and without conscious intention on the part of the individual lead to the greatest advantage of society, became the starting-point of a school of propagandists of economic truth who too often identified the indicative with the imperative mood, and blurred the distinction between scientific generalisations and moral precepts of conduct.

To those who adopted this view of the Economic Harmonies in its extreme form the question whether political economy is concerned with ends as distinct from means became a relatively unimportant question, and fell naturally into the background.

The maximising of production (or, as we should now say, of the national dividend) is the only end that these economists could be said to propound, the distribution of the resultant wealth being automatically determined by the beneficent action of the 'system of natural liberty.' Sooner or later the current utilitarian philosophy, with its principle of 'greatest happiness,' was bound to come into conflict with this ideal, for the policy of maximising satisfaction is clearly not identical with that of maximising production. The enunciation of 'maximum satisfaction' as an end necessarily raised—though it could not solve—the question of distribution of wealth among different social classes. In regard to this matter it shook confidence in the shallow dogmatism of the propagandist economists, but it substituted no definite alternative commanding general assent, and accordingly the immediate practical result on economic thought was not to inspire it with a new creed, but to deprive it of all creed, and to replace the art of political economy by the conception of an economic science concerned solely with the ascertainment of the results which flow from certain hypothetical assumptions, and not at all with guiding mankind towards a desirable goal.

Such a view could hardly hold permanent sway, though it was a great advance on the dogmatic and insolent optimism which it displaced, and nominally at least it dominated English economic thought from the middle of the nineteenth century almost to the present day. This domination has, however, been more nominal than real. The limitation was from the first subjected to vigorous criticism, and at bottom the critics were right, for however carefully we may expel the idea of ends from our reasoning, current ideals and even prejudices

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are certain to affect our choice of hypotheses. As a fact, all the latter-day economists have by one expedient or another escaped from their own theoretic limitation. To take a single example, it has become a recognised axiom of economic reasoning that the diminution of poverty is a proper object of economic effort. Of course, the pure utilitarian would have nothing to do with distinctions of quality in happiness—distinctions which are fatal to the simplicity of his magic formula—and the utilitarian school of economists attempted no direct discrimination in their measurements of utility and value between the qualities which render an article an object of desire. The fact that a thing is desired proved its right to be called 'useful' within the meaning of their theory, and it must be admitted that no coherent objective theory of value could be built up on any other basis. Nevertheless, it is no new discovery that things of equal value to the individuals who possess them at a given moment may conduce in very different degrees to the ultimate national advantage. The old distinction between productive and unproductive expenditure, and Adam Smith's difficult argument as to the relative advantages of near and distant trade, are examples of distinctions of this kind which were present to the minds even of the economists who were most dominated by the theory of natural liberty.

The great and growing importance attached by the best modern economists to the element of time, and the consequential recognition of the importance of ultimate as distinct from immediate effects, tend *pro tanto* to discriminate between different qualities of satisfaction, and to give increased weight to those kinds which tend to the building up and husbanding of the permanent economic interests of the Commonwealth, as compared with the transitory satisfactions which perish in their own gratification—in short, between the nobler and ignobler forms of utility.

I think it is a matter which needs the careful consideration of economists at the present day, whether the time has not come when they should accept fully and frankly the task, from which in any case they cannot entirely escape, of distinguishing between noble and ignoble ends of economic conduct, and should regard all their methods of research—historical, analytical, comparative, and statistical—as only means to this end.

On the present occasion I cannot do more than illustrate my meaning by a single important example.

The recognition that the purposes and modes of consumption of commodities have to be taken into account, as well as the mere amount of the satisfaction yielded by them to their consumers, brings with it the necessity for recognising the distribution of income in respect of time, no less than in respect of class, as an essential factor in the national well-being.

Thus, for example, a regular income of 2*l.* a week may have a very different economic significance from an income amounting in the aggregate to 104*l.* in the year, but receivable in irregular and unequal instalments. Still more widely does it differ economically from the chance of a variable annual income averaging 104*l.* one year with another.

Now one of the most significant and important economic tendencies of the present day is the growing recognition of the importance of security and regularity in all operations of industry and commerce. It is, of course, a trite commonplace that the foundation of commerce is security—that safety of person and property and security for the performance of legal obligations are essential conditions of all industrial and commercial development. But it is not of these elementary guarantees that I am speaking, but of the tendency which I see to attach ever greater importance to the certainty and regularity of sequence as distinguished from the mere aggregate volume of business transactions. This tendency is reflected in the enormous development of the method of insurance as a protection against risk.

Nor is this development confined to business transactions properly so-called. A number of the risks and contingencies of human life which cause irregularity and uncertainty in working-class incomes have been brought within the sphere of insurance, whether by voluntary institutions or, as in Germany, by a State system of organisation. And the question of the perfec-

tion and further development of the methods of social insurance is absorbing a large amount of the best thought of the day.

All this points to the growing importance attached by social observers to stability and regularity, and the grounds for this attitude are sufficiently obvious, whether we look at the matter from the point of view of the economy of the workman's household, or of the deteriorating effects of irregular habits on physique and character. It may perhaps be suggested that the growing social concern for the maintenance of stability is the counterpart of the growing conviction that with the world-wide development of industry the causes of fluctuations and irregularity are becoming continually more incalculable and their effects more unavoidable by unaided individual effort.

Is this tendency to exalt security as an end a healthy tendency, or ought it to fill us with apprehension?

The ideal of security may not at first sight seem a very heroic aim to put before a country whose economic traditions form a veritable romance of adventure full of the joy of risks encountered and dangers overcome. Some may think with misgiving that the conscious pursuit of a policy of safety implies that we have passed the stage of economic youth and expansion and are entering on the dusk of old age. They may feel as when at Rome we contemplate Aurelian's great wall which for centuries withstood the inroads of barbarians, but the building of which none the less marked the definite close of the period of the fearless and aggressive supremacy of Rome. Are the nations of Europe being invited to enter with the old gods into the fortress of Valhalla, there to await in well-planned security but in growing gloom their inevitable decline? The question is cogent and searching, and modern nations must find the true answer at their peril, for if the two ideals of free adventure and economic security admit of no reconciliation, then the fate of our civilisation is only a matter of time.

But fortunately it is not necessary to admit the essential opposition of these two ideals rightly conceived. For as it seems to me there is a noble as well as an ignoble ideal of adventure, and, corresponding thereto, there is a noble as well as an ignoble ideal of security, and the great problem that lies before us in the future is to distinguish rightly between them and to direct our national policy accordingly.

The first step towards making this distinction is to recognise that ignoble as well as noble results are produced by exposure to risks. If fearless resolution and foresight in encountering and combating danger and risk produced the race of Elizabethan mariners and explorers, and to-day gives us a Shackleton or a Sven Hedin, we know also the craven and panic-stricken population which lives on the slopes of a volcano, exposed every day to incalculable risks against which no precautions can avail.

It is, I think, a definite induction from history and observation that when risk falls outside certain limits as regards magnitude and calculability, when in short it becomes what I may call a gambler's risk, exposure thereto not only ceases to act as a bracing tonic, but produces evil effects of a very serious kind.

It is to the general interest, and it tends to the building up and strengthening of the national character, that everyone should have as strong a motive as possible to guard against risks which can be avoided by reasonable precautions on the part of the individual, and it is also to the general interest that within certain limits the individual should have sufficient resisting power and reserve strength to encounter without the support of his fellows the ordinary minor ups and downs of life which it is not within his power to avoid. What these limits are cannot be laid down dogmatically: they vary widely from nation to nation, from class to class, and from age to age. Vicissitudes which mean famine to the savage pass quite unnoticed in advanced industrial communities, and classes who are accustomed to yearly salaries are unconcerned with fluctuations which bring privation to the weekly wage earner. But within any given nation and class the limits probably change but slowly, and though different schools of social observers will certainly fix the limits at somewhat different points, and there is no doubt a neutral zone within which the relative public advantages and disadvantages of exposure to risk are fairly equally balanced, or at least may be open to legitimate debate, I am disposed to think that the majority of fair-minded men would not differ very widely in the principles governing the demarcation between the

of individual and of social protection against economic risk. To take, for example, the risks of unemployment, I think most people would agree that the personal risk of losing employment through bad work, irregular attendance, or drunken habits is one which it is absolutely necessary in the public interest to leave attached in all its force to the individual workman. For the community to guarantee employment to all irrespective of personal effort or efficiency would necessarily impair the national character and lower the national standard. This is, therefore, a risk the direct incidence of which must be borne by the individual, the action of the community being confined to such indirect measures as may strengthen the power of the individual to meet the risk, as, for example, by technical and general training.

On the other hand I think that most people would agree that in a country like the United Kingdom at the present time, the incalculable risk of a prolonged depression of trade, due perhaps to some financial catastrophe thousands of miles away, is one the exposure to which of the individual workman does little but harm. Such a risk is too much beyond his powers of foresight, and also too great in magnitude in proportion to his reasonable opportunities of making provision, to exercise any appreciable effect in stimulating self-help, while the liability to see all his savings swept away in a few weeks by cyclical fluctuations in employment which he can do nothing to avoid is a demoralising risk acting on his character precisely like the liability to earthquake or other cataclysm, and discouraging to a marked extent the accumulation of savings and the development and maintenance of habits of providence.

Between these two extremes, the risk due to personal inefficiency and that resulting from a world-wide depression of trade, lie intermediate classes of risks about which there might be more difference of opinion, and the incidence of which probably acts on national character in very different ways in countries at different stages of development.

I propose presently to examine more closely some of these classes of risks. At the moment, however, I am only concerned to illustrate my general proposition that neither free adventure nor economic security suffices singly as an ideal of economic conduct without careful discrimination, and that the criterion for such discrimination is the effect of exposure to each class of risk in building up or degrading the national character.

In suggesting that the attention of economists is being directed and will continue to be directed in an increasing degree to the ends of economic conduct as distinct from a mere analysis and description of existing conditions, I have taken a single example, the pursuit of economic security as an objective, and have drawn a vital distinction between the classes of economic risks exposure to which tends to the building up or to the degradation of the national character. And as regards these risks I have taken a single illustration, that of unemployment, partly because the evils resulting therefrom have been very much in our thoughts during the last few years, partly because their analysis affords good illustrations of almost every class of economic risk.

I might go on to take other examples, but I think that it may perhaps serve a more useful purpose if during the time that remains to me I follow up in further detail the particular illustration which I have chosen, and inquire specifically how far the risks of unemployment are risks which it is expedient in the public interest that each individual should be left to meet unaided, or how far they are from the social point of view 'insurable risks' which can properly be met by combined action.

We shall find that the reply to the proposition is by no means a simple one, that it will differ to a large extent for different trades, and that probably it will also differ widely for different countries.

At the outset it is to be noted that I use the term 'insurable risk' for the purpose of this inquiry in a much narrower sense than that which it bears in the ordinary language of the insurance world. Broadly speaking, if the term be used in its widest sense there are no risks that are not insurable except those which are the result of the direct wilful act of the insured person. Thus you can insure against fire but not arson, against death but not suicide. And even with regard to acts which are voluntary the modern tendency is to take a very broad view, and

to narrow the classes of cases excluded. Thus most life assurance companies will pay on death, even if due to suicide, provided that the policy was taken out sufficiently long before the death to make it fairly certain that suicide was not in contemplation at the time.

As I am now using the term 'insurable,' however, I mean not merely a risk in respect of which you could get some company or underwriter to quote you a premium, but a risk for which some sort of social insurance is a practicable and appropriate remedy—bearing in mind the critical distinction already drawn between different classes of risks.

Moreover, by 'insurable risk' I do not mean a risk which can be fully covered by insurance, but one the consequences of which may be mitigated by a payment which nevertheless falls far short of complete indemnity. It hardly needs demonstration that full indemnity against the risks of unemployment could not be offered without disastrous results, inasmuch as a large section of persons regard idleness as in itself more attractive than work. The universal practice of organisations, voluntary or public, which insure against sickness, accident, or unemployment, is to make the benefit payable much less than the full rate of wages, and in all that follows this condition is assumed.

For the purpose of the present inquiry the causes of unemployment group themselves naturally under three heads—periodic fluctuations, local and industrial displacements, and personal causes.

Of these I have already touched on the first group in discussing cyclical and seasonal fluctuations of employment. Seasonal changes are, of course, the direct result of cosmical causes, and whether or not cyclical fluctuations are ultimately psychological or (as Jevons thought) cosmical phenomena, there can be no doubt that for our present purpose we may regard them as ultimate facts beyond the control of the individual. These two elements in unemployment are pre-eminently insurable elements, since, being due to recurrent oscillations and not to progressive changes, they can only be met by some method, either individual or collective, of spreading the earnings of good periods over good and bad alike, and not by any remedy which aims at altering the permanent relation between the demand for labour and the supply. Moreover, of the two alternative methods, collective insurance is more appropriate for the purpose than individual providence, because while the oscillations are fairly well defined, their intensity and (in the case of cyclical fluctuations) their wave length are affected by many uncertain elements, climatic, financial, industrial, and political, which are incapable of exact prediction, and (what is even more important) the personal incidence of the unemployment due to the oscillations is uncertain.

The next group of causes includes changes in industrial processes or methods or in the local distribution of industries, or in the character of industrial demand. How far are these classes of risks properly insurable?

As regards local distribution, the answer depends on the scope of the insurance scheme. No purely local fund can, of course, compensate a workman for the shifting of his industry to other districts, without incurring ruinous expense besides impairing the mobility of labour. If, however, the insurance scheme be national in scope and be worked in conjunction with systematic machinery for notifying to the workman the existence of vacancies in other districts, the risk of unemployment due to local displacement is clearly an 'insurable' risk. As no national scheme could embrace a wider area than the United Kingdom, the above argument does not apply with its full force to the risk of displacement of industry by foreign competition, and this case needs separate treatment. It is undoubtedly a risk beyond the individual's control, and it has, therefore, one of the essential marks of an insurable risk; and if the scheme embrace a large group of trades of sufficient variety to insure each other against the risk of some particular branch being attacked by foreign competition, there is no reason why this class of risk should throw an excessive burden on a national fund. The only question to be considered is, therefore, whether the insurance of British workmen in an industry liable to be transferred by competition to a foreign country will operate prejudicially by checking industrial mobility, there being obviously not the same opportunity for the workman to follow the work as in the case of local redistribution of industry within the limits of the insuring country.

In this respect the case we are now considering is on all fours with that of a trade decaying through a permanent change of industrial demand, or an alteration of industrial processes. If there is appreciable mobility of labour between the decaying trade and other healthy branches embraced within the scope of the insurance scheme, and if its magnitude is small as compared with the total area of industry covered by the scheme, then the risk is fairly insurable. If, however, these conditions are not fulfilled, the case of the permanently decaying trade may present a real though by no means insuperable difficulty which will have to be carefully borne in mind by those responsible for devising and working any unemployment insurance scheme.

The conclusion seems to be that the extent to which the risk of unemployment due to industrial and local displacement is properly insurable depends partly on a wise choice being made of the group of trades and of the geographical area to be embraced by the scheme, partly on the judicious limitation of the benefits payable thereunder. Our analysis points to the necessity of a large area, both geographical and industrial, and further suggests that the groups of trades included should be such as are unlikely as a whole to undergo wholesale and rapid displacement, and within which any decay to be apprehended is likely to be only local and partial and not on a scale too great to be compensated by the expansion of other branches of trade within the insured group.

There remain the risks due to personal causes. Of these we have already ruled out the risks due to the wilful act of the workman, and to these we must now add the personal risk attributable to exceptional deficiencies, physical, mental, or moral. These are not properly trade risks, the burden of which ought to fall in a special degree on those following a particular industry, and if they were allowed to do so they would ruin any scheme of insurance based on the trade group. There is still, however, one important class of personal risk to which all are liable, and which is in the main beyond the control of the individual, viz. the increasing liability to unemployment due to advancing years. I do not intend to trench on the important but quite separate problems of national provision for old age and invalidity as such. I am solely referring to the ascertained statistical fact that the chance of unemployment is a function of age, and that beyond a certain age the risk is materially increased. For example, among a body of nearly eight thousand engineers whose industrial records were analysed for the purpose, I found that whereas the average number of working days lost in the year by the whole body was fifteen, that for members below the age of forty-five was less than twelve, while for members between the ages of forty-five and fifty-five it was twenty, and for members between fifty-five and sixty-five, thirty-three. (Above sixty-five the figures are affected by superannuation.) The question we have to ask is, how far this class of risk is insurable?

The answer depends again on the scope of the scheme. A voluntary scheme which workmen are free to join and leave at their pleasure cannot deal satisfactorily with a risk of this kind, especially as no scheme of graduating contributions according to age is likely to be administratively feasible. Trade unions which give unemployment benefit are in an exceptional position, because they exist primarily for trade protection purposes, and hence have a hold on their members which no voluntary insurance scheme pure and simple could possess. Generally speaking, personal unemployment due to advancing years is insurable, and only insurable, under a scheme which applies compulsorily throughout the whole period of the workman's industrial life.

It results from our analysis that some of the risks of unemployment are properly insurable and others are not, and the next step is to ascertain broadly the relative importance of the insurable and non-insurable elements. Now an examination of the available statistics indicates clearly that at all events as regards certain large groups of trades in which unemployment is acute—namely, the building, engineering, and shipbuilding trades—the insurable element in the risk of unemployment predominates largely over the non-insurable element.

The method of statistical proof of this proposition may be indicated as follows:—

1. The percentage of unemployment in these trades—taking an average of good and bad years together—has not varied very widely during the period of

fifty years during which the statistics have been collected (the average for the first decade of the period was 5·6; for the second, 4·5; for the third, 6·8; for the fourth, 5·2; and for the fifth, 7·2. The average for the whole period was 5·9). As the period of oscillation is not exactly ten years, part even of the differences shown above is accounted for by the presence of an excessive proportion of good or bad years in particular decades. Thus we may fairly say that the element of unemployment due to progressive expansion or contraction of the demand for labour has been relatively small.

2. The percentage of unemployment found during the seven best years of the cycles has averaged 2·4, and only in two out of these seven years has it diverged by more than unity from this average.

3. The variation between the worst and the best years of the various cycles has averaged 8·5 per cent.—*i.e.*, more than three times the average percentage of unemployment in good years.

Now, broadly speaking, if we neglect any progressive changes in the total demand for labour, which are evidently slight as compared with the intensity of the periodic fluctuations in that demand, we may say that the percentage who are unemployed in years of good employment gives a maximum limit which the voluntary or non-insurable risk cannot exceed, since it also includes a number of minor accidental risks which are properly insurable—*e.g.*, the risk of unemployment through a fire or other accidental stoppage of work, or through defects in the local distribution of work and labour. Moreover, through the method of averaging employment over the year, the risk of seasonal want of employment is included, and this is mainly an insurable risk.

We may further regard the difference between unemployment in a good and bad year as giving a minimum measure of the insurable element in unemployment, since this difference is wholly the result of changes in the demand for labour, and is independent alike of the choice of the individual and of the gradual progressive changes, if any, that affect the total field of employment. Hence, as this difference is much greater than the minimum percentage in a good year, we may regard our proposition as being proved.

But at this point it is necessary to forestall and reply to an objection that will certainly be taken to the proposition just laid down. It will be pointed out that the experience of all relief works and of all schemes for the relief of distress due to unemployment establishes clearly that the great majority of the unemployed, or at least those who seek relief from distress, are very markedly inferior both as regards their industrial capacity and their physical and moral qualifications to the average employed workmen in the same trades. It is possible in a large number—probably in the majority—of these cases to trace clearly the operation of the personal defects which have contributed to unemployment—bad time-keeping, drink, slovenly work, and so forth—and those who are most familiar with the personal side of the problem are, I think, likely to put the personal or non-insurable element in the risk of unemployment very much higher than I have done in relation to the involuntary insurable element.

But in this criticism there is, I think, confusion of thought. Of course, if fifty men out of every thousand are out of work, those fifty individuals are likely to be less eligible than any other fifty taken at random. We might, if so disposed, construct a geometrical curve like those used in expounding the doctrines of utility and rent, in which the number of workmen employed is expressed by abscissæ and the degrees of efficiency by ordinates. Then it will appear at a glance that in a time of good trade the efficiency of the 'marginal' labourer—that is, of the worst man who just manages to retain his employment—is necessarily less than when the total demand for labour has shrunk from any cause. In the latter case the workmen discharged will for the most part be the less eligible section; and this state of things is quite independent of the true cause of the shrinkage in the demand for labour, so that while the personal defects of A may be the decisive reason why he is selected for unemployment instead of B, it does not follow that these defects are a principal or even a contributory cause of his unemployment.

It is a very complex and difficult question, only to be determined in any given case with full regard to all the circumstances, to what degree the increase or

decrease of the personal efficiency of the labourer conduces to an increase or decrease in the total demand for labour, or to what degree it merely enables him to shift the burden of unemployment on to someone else. Broadly speaking, there is no doubt that the total demand for labour is to a material extent dependent on its average efficiency. For example, a quite new demand for labour would be created if it were possible to level up all the feeble-minded and the physically and morally defective members of the community to the normal level. The abnormal defects of these persons (the true unemployables) are the *vera causa* of their unemployment, which does not in the main result from any deficiency in industrial demand, but from the fact that their services are so worthless relatively to that of the normal workman, that to all intents and purposes they may be regarded as an industrially useless surplus. Their unemployment is, therefore, emphatically not an 'insurable risk,' and they would need to be excluded from the scope of any scheme of insurance as rigorously as exceptionally bad lives are excluded from life and sickness insurance.

But if we put aside the comparatively small section of abnormals, there is ground for asserting that at all events within the great groups of trades to which I have already referred the influence of variations in efficiency among ordinary normal workmen on the total demand for labour at any given time, though by no means negligible, is not nearly so powerful as that of variations in industrial conditions which are beyond the control of the individual workman.

If, then, the insurable elements in unemployment in these trades largely predominate over the uninsurable elements, it would be comparatively simple to devise an appropriate scheme for dealing with the evil, if every separate case of unemployment could be readily assigned to its appropriate category, so that the benefits of the scheme should be exclusively available in the case of unemployment falling within the insurable category, just as a policy of marine insurance excludes in terms losses due to a number of specified causes. But in actual practice I need hardly say that any such separation of causes can only be made to a very limited extent. In the real world of industry the various elements that contribute to unemployment are inextricably intermixed. We can imagine the case of a carpenter who with equal truth might ascribe his unemployment to the competition of structural steel, to the general trade depression, to the severity of the winter, to local overbuilding, or to the defects in his own training.

There are a few, but only a few, of the causes of unemployment which can be definitely distinguished and excluded in terms from the benefit of an insurance scheme, such, for example, as holidays, strikes and lock-outs, voluntary leaving of a situation, sickness, and crime. If, then, it is necessary, as it certainly is for the success of a scheme, that it should discriminate against unemployment due either to exceptional defects or to causes within the control of the individual, this discrimination must be effected automatically in the course of the working of the scheme itself rather than by any rule professing to exclude ineligible cases from its scope.

The crucial question from a practical point of view is, therefore, whether it is possible to devise a scheme of insurance which, while nominally covering unemployment due to all causes other than those which can be definitely excluded, shall automatically discriminate as between the classes of unemployment for which insurance is or is not an appropriate remedy.

We can advance a step towards answering this crucial question by enumerating some of the essential characteristics of any unemployment insurance scheme which seem to follow directly or by necessary implication from the conditions of the problem as here laid down.

1. The scheme must be compulsory; otherwise the bad personal risks against which we must always be on our guard would be certain to predominate.

2. The scheme must be contributory, for only by exacting rigorously as a necessary qualification for benefit that a sufficient number of weeks' contribution shall have been paid by each recipient can we possibly hope to put limits on the exceptionally bad risks.

3. With the same object in view there must be a maximum limit to the amount of benefit which can be drawn, both absolutely and in relation to the amount of contribution paid; or, in other words, we must in some way or other

secure that the number of weeks for which a workman contributes should bear some relation to his claim upon the fund. Armed with this double weapon of a maximum limit to benefit and of a minimum contribution, the operation of the scheme itself will automatically exclude the loafer.

4. The scheme must avoid encouraging unemployment, and for this purpose it is essential that the rate of unemployment benefit payable shall be relatively low. It would be fatal to any scheme to offer compensation for unemployment at a rate approximating to that of ordinary wages.

5. For the same reason it is essential to enlist the interest of all those engaged in the insured trades, whether as employers or as workmen, in reducing unemployment, by associating them with the scheme both as regards contribution and management.

6. As it appears on examination that some trades are more suitable to be dealt with by insurance than others, either because the unemployment in these trades contains a large insurable element, or because it takes the form of total discharge rather than short time, or for other reasons, it follows that, for the scheme to have the best chance of success, it should be based upon the trade group, and should at the outset be partial in operation.

7. The group of trades to which the scheme is to be applied must, however, be a large one, and must extend throughout the United Kingdom, as it is essential that industrial mobility as between occupations and districts should not be unduly checked.

8. A State subvention and guarantee will be necessary, in addition to contributions from the trades affected, in order to give the necessary stability and security, and also in order to justify the amount of State control that will be necessary.

9. The scheme must aim at encouraging the regular employer and workman, and discriminating against casual engagements. Otherwise it will be subject to the criticism of placing an undue burden on the regular for the benefit of the irregular members of the trade.

10. The scheme must not act as a discouragement to voluntary provision for unemployment, and for that purpose some well-devised plan of co-operation is essential between the State organisation and the voluntary associations which at present provide unemployment benefit for their members.

Our analysis, therefore, leads us step by step to the contemplation of a national contributory scheme of insurance universal in its operation within the limits of a large group of trades—a group so far as possible self-contained and carefully selected as favourable for the experiment, the funds being derived from compulsory contributions from all those engaged in these trades, with a subsidy and guarantee from the State, and the rules relating to benefit being so devised as to discriminate effectively against unemployment which is mainly due to personal defects, while giving a substantial allowance to those whose unemployment results from industrial causes beyond the control of the individual.

Is such a scheme practicable?

This is a question partly actuarial, partly administrative, and partly political. and it is, of course, quite impossible to discuss it adequately on an occasion such as this.

I may, however, say that so far as can be judged from such data as exist (and those data are admittedly imperfect and rest on a somewhat narrow basis), a scheme framed on the lines I have indicated is actuarially possible, at least for such a group of trades as building, engineering, and shipbuilding—that is to say, a reasonable scale of contributions will yield benefits substantial in amount and of sufficient duration to cover the bulk of the unemployment ordinarily met with in these trades.

The administrative difficulties of such a scheme are, of course, great, but none of these difficulties is, I think, insuperable if there be a general desire that the experiment should be made. Certainly the experience of the few foreign schemes which have broken down creates no presumption against success, for the failures have been quite clearly attributable to causes which would not operate in the case of a national scheme such as is now under discussion, especially if it were worked, as it naturally would be, in close connection with the new Labour Exchanges.

Perhaps the most difficult administrative problem would be the adjustment of the scheme, so that while its benefits are not confined to workmen for whom provision is made by voluntary associations, it would yet operate so as to encourage the work of these associations, and not to undermine and destroy them, either by competition or detailed control. The problem, however, though difficult, is one for which a solution can assuredly be found if it be the general desire that a scheme shall be brought into operation.

The remaining question is one of high policy. What importance do we as a nation attach to the policy of promoting industrial security by collective action? And what sacrifices are those interested prepared to make for such an object, and, in particular, to minimise the irregularity of working-class incomes so far as affected by irregular demand for labour? The final answer will depend not only on the general view taken of the relations of the individual and the State, and of the scope and limits of political action, but also on the relative weight attached to this particular object as compared with other objects which also have claims on public funds and energy.

British Association for the Advancement of Sci

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ADDRESS TO THE ENGINEERING SECTION BY PROFESSOR W. E. DALBY, M.A., M.INST.C.E., PRESIDENT OF THE SECTION.

British Railways: Some Facts and a Few Problems.

It is remarkable how few among us really realise the large part that railways play in our national life. How many of us realise that the capital invested in the railway companies of the United Kingdom is nearly twice the amount of the national debt; that the gross income of the railway companies is within measurable distance of the national income; that to produce this income every inhabitant of the British Islands would have to pay annually 3*l.* per head; that they employ

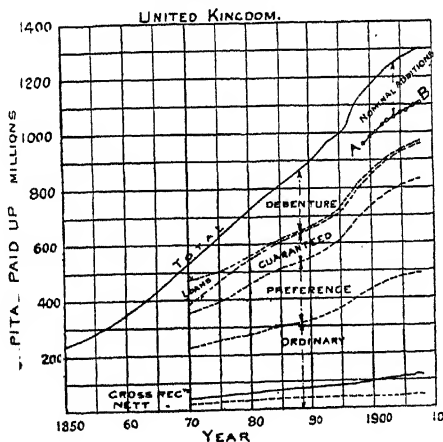


FIG. 1.

over six hundred thousand people; and that about eight million tons of coal are burnt annually in the fire-boxes of their locomotives? I hope to place before you in the short time which can be devoted to a presidential address a few facts concerning this great asset of our national life and some problems connected with the recent developments of railway working—problems brought into existence by the steady progress of scientific discovery and the endeavour to apply the

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new discoveries to improve the service and to increase the comfort of the travelling public.

A great deal of interesting information is to be found in the Railway Returns issued by the Board of Trade. I have plotted some of the figures given, in order to show generally the progress which has been made through the years, and at the same time to exhibit the rates of change of various quantities in comparison with one another.

Consider in the first place what the railways have cost the nation. This is represented financially at any instant by the paid-up capital of the companies. The total paid-up capital in 1850 was 240 millions sterling. In 1908 this amount had increased to 1,810 millions. The curve marked 'Total' in fig. 1 shows the total paid-up capital plotted against the year. It will be noticed that the increase per annum is remarkably regular up to about 1896 and is at the rate of not quite 100 millions per annum. After this date the capital increases at a somewhat greater rate, but in 1900 the rate drops with a tendency to a gradually decreasing value. Part of the increase immediately after 1896 is, however, due

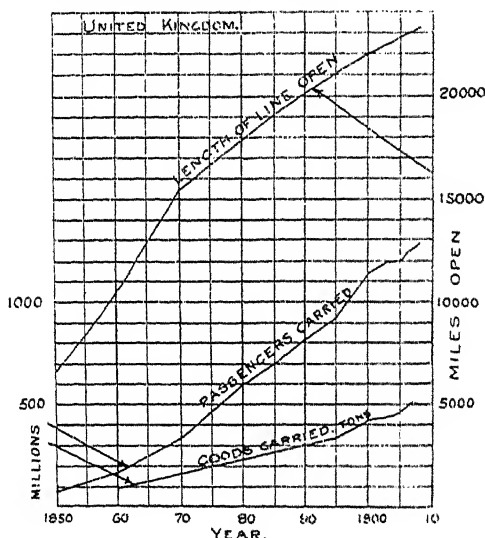


FIG. 2.

to nominal additions to the capital. The extent to which this process of watering the stock has been carried is indicated over the period 1898 to 1908 by the curve A B. In the year 1908 the nominal additions to capital amounted to 196 millions of pounds.

Curves are also plotted showing the amounts of the different kinds of stock making up the total. It will be noticed that the ordinary stock is a little over one-third of the total paid-up capital in 1908—viz., 38 per cent. In 1870 it was about 43 per cent.

The lower curve on the diagram shows the gross receipts, which amounted to 120 millions of pounds in 1908. The dotted line indicates the net revenue after deducting from the total receipts the working expenditure. This, for 1908, was 43½ millions, corresponding to 3·32 per cent. of the total paid-up capital. If the net receipts are reckoned as a percentage of the paid-up capital after deducting the nominal additions the return is increased to 3·9 per cent. These figures practically represent the average dividend reckoned in the two ways for the year 1908.

PRESIDENTIAL ADDRESS.

Fig. 2 shows by the upper curve the number of miles open for traffic plotted against the year. This curve indicates great activity of construction during the period 1850 to 1870, with a regular but gradually decreasing addition of mileage from year to year afterwards.

At the end of 1908 there were 23,205 miles open, corresponding to 53,669 miles of single track, including sidings. Of this, 85 per cent. was standard 4 feet 8½ inches gauge, 12·3 per cent. 5 feet 3 inches, and 2·2 per cent. 3 feet gauge. The remainder was made up of small mileages of 1 foot 11½ inches, 2 feet 3 inches, 2 feet 4 inches, 2 feet 4½ inches, 2 feet, 2 feet 9 inches, 4 feet, and 4 feet 6 inches gauges.

The two lower lines of the diagram show respectively the number of passengers carried and the tons of goods carried from year to year.

The curves of mileage, passengers carried, and goods carried increase regularly with the increase of capital, indicating that up to the present time the possibility of remunerative return on capital invested in railway enterprise in this country is not exhausted. It is true that there is a maximum of goods carried in the year 1907; but the sudden drop in the curve between the years 1907 and 1908 suggests

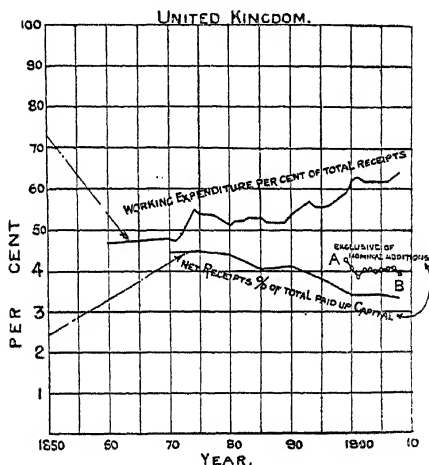


FIG. 3.

that the drop is only of a temporary character, and there is every reason to believe that the curve will resume its upward tendency with time. In 1908 the railways of the United Kingdom carried 1,278 millions of passengers, exclusive of season-ticket holders, and 491 million tons of goods; the quantity of goods carried in 1907 was nearly 515 millions of tons. It is curious that very approximately the companies carry per annum one passenger and about 0·4 ton of goods for every pound sterling of paid-up capital.

The proportion of the gross receipts absorbed in carrying out this service is shown by the upper curve of fig. 3. The proportion has increased, on the whole regularly, from 47 per cent. in 1860 to 64 per cent. in 1908.

The lower curve shows the net receipts as a percentage of the paid-up capital. From 1899 onwards the curve A B shows the net receipts reckoned on the paid-up capital exclusive of the nominal additions. It will be observed that the net receipts have not declined more than half a per cent. since 1870, notwithstanding the increase in working expenditure.

Fig. 4 indicates the cost of working the traffic calculated in terms of the train-mile, no data being available regarding the actual work done as represented by

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the ton-mile or the passenger-mile. In some respects the train-mile is the fairest way of comparing costs, because when a train is running, whether it is full or empty, the same service must be performed by the majority of the departments.

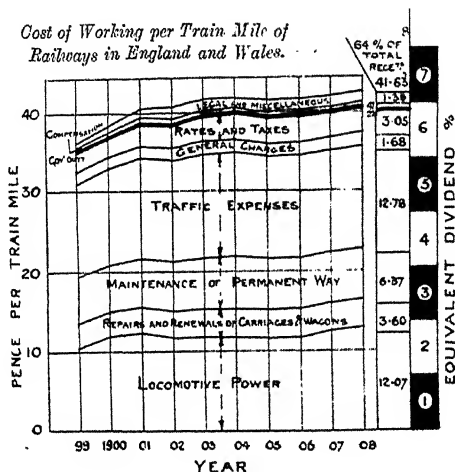


FIG. 4.

The curves bring out clearly that the proportion of the total expenditure per train-mile absorbed by these several services remains fairly constant over a series of years. To the right is exhibited the average for the four years 1905 to 1908. The figures are also reproduced in the following table:—

TABLE I.
Average Working Costs per Train-mile of the Railways in England and Wales taken over the Years 1905 to 1908.

	Pence per train-mile.
Locomotive power	12.07
Repairs and renewals of carriages and waggon	3.60
Maintenance of permanent-way	6.37
Traffic expenses	12.78
General charges	1.68
Rates and taxes	3.05
Government duty	0.22
Compensation	0.47
Legal and miscellaneous	1.39
Total	41.63

Locomotive power absorbs an amount about equal to the traffic expenses; and companies actually pay in rates and taxes a sum nearly equal to the whole amount required to maintain the rolling-stock in an efficient state.

To the right is shown a scale the divisions of which represent an amount estimated in pence per train-mile corresponding to 1 per cent. of the average dividend. This shows that if the whole of the locomotive power could be obtained for nothing, the average dividend would only be increased by 1½ per

cent. Reckoned on the ordinary stock alone, however, the increase would be about three times this amount.

It may be of interest at this stage to compare the financial position and the cost of the working of railways in their earlier days with the state of things now. For this purpose the position of the old London and Birmingham Railway is compared with the position of the London and North Western Railway, the system into which it has grown. The years selected are 1840 and 1908.

I have taken out the cost per mile of working the traffic of the London and Birmingham Railway from some accounts given in Winshaw's 'Railways.' The details are grouped somewhat differently in the list just given, but in the main the various items may be compared.

The number of train-miles on the London and Birmingham Railway recorded for the year January to December, 1839, is 714,998. The accounts given are for the year June 1839 to June 1840. The mileage record is thus not strictly comparable with the expense account, but it may be regarded as covering the same period with sufficient accuracy for our purpose.

The costs work out as follows :—

TABLE II.

Cost per Train-mile for the Year ending June 1840, London and Birmingham Railway.

	Pence per mile.
Locomotive power	23·2
Maintenance of way	27·2
Traffic expenses, including repairs to waggons	25·9
General charges, including legal charges	4·5
Rates and taxes	4·5
Government duty	7·65
Accident account	0·35
Total	93·80

The receipts amounted to 231*d.* per train-mile. Hence the working expenditure was 40 per cent. of the gross receipts.

The gross receipts for the year ending June 30, 1840, were 687,104*l.*, which, after deducting charges for loans, rents, and depreciation of locomotives, carriages, and waggons, enabled a dividend of 9½ per cent. to be paid on the ordinary stock.

There are two noteworthy facts in these old accounts. First, the allowance for depreciation on the rolling-stock of nearly 4 per cent. of the receipts. Secondly, the fact that the cost of working the traffic is given per ton-mile. This method of estimating the cost of working has gradually fallen into desuetude on British railways. One company only at the present time records ton-mile statistics. Quite recently (in 1909) the committee appointed by the Board of Trade to make inquiries with reference to the form and scope of the accounts and statistical returns rendered by the railway companies under the Railway Regulation Acts have had the question of ton-mile and passenger-mile statistics under consideration. There was considerable difference of opinion concerning the matter, and in the end the committee did not recommend that the return of ton-mile and passenger-mile statistics should be made compulsory on the railway companies.

Returning to the London and Birmingham Railway accounts, the actual figures given by Mr. Bury, the locomotive engineer, were, for the year ending December 1839 :—

Passenger Trains.—Ton-miles, 21,159,796, giving an average of 542,533 ton-miles per engine at 0·86 lb. of coke per ton-mile costing 0·17*d.*

Goods Trains.—17,527,439 ton-miles, giving an average of 584,247 per engine at 0·57 lb. of coke per ton-mile costing 0·11*d.* per ton-mile.

Table III. shows various amounts and quantities in comparison with one another. Beneath the actual figures are placed proportional figures, the London and Birmingham item being in every case denoted by unity.

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TABLE III.

Comparison of Capital, Receipts, Miles Open, Train-miles, and Cost of Working between the London and Birmingham Railway for the Year ending June 1840 and the London and North Western Railway for the Year ending December 1908.

Stock and Sh Capital		Loans and Debentures		Total	Gross Receipts	
		Interest per cent.	£	Interest per cent.	£	
L. & B. Ry., 1840	3,125,000		2,125,000		5,250,000	687,000
L. & N. W. Ry., 1908	85,861,760	app. ave- rage on all types of stock	39,175,374	3 average	125,037,134	15,515,334
L. & B. Ry., 1840				1	1	
L. & N. W. Ry., 1908				24	22.6	
	Miles Open in Equivalent Single Track	Train-miles Run	Receipts per Train-mile	Cost of Working per Train-mile	Expenditure to Gross Receipts per cent.	
L. & B. Ry., 1840	250	714,998		93 pence	40	
L. & N. W. Ry., 1908	5,406	48,732,644		50 „	65	
L. & B. Ry., 1840	1	1	1	1	1	
L. & N. W. Ry., 1908	21.6	68.3	0.33	0.54	1.62	

The comparison brings out some curious facts. For instance, it will be noticed that the gross receipts of the London and North Western Railway in 1908 were twenty-two and a half times as much as those of the London and Birmingham Railway in 1840, and that the track mileage open was about twenty-two times as great. The money earned per mile of track open is thus practically the same after a lapse of seventy years. To earn the same amount per mile of track open, however, the trains of the London and North Western Railway had in 1908 to run 68.3 times the number of train-miles that the trains of the London and Birmingham Railway ran in 1840. That is to say, in order to earn a sovereign a London and North Western train has now to run three times the distance which it was necessary for a London and Birmingham train to run to earn the same amount.

Another point to notice is that although the mileage and the receipts per mile of track open have each increased in the same proportion, yet the capital has increased at a greater rate, being on the total amount twenty-four times as much as in 1840, and the stock and share capital has increased twenty-eight times. So that with the necessity of running three times the train-mileage to obtain the same return per mile of track open there runs the obligation to pay interest on an ordinary stock which has been increased in a greater proportion than the mileage and in a greater proportion than the earning power of the line. Lower dividends are therefore inevitable. The cost of working per train-mile has decreased gradually to about half its value in 1840, but at the same time the receipts per train-mile have dwindled to one-third of the amount in

These figures show that a more conservative system of financing the railways might have been adopted in the earlier days with advantage. If when the receipts per train-mile were larger, a proportion of the revenue had been used annually for the construction of new works and for the provision of new rolling-stock instead of raising fresh capital for everything in the nature of an addition to the railway, the companies would to-day have been in a position to regard with equanimity the increasing cost of working.

It is too late in the day to recover such a strong financial position, but even now on many lines a larger proportion of the revenue could be sunk in the line with great ultimate advantage to the financial position.

The Problem of the Locomotive Department.

During the last twenty years the demand on the locomotive has steadily increased. The demand has been met, though with increasing difficulty, owing to the constructive limitations imposed by the gauge. The transference of a train from one place to another requires that work should be done continuously by the locomotive against the tractive resistance. The size of the locomotive is determined by the rate at which this work is to be done. If T represents the tractive resistance at any instant, and V the speed of the train, then the rate at which work is done is expressed by the product TV . The pull exerted by the locomotive must never be less than the resistance of the whole train considered as a dead load on the worst gradient and curve combination on the road, and it can never be greater than about one-quarter of the total weight on the coupled wheels of the engine.

Again, the tractive pull of the engine may be analysed into two parts—one the pull exerted to increase the speed of the train, the other the pull required to maintain the speed when once it has been reached. For an express train the number of seconds required to attain the journey speed is so small a fraction of the total time interval between the stops that the question of acceleration is not one of much importance. But for a local service where stops are frequent the time required to attain the journey-speed from rest is so large a fraction of the time between stops that this consideration dominates the design of the locomotive and, in fact, makes it desirable to substitute the electric motor for the locomotive in many cases.

An accurate estimate of the rate at which work must be done to run a stated service can only be made if there are given the weight of the vehicles in the train, the weight of the engine, the kind of stock composing the train, the speed and acceleration required at each point of the journey and a section of the road; and, in addition to this, allowance must be made for weather conditions.

A general idea of the problem can, however, be obtained by omitting the consideration of acceleration, gradients, and the unknown factor of weather conditions, considering only the rate at which work must be done to draw a given load at a given speed on the level. Even thus simplified the problem can be solved only approximately, because, although the tractive resistance of a train as a whole is a function of the speed, the tractive resistance per ton of load of the vehicles and per ton of load of the engine differ both in absolute value and in their rates of change for a stated speed, and, further, the ratio between the weight of the vehicles and the weight of the engine is a very variable quantity.

For our purpose, however, it will be sufficiently accurate to assume that the resistance of the whole train, expressed in pounds per ton, is given by the formula

It follows that the horse-power which must be developed at the driving-wheels to maintain a speed of \bar{V} miles per hour on the level with a train weighing W tons is

$$HP = W \left\{ \frac{\bar{V}}{70} + \frac{\bar{V}^3}{96,000} \right\}.$$

Fig. 5 shows curves of horse-power plotted from this equation for various weights of train. From this diagram a glimpse of the problem confronting locomotive engineers at the present day can readily be obtained.

To illustrate the point consider the case of the Scotch express on the West Coast route.¹ This is an historic service and goes away back to 1844, in which year the first train left Euston for Carlisle, travelling by way of Rugby, Leicester, York, and Newcastle, and occupying 15½ hours. It was not until 1847, however, that there was a through service to Edinburgh *via* Berwick.

In September 1848 the West Coast service for Edinburgh was established by way of Birmingham and Carlisle, the timing being 8 hours 55 minutes to Carlisle, and 12 hours to Edinburgh.

In September 1863 the starting time from Euston was fixed at 10 A.M., and in 1875 the train ran *via* the Trent Valley between Rugby and Stafford, thus cutting out Birmingham and shortening the journey to Carlisle from 309 miles to 299 miles, the timing being 7 hours 42 minutes to Carlisle, and 10 hours and 25 minutes to Edinburgh. The speed has gradually been increased, and in 1905

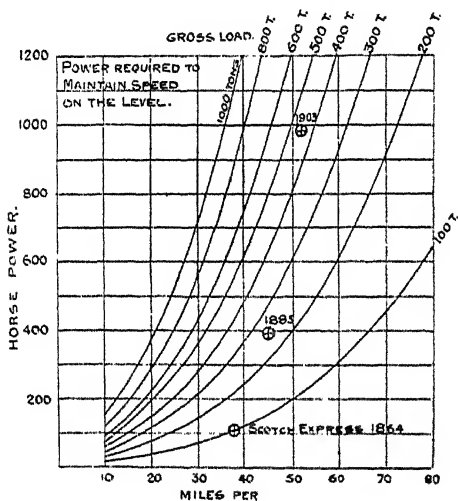


FIG. 5.

the timing was 5 hours 54 minutes to Carlisle, and 8½ hours to Edinburgh. Now the timing is 5 hours 48 minutes to Carlisle, but is still 8½ hours to Edinburgh.

Three specific examples are plotted on the diagram, showing the power requirements in 1864, 1885, and 1903 for this train. Typical trains in 1864, 1885, and 1903 weighed, including engine and tender, 100 tons, 250 tons, and 450 tons respectively. The average speeds were thirty-eight, forty-five, and fifty-two miles per hour respectively. A glance at the diagram will show that the power required to work this train was about 100 horse-power in 1864, 400 horse-power in 1885, and 1,000 horse-power in 1903.

It must not be supposed that the increase in the weight of the train means a proportionate increase in the paying load. Far from it. On a particular day in 1903, when the total weight of the Scotch express was 450 tons approximately, the weight of the vehicles was about 346 tons. There were two dining-cars on the train, and the seating accommodation, exclusive of the seats in the dining-

¹ I am indebted to Mr. Bowen Cooke for particulars of the Scotch Express Service.

cars, was for 247 passengers, giving an average of 1.4 tons of dead load to be hauled by the engine per passenger assuming the train to be full. In the days before corridor stock and dining-cars were invented the dead load to be hauled was about a quarter of a ton per passenger for a full train.

In a particular boat special, consisting of two first-class saloons, one second and third class vehicles, one first-class dining-car, one second and third class dining-car, one kitchen-car, and two brake-vans, seating accommodation was provided, exclusive of the dining-cars, for 104 passengers, and the dead load to be hauled averaged 2.72 tons per passenger. Notwithstanding this increase in the dead load of luxurious accommodation, the fares are now less than in former days on corresponding services. Similar developments have taken place in almost every important service, and new express services are all characterised by heavy trains and high speeds.

Characteristic Energy-curves of Steam Locomotives.

This steadily increasing demand for power necessarily directs attention to the problem, What is the maximum power which can be obtained from a locomotive within the limits of the construction-gauge obtaining on British railways? The answer to this can be found without much ambiguity from a diagram which I have devised consisting of a set of typical characteristic energy-curves to represent the transference and transformation of energy in a steam locomotive, an example of which is given in fig. 6. While examining the records of a large

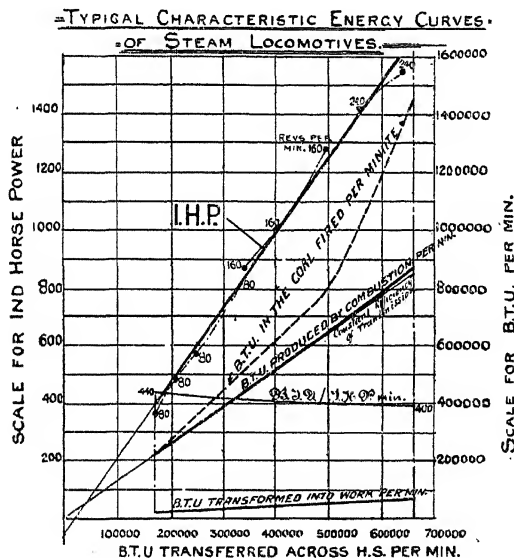


FIG. 6.

number of locomotive trials, I discovered that if the indicated horse-power be plotted against the rate at which heat energy is transferred across the boiler-heating surface the points fall within a straight-line region, providing that the regulator is always full open and that the power is regulated by means of the reversing-lever—that is to say, by varying the cut-off in the cylinders. It is assumed at the same time, of course, that the boiler-pressure is maintained con-

stant. I have recently drawn a series of characteristic energy-curves for particular engines, and these are published in *Engineering*, August 19 and 26, 1910. A typical set is shown in fig. 6.

The horizontal scale represents the number of British thermal units transferred across the boiler-heating surface per minute. This quantity is used as an independent variable. Plotted vertically are corresponding horse-powers, each experiment being shown by a black dot on the diagram. The small figures against the dots denote the speed in revolutions of the crank-axle per minute. Experiments at the same speed are linked by a faint chain dotted line. A glance at the diagram will show at once how nearly all the experiments fall on a straight line, notwithstanding the wide range of speed and power.

The ordinates of the dotted curve just below the i.h.p. curve represent the heat energy in the coal shovelled per minute into the fire-box—that is, the rate at which energy is supplied to the locomotive. The thick line immediately beneath it represents the energy produced by combustion. The vertical distance between these two curves represents energy unproduced, but energy which might have been produced under more favourable conditions of combustion. Some of the unproduced energy passes out of the chimney-top in carbon monoxide gas, but the greater proportion is found in the partially consumed particles of fuel thrown out at the chimney-top in consequence of the fierce draught which must be used to burn the coal in sufficient quantity to produce energy at the rate required. The rate of combustion is measured by the number of pounds of fuel burnt per square foot of grate per hour. In land practice with natural draft 20 lb. of coal per square foot of grate per hour is a maximum rate. In a locomotive the rate sometimes reaches 150 lb. per square foot per hour. In the diagram shown the maximum rate is about 120 lb. per square foot, and the dotted curve begins to turn upwards at about 70 lb. per square foot per hour. The vertical distance between the curves shows what has to be paid for high rates of combustion.

I found that in almost every case the curve representing the energy actually produced by combustion differed very little from a straight line, passing through the origin, showing that at all rates of working the efficiency of transmission is approximately constant. That is to say, the proportion of the heat energy actually produced by combustion in the fire-box which passes across the boiler-heating surface per minute is nearly constant and is therefore independent of the rate of working.

The lowest curve on the diagram represents the rate at which heat energy is transformed into mechanical energy in the cylinders of the locomotive. It seems a small rate in proportion to the rate at which heat energy is supplied to the fire-box, but it is not really so bad as it looks, because the engine actually transformed 60 per cent. of the energy which would have been transformed by a perfect engine working on the Rankine cycle between the same limits of pressure. The engine efficiency is represented in a familiar way by a curve labelled 'B.T.H. per i.h.p. minute.' It will be seen that the change of efficiency is small, notwithstanding large changes in the indicated horse-power.

The diagram indicates that the indicated horse-power is practically proportional to the rate at which heat is transferred across the boiler heating-surface, and as this is again proportional to the extent of the heating-surface, the limit of economical power is reached when the dimensions of the boiler have reached the limits of the construction-gauge, the boiler being provided with a fire-grate of such size that, at maximum rate of working, the rate of combustion falls between 70 and 100 lb. of coal per square foot of grate per hour. A boiler of large heating-surface may be made with a small grate necessitating a high rate of combustion to obtain the required rate of heat-production. Then, although a large power may be obtained, it will not be obtained economically.

Returning now to the consideration of the type of locomotive required for a local service with frequent stops, the problem is to provide an engine which will get into its stride in the least time consistent with the comfort of the passengers. The average speed of a locomotive on local service is low. The greater part of the time is occupied in reaching the journey speed, and the brake must then often be applied for a stop a few moments after the speed has been attained. In some cases the stations are so close together that there is no period between acceleration and retardation. Without going into the details of the calculation I

may say that to start from rest a train weighing, including the engine, 300 tons, and to attain a speed of thirty miles per hour in thirty seconds requires about 1,350 i.h.p. During the period of acceleration the engine must exert an average tractive pull of nearly fifteen tons.

Mr. James Holden, until recently locomotive engineer of the Great Eastern Railway, built an engine to produce an acceleration of thirty miles per hour in thirty seconds with a gross load of 300 tons. The engine weighed 78 tons, and was supported on ten coupled wheels each 4 feet 6 inches diameter. There were three high-pressure cylinders, each 18½ inches diameter and 24 inches stroke. A boiler was provided with 3,000 square feet of heating surface and a grate of 42 square feet area. Boiler pressure, 200 pounds per square inch. This engine practically reached the limit of the construction-gauge.

An acceleration of thirty miles per hour in thirty seconds is considerably below what may be applied to a passenger without fear of complaint. But it is clear that it is just about as much as a locomotive can do with a train of reasonable weight. Even with a gross load of 300 tons nearly one-third of it is concentrated in the locomotive, leaving only 200 tons to carry paying load. The problem of quick acceleration cannot therefore be properly solved by means of a steam locomotive. But with electric traction the limitations imposed on the locomotive by the construction-gauge and by the strength of the permanent way are swept away.

The equivalent of the boiler power of a dozen locomotives can be instantaneously applied to the wheels of the electric train, and every axle in the train may become a driving axle. Thus the whole weight of the stock, including the paying load, may be utilised for tractive purposes. If, for instance, the train weighed 200 tons, then a tractive force equal to one-fifth of this, namely, forty tons, could be exerted on the train, but uniformly distributed between the several wheels, before slipping took place. The problem of quick acceleration is therefore completely solved by the electric motor.

Electric Railways.

December 18, 1890, is memorable in the history of railway enterprise in this country, for on that date the City and South London Railway was opened for traffic, and the trains were worked entirely by electricity, although the original intention was to use the endless cable system of haulage. This line inaugurated a wonderful system of traction on railways in which independent trains moving at different speeds at different parts of the line are all connected by a subtle electric link to the furnaces of one central station.

Since that epoch-marking year electric traction on the railways of this country has made a gradual if somewhat slower extension than anticipated. But electrically operated trains have in one branch of railway working beaten the steam locomotive out of the field and now reign supreme—that is, in cases, as indicated above, where a quick frequent service is required over a somewhat short length of road. The superiority of the motor over the steam locomotive, apart from questions of cleanliness, convenience, and comfort, lies in the fact that more power can be conveyed to the train and can be utilised by the motors for the purpose of acceleration than could possibly be supplied by the largest locomotive which could be constructed within the limits of the construction-gauge. There are many other considerations, but this one is fundamental and determines the issue in many cases.

A few facts relating to the present state of electric railways in the United Kingdom may prove of interest. At the end of 1908 there were in the United Kingdom 204 miles of equivalent single track worked solely by electricity and 200 miles worked mainly by electricity, corresponding to 138 miles of line open for traffic. Of this 102 miles belong to the tube railways of London and 201 miles to the older system formed by the District and the Metropolitan Railways and their extensions.

It is not an easy matter to ascertain exactly how much capital is invested in these undertakings for the purpose of electric working alone, since some of the lines originally constructed for a steam locomotive service have been converted to electric working. On the converted lines there is the dead weight of capital

corresponding to the locomotive power provided before electrification took place. The capital invested in the 102 miles of tube railways in London is a little over 25,000,000*l*.

The total number of passengers carried (exclusive of season tickets) on the 138 miles of electrical track during the year 1908 was nearly 342 millions, being roughly one-third of the total number of passengers carried on all the railways of England and Wales during the same period.

The average cost of working this traffic is 22*3d*. per train-mile. This figure includes the service of the lifts, which is presumably returned with the traffic expenses. The charges work out in this way :—

TABLE IV.

Average Working Cost per Train-mile of the Electric Railways worked wholly or mainly by Electricity in England and Wales for the Year 1908.

	Pence per train-mile.
Locomotive power	8'40
Repairs and renewals of carriages and waggons	1'50
Maintenance of permanent way	2'40
Traffic expenses	5'22
General charges	1'52
Rates and taxes	2'36
Government duty	0'088
Compensation	0'116
Legal and miscellaneous	0'75
Total	22'35

The corresponding total receipts were 38'65*d*. per train-mile. The working expenses are thus 58 per cent. of the total receipts. Comparing this with the figures given above for the whole of the lines in England and Wales, it will be seen that the cost for locomotive power on the electric railways appears to be about two-thirds of the cost on steam lines per mile run, the cost for repairs and renewals of carriages and waggons about one-half, and the cost for traffic expenses about one-half.

The two kinds of working are not, however, strictly comparable, as all the conditions of traffic in the two cases are different and the length of the electric lines is relatively so small that the problems which arise out of the transmission of electric power over long distances are excluded. The traffic expenses and the cost of repairs and renewals of carriages and waggons, general charges, etc., are practically independent of the kind of power used for locomotive purposes, and, moreover, the difference in weight of electric trains and the steam-hauled trains is on the average so great that no comparison can be instituted without ton-mile statistics.¹

Method of Working.

With two exceptions the method of working the electrified lines of this country is in the main the same. A third conductor rail is laid on insulators fixed to the ordinary track sleepers, and is maintained throughout the whole of its length at as nearly as possible a pressure of 600 volts, except in a few cases where the pressure is 500 or 550 volts. Collecting shoes sliding along the rails are fixed to the trains, and through them current is supplied to the armatures fixed to or geared with the axles. The current flows through the armatures back to the stations

¹ Much valuable information regarding the cost of converting the line between Liverpool and Southport from steam to electric working will be found in Mr. Aspinall's presidential address to the Institution of Mechanical Engineers.

or sub-stations through the running rails, which are bonded for the purpose, or sometimes through a fourth rail carried on insulators fixed to the track sleepers as in the cases of the District and Metropolitan Railways.

Differences in the equipment arise out of the geographical necessities of the distribution. For a short line the power is produced at a central station and is distributed by feeders to the conductor rail direct. For longer lines power is produced at higher voltage (11,000 volts in the case of the District Railway), and is then distributed to sub-stations conveniently placed along the line where it is transformed to a lower voltage, converted to direct current, and then by means of feeders is distributed at 600 volts or thereabouts to the third rail.

In 1908 the Midland Railway Company opened for traffic the electrified line connecting Lancaster, Morecambe, and Heysham. The method of electrification was a departure from the general direct current practice hitherto applied to electrified lines in this country. Power was supplied to the trains at 6,600 volts, single phase, at twenty-five alternations per second, along an overhead conductor. The pressure was reduced by transformers carried on the motor-coach itself, and was then used by single-phase motors. The traffic conditions on this line are simple.

In December 1909 the electrified portion of the London, Brighton, and South Coast Railway from Victoria, round by Denmark Hill to London Bridge was opened for traffic. This work marks an epoch in the history of electric traction in England. For the first time the single-phase system was applied to meet the exacting traffic conditions of a London suburban service where the main condition is that the trains should be accelerated rapidly. The system has shown that it can meet all the conditions of the service perfectly. Energy is purchased and is distributed by overhead conductors direct to the trains at 6,600 volts, single phase, at twenty-five alternations per second where it is used by the single-phase motors after suitable transformation by apparatus carried under the motor carriage. The results of this electrification will be of unusual interest because not only has the method applied shown itself to be quite suitable for dealing with a stopping traffic where quick acceleration is the dominating condition, but it contains the germ of practicable long-distance electrification. The near future may see the extension of the system to the line between London and Brighton, giving a frequent non-stop service which would bring Brighton in point of time nearer than the suburbs on opposite sides of London are to one another.

Some particulars of the electric railways of the country are given in Table V. More details will be found in a table published annually by the *Electrician* entitled 'Tables of Electric Lighting, Power, and Traction Undertakings of the United Kingdom' and in the Railway Returns of the Board of Trade.

Power Signalling.

During the last ten years a considerable number of trial installations of power-signalling apparatus have been made by the railway companies of this country. The electric lines have generally adopted power signalling, and the District Railway has installed a complete system on all its lines and branches.

The term 'power signalling' is applied to any equipment in which the actual movements of the points and signals are done by power, the signalman's work being thus reduced to the movement of small light control levers or switches. Of the several systems tried and proposed three bulk largest in the equipments applied in this country, namely, the all-electric, the low-pressure pneumatic, and the electro-pneumatic systems.

The 'all-electric' system is represented by installations of the McKenzie-Holland and Westinghouse system on the Metropolitan and Great Western Railways, by installations of the 'Crewe' system on the London and North Western Railway, and by installations of Siemens Brothers on the Great Western Railway. The general feature of the all-electric system is that the points are operated by motors sunk in a pit by the side of the rails, the signals are pulled off electrically, and all the apparatus is controlled electrically.

The low-pressure pneumatic system is represented by installations on the London and South Western Railway and the Great Central Railway. The points

and signal arms are moved by air compressed to about 20 lb. per square inch, and led to cylinders connected to the points and to the signal-arms. The control is also done by means of compressed air, small pipes leading from each air-cylinder to the cabin.

The electro-pneumatic system has found most favour in this country up to the present time. The equipment installed includes such notable stations as the Central at Newcastle with 494 levers, and the Glasgow Central with 374 levers, and the whole of the Metropolitan District system of underground railways. In this system an air-cylinder is connected to each set of points and to each signal-arm. Air compressed to 65 lb. per square inch is supplied to the cylinders from a main running alongside the railway kept charged by small air-compressors placed at convenient intervals. Each air-cylinder is provided with a small three-way air-valve operated by an electro-magnet. The movement of each air-valve is controlled electrically from the cabin through the electro-magnet associated with it. The system grouped round any one signal-cabin may be regarded as an engine fitted with a large number of cylinders, each working intermittently by compressed air, and where in each the valve-rod has been changed to an electric cable, all the cables being led to a signal-cabin where the operation of the valves is done by means of an apparatus which is as easily played upon as a piano, with this difference, however, that the notes are mechanically interlocked so that a signalman cannot play any tune he pleases, but only a tune which permits of safe traffic movement. Moreover, the instrument is so arranged that the movement of the small lever determining the movement of a signal-arm cannot be completed unless the signal-arm actually responds to the intention of the signalman, thus detecting any fault in the connections between the box and the arm.

The obvious advantage of power signalling is the large reduction of physical labour required from the signalman. His energy can be utilised in thinking about the traffic movements rather than in hauling all day at signal levers. One man at a power frame can do the work of three at the ordinary frame. The claims made for power signalling, in addition to the obvious advantage of the reduction of labour, are briefly that the volume of traffic which can be dealt with is largely increased, that the area of ground required for the installation is considerably less than with the ordinary system, with its rodding, bell-crank levers, chains, and pulleys, and that where the conditions are such that power signalling is justified the maintenance cost is less than with a corresponding system of normal equipment.

Automatic Signalling.

Several of the power-signalling installations are automatic in the sense that between signal-cabins on stretches of line where there are no junctions or crossover roads requiring the movement of points, the movement of the signal-arm protecting a section is determined by the passage of the train itself. The most important equipment of this kind is that installed on the group of railways forming the 'Underground' system. This includes the District Railway with all its branches. On this line the particular system installed is the electro-pneumatic, modified to be automatic except at junctions. Signal-cabins are placed only at junctions and at places where points require to be operated. The stretch of line to be automatically signalled is divided into sections, and the entrance to each section is guarded by a signal-post. Calling two successive sections A and B, the train as it passes from Section A to Section B must automatically put the signal at the entrance to B to danger, and at the same time must pull off the signal at the entrance to A. These operations require the normal position of the signal-arm to be 'off' instead of at danger, as in the usual practice. The position of the arm in this system conveys a direct message to the driver. If 'on' he knows that there is a train in the section; if 'off' he knows that the section is clear. Each signal-arm is operated by an air motor as briefly described above, but the cables from the valves are now led to relays at the beginning and end of the section which the signal protects. The contrivance by means of which the train acts as its own signalman is briefly as follows. One rail of the running track is bonded, and is connected to the positive pole of a battery or generator. The opposite rail is divided into sections.

each about 300 yards long, bonded, but insulated at each end from the rails of the adjacent sections and each section is connected to a common negative main through a resistance. A relay is placed at the beginning and at the end of each section, and is connected across from the positive to the negative rail. Current flows and energises the relay, in which condition the relay completes a circuit to the electro-magnet operating the admission-valve of the air-cylinder on the signal-post, air is admitted, and the signal-arm is held off. This is the normal condition at each end of the circuit. When a train enters a section it short-circuits the relays through the wheels and axles, in consequence of which the relays, de-energised, break the circuit to the admission-valve, which closes, and allows the air in the cylinder to escape, and the signal-arm, moved by gravity alone, assumes the 'on' or danger position. At the same time the short circuit is removed from the section behind, directly the train leaves it, the relays are at once energised, the admission-valve to the air-cylinder on the protecting post of the section is opened, air enters, and the signal is pulled down to the 'off' position.

The speed at which traffic can be operated by this system of power signalling is remarkable. At Earl's Court junction box forty trains an hour can be passed each way—that is eighty per hour—handled by the one signalman in the box. As the train approaches the box both its approach to the section and its destination must be notified to the signalman. When it is remembered that with ordinary signalling, to take an express train for example, a signalman hears some twenty-four beats on the gongs in his box, and sends signals to the front and rear box which give altogether some twenty-four beats on the gongs in these two boxes, forty-eight definite signals in all, for every express train he passes into the section which his signals protect, it will be understood that the system must be profoundly modified to admit such a speed of operation as eighty trains per hour per man. The modification is radical. No gong signals are used at all. There is a small cast-iron box standing opposite the signalman with fifteen small windows in it, each about $1\frac{1}{2}$ inch square. Normally each window frames a white background. A click in the box announces the approach of a train, and a tablet appears in one of the empty windows showing by code the destination of the train. The signalman presses a plug in the box, a click is heard, and a tablet is seen in a precisely similar apparatus in the next box. When the train passes the man presses another plug and the tablet disappears.

Four wires run between the signal-boxes along the railway, and by combining the currents along the four wires in various ways fifteen definite signals can be obtained, a number sufficient for the District traffic. Each of the fifteen combinations is arranged to operate one particular tablet in the box. Current from these four wires is tapped off at intermediate stations and is used to work a train indicator showing the passengers assembled on the platform the destinations of the next three trains. The whole equipment is a triumph of ingenuity and engineering skill, and is a splendid example of the way electricity may be used to improve the railway service quite apart from its main use in connection with the actual driving of the trains.

The facts and problems I have brought before you will, I think, show the important influence that scientific discovery has had upon our railway systems. Scientific discovery and mechanical ingenuity have reduced the cost of locomotive working to a point undreamt of by the pioneer locomotive builders. Electric railways are the direct fruit of the discoveries of Faraday. The safety of the travelling public was enormously increased by the invention of continuous brakes and by the discovery of the electric telegraph, and is greatly increased by the development of modern methods of signalling; and the comfort of travellers is increased by modern methods of train-lighting, train-warming, and the train kitchen. Inventions of a most ingenious character have from time to time been made in order to furnish a steady and ample light in the carriages. The smoothness of travelling on our main lines is evidence of the thought which has been lavished both on the wheel arrangements of the carriages and on the permanent way. Problems in connection with the continuous brake are many and interesting. Some of the problems of modern signalling would have quite baffled the scientific electrician a quarter of a century ago. When engineers endeavour to apply the results of scientific discovery they often find themselves confronted by problems unperceived by the scientist. Together they may find a solution and

thus enlarge the boundaries of knowledge and at the same time confer a practical advantage on the community. The pure scientist, the practical engineer, act and react on one another both to the advantage of pure science and to the advantage of the national welfare. The future success of our railways depends upon the closer application of scientific principle both to the economic and engineering problems involved in their working, some decrease in unprofitable competition with one another, and a more just appreciation on the part of the State of the part railway companies play in our national well-being.

British Association for the Advancement of Science.

SHEFFIELD, 1910.

ADDRESS TO THE ANTHROPOLOGICAL SECTION

BY
W. CROOKE, B.A.,

PRESIDENT OF THE SECTION.

ONE-AND-THIRTY years have passed since the British Association visited this city. At that time anthropology was in the stage of probation and was represented by a branch of the section devoted to biology. Since then its progress in popularity and influence has been continuous, and its claims to be regarded as a science, with aims and capabilities in no way inferior to those of longer growth, are now generally admitted. Its advance in this country is largely due to the distinguished occupant of this chair at our last meeting in Sheffield. During the present year Dr. E. B. Tylor has resigned the professorship of anthropology in the University of Oxford. Before this audience it is unnecessary for me to describe in detail the services which this eminent scholar and thinker has rendered to science. His professorial work at Oxford; his unflinching support of the Royal Anthropological Institute and of this section of the British Association; his sympathetic encouragement of a younger generation of workers—these are familiar to all of us. Many of those now engaged in anthropological work at home and abroad date that interest in the study of man, his culture and beliefs, which have given a new pleasure to their lives, from the time when they first became acquainted with his 'Primitive Culture' and 'Researches into the History of Mankind.' These works enjoy the almost unique distinction that, in spite of the constant accumulation of new material to illustrate an advancing science, they still maintain their authority; and this because they are based on a thorough investigation of all the available material and a profound insight into the psychology of man at the earlier stages of culture. He has laid down once for all the broad principles which must always guide the anthropologist: that a familiarity with the principles of the religions of the lower races is as indispensable to the scientific student of theology as a knowledge of the lower forms of life, the structure of mere invertebrate creatures, is to the physiologist. 'Few,' he assures us, 'who will give their minds to master the general principles of savage religion will ever think it ridiculous or the knowledge of it superfluous

to the rest of mankind. . . . Nowhere are broad views of historical development more needed than in the study of religion. . . . Scepticism and criticism are the very conditions for the attainment of reasonable belief.' I need hardly say that his exposition of the principles of animism, as derived from the subconscious mental phenomena of dreams and waking visions, has given a new impulse and direction to the study of the religion of savage races.

Dr. Tylor, on his retirement from the active work of teaching, carries with him the respectful congratulations and good wishes of the anthropologists here assembled, all of whom join in the hope that the Emeritus Professor may be able to devote some of his well-earned leisure to increasing the series of valuable works for which we are already indebted to him.

In his address from this chair Dr. Tylor remarked that twenty years before that time it was no difficult task to master the available material. 'But now,' he added, 'even the yearly list of new anthropological literature is enough to form a pamphlet, and each capital of Europe has its anthropological society in full work. So far from any finality in anthropological investigation, each new line of argument but opens the way to others behind, while those lines tend as plainly as in the sciences of stricter weight and measure toward the meeting ground of all sciences in the unity of Nature.'

Since these words were written there has been a never-ceasing supply of fresh literature, which is well represented in the publications of the present year. Every contributor to this science must now be a specialist, because he can with advantage occupy only one tiny corner of the field of humanity; and even then he is never free from a feeling of anxiety lest his humble contribution may have been anticipated by some indefatigable foreign scholar. In short, the attempt to give a general exposition of the sciences devoted to the study of mankind has been replaced by the monograph. Of such studies designed to co-ordinate and interpret the facts collected by workers in the field we welcome two contributions of special importance.

Professor J. G. Frazer has given us a monumental treatise on totemism and exogamy, in which, relying largely on new Australian evidence and that collected from Melanesia by Dr. Haddon and his colleagues, Dr. Rivers and Dr. Seligmann, he endeavours to prove that totemism originated in a primitive explanation of the mysteries of conception and childbirth. As contributing causes he discusses the influence of dreams and the theory of the external soul, the latter being occasionally found connected with totemism; and he points out that one function of a totem clan was to provide by methods of mimetic or sympathetic magic a supply of the totem plant or animal on which the existence of the community depends, this function being not metaphysical or based on philanthropic impulse, but on a cool but erroneous calculation of economic interest. He has also cleared the ground by dissociating totemism from exogamy, the latter, as an institution of social life, being, he believes, later in order of time than totemism, and having in some cases accidentally modified the totemic system, while in others it has left that system entirely unaffected. The law of exogamy is, in his opinion, based mainly on a desire to prevent the union of near relations, and on the resulting belief in the sterilising effects of incest upon women in general and edible animals and plants. In dealing with totemism as a factor in the evolution of religion he gives us a much-needed warning that it does not necessarily develop, first into the worship of sacred animals and plants, and afterwards into the cult of anthropomorphic deities with sacred plants and animals for their attributes. In the stage of pure totemism totems are in no sense deities, that is to say, they are not propitiated by prayer and sacrifice; and it is only in Polynesia and Melanesia that there are any indications of a stage of religion evolved from totemism, a conclusion which demolishes much ingenious speculation. It is hardly to be expected that in a field covered by the wrecks of many controversies these views will meet with universal acceptance. But the candour with which he discards many of his own theories, and the infinite labour and learning devoted to the preparation of his elaborate digest, deserve our hearty recognition.

In his treatise on 'Primitive Paternity,' Mr. E. S. Hartland deals with the problems connected with the relations of the sexes in archaic society. Mother-right he finds to be due not so much to the difficulty of identifying the father

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as to ignorance of physiological facts; and he supposes that the transition from mother-right to father-right originated not from a recognition of the physical conditions of paternity, but from considerations connected with the devolution of property; as Professor Frazer states the case, it arises from a general increase in material prosperity leading to the growth of private wealth.

We also record the steady progress of the great 'Encyclopædia of Religion and Ethics,' under the editorship of Dr. J. Hastings, which promises to provide an admirable digest of the results of recent advances in the fields of comparative religion and ethnology.

It is now admitted by all students of classical literature that the material collected from the lower races is an indispensable aid to the interpretation of the myths, beliefs, and culture of the Greeks and Romans. Most of our universities provide instruction of this kind; and Oxford has opened its doors to a special course of lectures dealing with the relation of anthropology to the classics. One of its most learned mythologists, Dr. L. R. Farnell, when about halfway through his treatise on the cults of the Greek states, admitted the increasing value of the science in elucidating the problems on which he was engaged. Even with this well-advised change of method he has left the field of peasant religion, nature-worship, and magic, which must form the starting-points for the next examination of Greek beliefs, practically unworked. The formation of a Roman Society, working in co-operation with and following the methods which have been adopted by the Society for the Promotion of Hellenic Studies, is a fresh indication of the increasing importance of the work upon which we are engaged.

In the field of archæology Dr. A. J. Evans has commenced the publication of the Minoan records, which open up a new chapter in the early history of the Mediterranean. It is now certain that the origin of our alphabet is not to be found, as De Rougé supposed, in the hieratic script of Egypt, but in the Cretan hieroglyphs; and that the influence of the Phœnicians in its development was less important than has been generally supposed. Before the full harvest of these excavations can be reaped we may have to await the discovery of some bilingual document, like the Rosetta Stone, which will solve the mysteries of the Minoan syllabary.

As regards physical anthropology, the validity of the use of the cephalic index, particularly in discriminating the elements of mixed populations, has been questioned. The recent Hunterian lectures delivered by Professor A. Keith, as yet published only in the form of a summary, are designed to place these investigations on a more scientific basis. In particular increased attention is being given to the influence of environment in modifying a structure generally so stable as the human skull. Thus it has been ascertained that the immigrant into our towns, by some process of selection or otherwise, develops a longer and narrower head than the countryman. The recent American Commission, under the presidency of Professor Boas, reports that 'racial and physical characteristics do not survive under the new climate and social environment. . . . Children born even a few years after the arrival of their parents show essential differences as compared with their European parentage. . . . Every part of the body is influenced, even the shape of the skull, which has always been considered to be the most permanent hereditary characteristic.' Similar results appear from a comparison of the American negro with his African ancestor.

I may refer briefly to the work on folk-lore. Though in recent years it has not maintained the importance which it at one time secured in the proceedings of this section, we still regard it as an essential branch of the study of man. The Folk-lore Society, after thirty-two years' useful work, finds that much still remains to be done in these islands to secure a complete record of popular beliefs and traditions, many of which are rapidly disappearing. It has therefore formulated a scheme for more systematic investigation in those districts which have hitherto been neglected. A committee including representatives of the two allied sciences is also engaged on the necessary task of revising and defining the terminology of anthropology and folk-lore.

The materials collected by field workers in various regions of the world, and popular accounts of savage religion, customs, and folk-lore continue to arrive in such increasing numbers that the need of a central bureau for the classification of this mass of facts has become increasingly apparent. It is true

that we have suffered a set-back, it is to be hoped only temporary, in the rejection of an appeal made to the Prime Minister for a grant-in-aid of the Royal Anthropological Institute. But if we persist in urging our claims to official support the establishment of an Imperial Bureau of Ethnology cannot be long deferred.

One result of this accession of fresh knowledge, largely due to improved methods of research, is to modify some of our conceptions of savage psychology. We now understand that side by side with physical uniformity there may be wide differences arising from varieties of race and environment. It is becoming generally recognised that we can no longer evade the difficulty of interpreting beliefs and usages by referring them to that elusive personality, primitive man. Between the embryonic stage of humanity and the present lie vast periods of time; and no methods of investigation open to us at present offer the hope of successfully bridging this gap in the historical record. To use the words of Professor Frazer: 'It is only in a relative sense, by comparison with civilised men, that we may legitimately describe any living race of savages as primitive.' Hence the hypothesis of the unilinear evolution of culture which satisfied an earlier school will no longer bear examination.

Further, not to speak of the artistic endowments of paleolithic man, we find to our surprise that a race like the Australian Arunta, whose lowness in the scale of humanity does not necessarily connote degradation, has worked out with exceptional ability through its tribal council their complex and cumbrous systems of group marriage and totemism. They have developed a cosmogony which postulates the self-existence of the universe; they have reached a belief in reincarnation and transmigration of the soul. So far from their social system being rigid it is readily modified to suit new conditions. They live in peace with neighbouring tribes, and have established the elements of international law. On the moral side, though there is much that is cruel and abhorrent, they are not wanting in kindness, generosity, gratitude. The savage, in short, is not such an unobservant simpleton as some are inclined to suppose; and any interpretation of his beliefs and usages which ignores this fact is certain to be misleading.

This popularisation of our science has not, however, been universally welcomed. It has been urged with much reason that this overabundance of material tends to encourage an unscientific method, particularly the comparison of isolated facts without due regard to the context of culture to which they are organically related. There is much force in this contention; and probably when the work of this generation comes to be critically reviewed we shall be rightly charged with rashly attempting a synthesis of facts not generically related, with reposing too much confidence in evidence collected in a haphazard fashion, and with losing sight of their historical relations in our quest after survivals. Those who have practical experience of work among savage or semi-savage races understand the difficulty of collecting information on subjects outside the range of their material interests. Only a skilled linguist is able to interpret their hazy religious beliefs. We fail to evolve order from what is and always must be chaotic; we fail to discriminate religion from sociology because both are from the savage point of view identical; and generally it is only the by-products of religion, such as demonology, witchcraft, mythology which reward our search. The most dogmatic among us, when they consider the divergent views of Messrs. Spencer and Gillen and Strehlow, may well hesitate to frame theories about the Arunta.

In the next place it has been objected that the scientific side of anthropology is in danger of being submerged by a flood of amateurism. It is only within recent years that a supply of observers trained in scientific methods has become available. Much of the work in India, the dominions, and other parts of the empire has been done by amateurs, that is to say, by officers in the service of the Crown, missionaries, or planters, who understand the languages, manners, and prejudices of the people, but have not received the advantage of scientific training. Some of this work is, in its kind, useful; but there seems reason to believe that inquiries conducted by this agency have almost reached their limit. The existing material may be supplemented and corrected by workers of the same class; but from them no important additions to our knowledge can reasonably be expected.

Criticisms such as these have naturally suggested proposals for improving the

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qualifications of this agency by providing a course of training for public servants before they join their appointments; and excellent arrangements with this object have been made by several of our universities. In addition to these schemes are in the air for the establishment of a School of Oriental Studies in London or of a College for Civilians in Calcutta. We must, however, recollect that the college established by Lord Wellesley at the beginning of the last century with the intention, to use his own words, of promoting among junior officers 'an intimate acquaintance with the history, language, customs, and manners of the people of India,' failed to meet the aims of its founder. We must also remember that recruits for the Colonial services do not undergo any training in this country; and that in the case of the Covenanted Civil Service of India the period extends only to a single year, during which the candidate is expected to learn the rudiments of at least one Oriental language and to acquire some knowledge of the law and history of India. It seems obvious that this leaves little time for the scientific study of anthropology; and the most that can be expected is to excite in the young official a desire to study the native races and to define the subjects to which his attention may usefully be directed. There is, again, the obvious risk of letting loose the half-trained amateur among savage or semi-savage peoples. He may see a totem in every hedge or expect to meet a corn-spirit on every threshing-floor. He may usurp the functions of the arm-chair anthropologist by adding to his own proper business, which is the collection of facts, an attempt to explain their scientific relations. As a matter of fact, the true anthropologist is born, not made; and no possible course of study can be useful except in the case of the few who possess a natural taste for this kind of work.

Having then practically exhausted our present agency it is incumbent upon us to press upon the governments throughout the empire the necessity of entrusting the supervision of ethnographical surveys to specialists. This principle has been recognised in the case of botany, geology, and archaeology, and it is high time that it was extended to anthropology. It is the possession of such a trained staff that has enabled the American Government to carry out with success a survey of the natives of the Philippine Islands; and it is gratifying to record that the Canadian legislature, in response to resolutions adopted by this section at the Winnipeg meeting, has recently voted funds to provide the salary of a superintendent of the ethnological survey. We may confidently expect that other governments throughout the empire will soon follow this laudable example. These governments will, of course, continue to collect at each periodical census those statistics and facts of sociology and economics which are required for purposes of administration. But beyond these practical objects there are questions which can be adequately investigated only by specialists.

The duties of such a director will necessarily be threefold: First, to sift, arrange, and co-ordinate the facts already collected by non-scientific observers; secondly, to initiate and control special investigations, in particular that intensive study of smaller groups within a limited area which, in the case of the survey of the Todas by Dr. Rivers, has so largely contributed to our knowledge of that tribe. Such methods not only open out new scientific fields, but, and this is perhaps more important, establish a standard of efficiency which improves later surveys of these or neighbouring races.

The field for inquiry throughout the empire is so vast that there is ample room for expeditions independent of official patronage. In some respects the private traveller possesses advantages over the official—in his freedom from the bondage of red tape and from the suspicion which inevitably attaches to the servant of government that his inquiries are conducted with the object of imposing taxation or of introducing some irksome measures of administration. He is always sure to receive the aid of local officers, whose familiarity with the native races must be of the highest value.

The third duty of the director will be to organise in a systematic way the collection of specimens for home and colonial museums. Our ethnographical museums, as a whole, have not reached that standard of efficiency which the importance of the empire and the needs of training in anthropology obviously require; and our students have to seek in museums at Berlin and other foreign cities for collections illustrative of tribes which have long been subject to British law. It is only necessary to refer to the recent handbook of the ethnographical

collections in the British Museum to see that there are wide gaps in the series which might easily be filled by systematic effort. No time is to be lost, because the tragedy of the extinction of the savage is approaching the final act, and our grandchildren will search for him in vain except perhaps in the slums of our greater cities.

Assuming then that in the near future anthropological inquiries will be organised on practical lines, I invite your attention to some special problems in India which deserve intensive study, and which can be solved in no other way. India is a most promising field for such inquiries. Here the student of comparative religion can trace with more precision than is possible in any other part of the empire the development of animism and the interaction on it of the forces represented by Buddhism, Hinduism, Islam, and Christianity. The anthropologist can observe the most varied types of moral and material culture, from those represented by the heirs of its historic civilisation down to forest and depressed tribes little raised above the level of savagery.

The first question which awaits examination is that of the prehistoric races and their relation to the present population. Unfortunately the materials for this inquiry are still imperfect. The operations of the archaeological survey, with the scanty means at its disposal, have rightly been concentrated upon the remains of architecture in stone, which starts from the Buddhist period, and upon the conservation of the splendid buildings which are our inheritance from older ruling powers. The prehistoric materials have been collected by casual workers who were not always careful to record the localities and circumstances of the discovery of their contribution to the local museums. Many links are still wanting, some altogether absent from Indian soil; others which systematic search will doubtless supply. We can realise what the position of prehistoric archaeology in Europe would be if the series of palæolithic barrows, the bone carvings of the cave-dwellers, the relics from kitchen-middens and lake dwellings were absent. The caves of central India, it is true, have supplied stone implements and some rude rock paintings. But the secrets of successive hordes of invaders from the north, their forts and dwellings, lie deep in the alluvium, or are still covered by shapeless mounds. Tropical heat and torrential rain, the ravages of treasure-hunters, the practice of cremation have destroyed much of the remains of the dead. The epigraphical evidence is enormously later in date than that from Babylon, Assyria, or Egypt; and the oriental indifference to the past and the growth of a sacred literature written to subserve the interests of a priestly class weaken the value of the historical record.

Further, India possesses as yet no seriation of ceramic types such as that devised by Professor Flinders Petrie which has enabled him to arrange the Egyptian tombs on scientific principles, or that which Professor Oscar Montelius has established for the remains of the Bronze Age. Mr. Marshall, the Director of the Archaeological Survey, admits that the Indian museums contain few specimens of metal work the age of which is even approximately known.

Though the record of the prehistoric culture is imperfect, we can roughly define its successive stages.

The palæolithic implements have been studied by Mr. A. C. Logan, whose work is useful if only to show the complexity of the problem. Those found in the laterite deposits belong to the later Pleistocene period, and display a technique similar to that of the river-drift series from western Europe. The Eoliths, which have excited such acute controversy, have up to the present not been discovered; and so far as is at present known the palæolithic series from India appears to be of later date than the European. Palæolithic man seems to have occupied the eastern coast of the peninsula, whence he migrated inland, using in turn quartzose, chert, quartzite, limestone, or sandstone for his weapons; that is to say, he seems not to have inhabited those districts which at a later time were seats of neolithic culture. Early man, according to what is perhaps the most reasonable theory, was first specialised in Malaysia, and his northward route is marked by discoveries at Johore and other sites in that region. Thence he possibly passed into India. The other view represents palæolithic man as an immigrant from Europe. At any rate, his occupation of parts of southern India was antecedent to the action of those forces which produced its present form ere the great rivers had excavated their present channels, and prior to the

deposition of the masses of alluvium and gravel which cover the implements which are the only evidence of his existence.

Between the palæolithic and the neolithic races there is a great geological and cultural gap; and no attempt to bridge it has been made except by the suggestion that the missing links may be found in the cave deposits when they undergo examination.

There is reason, however, to believe that the neolithic and the Iron Age cultures were continuous, and that an important element in the present population survives from the neolithic period. Relics of the neolithic are much more widely spread than those of the palæolithic age. They extend all over southern India, the Deccan, and the central or Vindhyan range. Up to the present they are scanty in the Punjab and Bengal; but this may be due to failure to discover or identify them. Mr. Bruce Foote has discovered at various sites in the south factories of neolithic implements associated with wheel-made pottery of a fairly advanced type, showing that the Stone Age has survived side by side with that of metal down to comparatively recent times. The Veddas of Ceylon, the Andamanese, and various tribes on the north-east frontier, in central and southern India, are, or were up to quite recent times, in the Age of Stone. In fact, when we speak of ages of stone or metal we must not regard them as representing division of time but generally continuous phases of culture.

There is no trustworthy evidence for the existence of an Age of Bronze. The single fine implement of this metal which has been discovered is probably, like the artistic vessels from the Nilgiri interments, of foreign origin; and other implements of a less defined type seem to be the result of imperfect metallurgy. This is not the place to discuss the problem of the origin and diffusion of bronze. Babylon, Asia Minor, and China have each been supposed to be a centre of distribution. The Egyptian specimen attributed to the third dynasty, say before the fourth millennium B.C., is believed by Professor Petrie to be the result of a chance alloy; but the metal certainly appears in Egypt about 1600 B.C., and it is believed to have originated in central Europe, where the Zinnwald of Saxony or the Bohemian mines provided a supply of tin. The absence of a Bronze Age in India has been explained by the scarcity of tin and the impossibility of procuring it from its chief source in the Malay-Burman region, where the mines do not seem to have been worked in ancient times. But another view deserves consideration. Professor Ridgeway has shown that all the sites where native iron is smelted are those where carboniferous strata and ironstone have been heated by eruptions of basalt; and iron was thus produced by a natural reduction of the ore. In Africa as well as India the absence of the Bronze Age seems to be due to the abundant supplies of iron ores which could be worked by processes simpler than those required in the case of bronze. In India iron may have been independently discovered towards the close of the neolithic period, and iron may have displaced copper without the intervention of bronze.

However this may be, the Copper Age in India, which has been carefully studied by Mr. V. A. Smith, is of great importance. Implements of this metal in the form of flat and bar celts, swords, daggers, harpoon, spear, and arrow heads, with ornaments and a strange figure probably human, have been found at numerous sites in northern India. In western Europe, according to Dr. Munro, the Copper Age was of short duration; but Mr. Smith believes that in India the variety of types indicates a long period of development.

No mention of iron occurs in the Rig-veda; but it appears in the Atharvan, which cannot be dated much later than 1000 B.C. It is now recognised that there is a still obscure stratum of Babylonian influence underlying the Aryan culture; and if, as is generally supposed, the manufacture of iron was established by the Chalybes at the head-waters of the Euphrates, who passed it down the delta, its use may have spread thence among the Indo-Aryans. It certainly appears late in the south Indian dolmen period; and we have the alternatives of believing that it was introduced there by the Dravidian trade with the Persian Gulf, which certainly arose before the seventh century before Christ, or that it was independently discovered by the Dravidians who still extract it in a rude way from the native ores.

The great series of dolmens, circles, and kistvaens which cover the hills and plateaux of the Deccan and the region to the south seem to belong to the Iron

Age. Whether the construction of these monuments was due to the migration of the dolmen-building race from northern Africa, or whether the builders were a local people utilising the material on the spot must remain uncertain. The excavations conducted by Mr. Breeks and others disclose tall jars, many-storeyed cylinders of varying diameter, with round or conical bases, fashioned to rest on pottery ring-stands, like the classical amphorae, or to be imbedded in softer soil. The lids of these vessels are ornamented with rude, grotesque figures of men, animals, or more rarely inanimate objects, depicting the arms, dress, ornaments, and domesticated fauna of the period. It has been suspected that these figurines may be of a date earlier than the implements of iron with which they are associated, and that they were deposited with the dead in a spirit of religious conservatism. At any rate, the costumes and arms represented on the older pottery present no resemblance to those depicted on the later series of dolmens and kistvaens. The pottery also seems to belong to different periods, the larger jars being of a later date than the true funereal urns which are found at a lower level, and contain a few cremated bones, gold ornaments, bronze and iron rings, with beads of glass or agate. These people clearly regarded bronze as an article of luxury, as it appears in the form of ornaments or in the series of splendid vases preserved in the Madras museum. It is difficult to suppose that these were of local origin; more probably they were imported in the course of trade along the western coast or from more distant regions.

Another and equally remarkable phase of culture, combining distinctly savage features with a fairly advanced civilisation, is illustrated by the Adittanalur cemetery in the Tinneveli district recently excavated by Mr. Rea. Two skulls discovered here are prognathous, suggesting a mixture of the Negrito and Dravidian types. There is no trace of cremation, and in most cases the smallness of the urn openings implies that the corpses were exposed to birds of prey, and that only such bones as could be discovered after removal of the flesh were collected for interment; or, according to another interpretation of the facts, we have an instance of the custom of mourners carrying with them, like the modern Andamanese, the relics of the dead. These interments certainly extended over a long period, neolithic weapons being found in some graves, while in others iron arms were discovered fixed point downwards near the urns, as if they had been thrust into the ground by the mourners. In the richer graves gold frontlets, like those of Mycenae and other Greek interments, were fastened over the forehead of the corpse. These were, like the Greek specimens, of such a flimsy type that they could never have been used in real life. It is a remarkable instance of a survival in custom that at the present day some tribes in this region tie a triangular strip of gold on the forehead of the dead, the import of which, on the analogy of the death masks of Siam, Cambodia, ancient Mexico, and Alaska, we may interpret as an attempt to guard the corpse from the glances of evil spirits while the spirit is on its way to deathland, or to be used in processions of the corpse.

The question remains: To what races may we attribute these successive phases of culture in southern India? The Tamil literature, as interpreted by Bishop Caldwell and Mr. V. Kanakasabhai, shows the existence of an advanced type of archaic culture in this region; but the evidence to connect this with the existing remains is as yet wanting. We may reasonably assume that neolithic man survives in the existing population, because we have no evidence of subsequent extensive migrations, except the much later arrival of Indo-Aryan colonies from the north, and that of the Todas, whom Dr. Rivers satisfactorily identifies with the Nayars and Nambutiri Brahmans of Malabar. The occurrence of a short-headed strain among some tribes in western India probably represents some prehistoric migration by sea or along the coast line from the direction of Baluchistan or the Persian Gulf. The suggestion that it is the result of a Scythian or Hun retreat from northern India in the face of an advancing Aryan movement is not corroborated by any historical evidence, and is in itself improbable. The customs of dolmen and kistvaen burial still persist among some of the present tribes, and they display some reverence for the burial places of their forgotten predecessors. This feeling may, however, be due to the habitual tendency of the Hindu to perform rites of propitiation at places supposed to be the haunts of spirits, and need not necessarily connote racial identity.

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The most primitive type identifiable in the population of south India is the Negrito, which appears among the Veddas of Ceylon, and among the Andamanese, who retain the Negrito skin colour and hair, but have acquired, probably from some Mongoloid stock, distinct facial characters. It has been the habit with some writers to exaggerate the Negrito strain in the south. But tribes like the Badagas and Kotas, which have been classed as representative of this type, possess none of the Negrito characters, which appear only among the more primitive Kurumbas, Malayans, Paniyans, and Irulas. In all the modern tribes the distinctive Negrito marks—woolliness of hair, prognathism, lowness of stature, and excessive length of arm—have become modified by miscegenation or the influences of environment.

The resemblances in culture of the Indian Negrito with the cognate races to the east and south-east of the Peninsula are too striking to be accidental. The Kadirs of Madras climb trees like the Bornean Dayaks, clip their teeth like the Jakun of the Malay Peninsula, and wear curiously ornamented hair combs like the Semang of Perak, among whom they serve some obscure magical purpose. The Negrito type deserves special examination in relation to the recent discovery of Pygmies in New Guinea, and the monograph on the Pygmy races in general by Dr. P. W. Schmidt, who regards them as the most archaic human type, from which he supposes the more modern races were developed, not by a process of gradual evolution, but *per saltum*. If there be any force in these speculations he is justified in expressing his conviction that the investigation of the Pygmy races is, at the present moment, one of the weightiest and most urgent, if not the most weighty and most urgent, of the tasks of ethnological and anthropological science.

This Negrito stock was followed and to a considerable extent absorbed by that which is usually designated the Dravidian. The problem of the origin of this race has been obscured by the unhappy adoption of a linguistic term to designate an ethnical group, and its unwarrantable extension to the lower stratum of the population of northern India. At present the authorities are in conflict on this, the most important question of Indian ethnology. One school denies that this people entered India from the north or north-west on the ground that the immigration of a dolichocephalic race from a brachycephalic area is impossible, and insists that the distinction between the so-called Dravidians and Kolarians is linguistic, not physical. The other theory postulates the origin of the Dravidians from the north-west, that of the Kolarians from the north-east; and avoids the difficulty of head form by referring the Dravidians to one of the long-headed races of central or western Asia or north Africa, or by suggesting that their skull form has become modified on Indian soil by environment or miscegenation.

Recent investigations, archaeological or linguistic, throw some new light on this complex problem. Sir T. Holdich, in his recent work 'The Gates of India,' asserts that Makrán, the sea-board division of Baluchistan, is full of what he calls 'Turanian,' or Dravidian remains. He explains the position of the Brahui tribe in Baluchistan, on whom the controversy mainly turns, by assuming that while they now call themselves Mingal or Mongal and retain no Dravidian physical characters, the survival of their Dravidian tongue is due to the fact that it is their mother language, preserved by Dravidian women enslaved by Turo-Mongol hordes. Relics of the original Dravidian stock, he suggests, may be found in the Ichthyophagi, or fish-eaters, whom Nearchus, the admiral of Alexander the Great, observed on the Baluchistan coast, living in dwellings made of whale-bones and shells, using arrows and spears of wood hardened in the fire, with claw-like nails and long shaggy hair, a record of the impression made upon the curious Greeks by the first sight of the Indian aborigines.

In the next place, inquiries by Dr. Grierson in the course of the Linguistic Survey prove that what is called the Mon-khmer linguistic family, which preceded the Tibeto-Burmans in the occupation of Burma, at one time prevailed over the whole of Further India, from the Irawadi to the Gulf of Tongking, and extended as far as Assam. To this group the Munda tongue spoken by some hill tribes in Bengal is allied; or, at least, it may be said that languages with a common substratum are now spoken not only in Assam, Burma, Annam, Siam, and Cambodia, but also over the whole of Central India as far west as the Berars. 'It is,' says Dr. Grierson, 'a far cry from Cochin-China to Nimár, and yet, even at the present day, the coincidences between the language of the Korkus of the

latter district and the Annamese of Cochin-China are strikingly obvious to any student of language who turns his attention to them. Still further food for reflection is given by the undoubted fact that, on the other side, the Munda languages show clear traces of connection with the speech of the aborigines of Australia.' The last assumption has been disputed, and it is unnecessary to discuss this wider ethnical grouping. Though identity of language is a slippery basis on which to found an ethnological theory, it seems obvious that the intrusive wedge of dialects allied to the Mon-khmer family implies that the Central Indian region was at one time occupied by immigrants who forced their way through the eastern Himalayan passes, their arrival being antecedent to the migration which introduced the Tai and Tibeto-Burman stocks into Further India.

When the solution of this problem is seriously undertaken under expert guidance, the first step will be to make an exhaustive survey of the group of forest tribes, from the Santāls and Pahārias on the east, passing on to the Kōls and Gonds, and ending with the Bhīls on the west. At present our information of the inter-relations of these tribes is fragmentary, and their superficial uniformity does not exclude the possibility that they represent more than one racial element. It will also be necessary to push inquiry beyond the bounds of the Indian Empire, and, like the trigonometrical surveyor, to fix the base line as a datum in India, and extend the triangulation through the borderlands. It is in these regions that the ethnological problems of India await their final solution. Many of these countries are still beyond our reach. Until the survey of the routes converging at Herat, Kabul, or Kandahar is complete the extent of the influence of the western races—Assyrian, Babylonian, Iranian, Arab, and Greek—cannot be determined. Recent surveys in Tibet have thrown much light on that region, but it is still only very partially examined. In Nepal the suspicious native Government still bars the way to the Buddhist sites in the Tarai and the Nepal valley, and thus a wide chapter in the extension of Hindu influence beyond the mountain range remains incomplete.

The second great problem is the origin and development of caste. We have yet to seek a definition which will cover the complex phases of this institution, and effect a reconciliation between the views of Indian observers who trace it to the clash of races or colours, and that of the sociologists, who lay little stress on race or colour and rely more upon the influence of environment, physical or moral. We must abandon the insular method which treats it only in relation to India, and ignores the analogous grouping of rank and class which were prepotent in Western Europe and elsewhere, and are now slowly losing ground in the face of industrial development. It is by the study of tribes which are on the borderland of Hinduism that we must look for a solution of the problem. The conflict of the Aryan and aboriginal culture, on which the religious and social systems of Hinduism were based, is reproduced in the contact between modern Hinduism and the forest tribes. Since the Hindus are the only members of the Aryan stock among whom we find endogamous groups with exogamous sections, the suggestion of Professor Frazer that they may have borrowed it from the non-Aryans gains probability. The Dravidians within the Indian totemic area have worked out an elaborate system of their own, which is well described in the recent survey of the Malaysians by Mr. F. T. Richards. How far this is connected with their preference for mother-right and their strong family organisation, of a more archaic type than the joint family of the Aryans, is a question which deserves examination. The influence, again, of religion must be considered, and this can be done with the most hopeful results in regions like eastern Bengal, where a people who have only in a very imperfect way adopted Hinduism are now being converted wholesale to Muhammadanism.

Again, when we speak of the tribe in India, we must remember that it assumes at least seven racial types, ranging from the elaborate exogamous groups of the Rajputs to the more archaic form characteristic of the Baloch and Pathān tribes of the western frontier, attached to which are alien sections affiliated by the obligation to join in the common blood-feud, which in process of time develops into a fiction of blood-brotherhood. Thus among the Marri of Baluchistan we can trace the course of evolution: admission to participate in the common blood-feud, admission to participation in a share of the tribal land, and finally admission to kinship in the tribe.

This elasticity of structure has permitted not only the admission of non-Aryan tribes into the Rajput body in modern times, but prepares us to understand how the majority of the Rajputs were created by a similar process of fusion, the newcomers being known as the Gurjaras, who entered India in the train of the Huns in the fifth or sixth centuries of our era. The recognition of this fact, by far the most important contribution made in recent times to the ethnology of India, is due to a group of Bombay scholars, the late Mr. A. M. T. Jackson, whose untimely death at the hand of an assassin we deeply regret, and the brothers Bhandarkar. Mr. D. R. Bhandarkar has recently proved that a group of these Gurjara Huns, possibly the tribal priests or genealogists, were admitted first to the rank of Brahmins, and then, by a change of function, of which analogies are found in the older Sanskrit literature, becoming Rajputs, are now represented by the Guhilots, one of the proudest septs. This opens up a new view of tribal and caste development. Now that we can certainly trace the blood of the Huns among the Rajput, Jat, and Gujar tribes, a fresh impulse will be given for the quest of survivals in belief and custom connecting them with their Central Asian kinsfolk.

In what I have said I have preferred to speculate on a problem for work in the future rather than dwell upon the progress which has been already made. In the sphere of religion we have passed the stage when, as Professor Max Müller said, 'the best solvent of the old riddles of mythology is to be found in the etymological analysis of the names of gods and goddesses, heroes and heroines,' or when the 'disease of language' theory was generally accepted. The position, in fact, has completely changed since Comparative Religion has adopted the methods of Anthropology. The study of myths has given way to that of cults, the former being often only naive attempts to explain the latter. India offers wide fields for inquiry by these new methods, because it supplies examples of cult in its most varied and instructive phases. The examination of Hinduism, the last existing polytheism of the archaic type, is likely to explain much hitherto obscure in the development of other pantheons. It is no longer possible to refer the complex elements of this or any other group of similar beliefs to a single class of physical concepts. The sun, the dawn, the golden gates of sunset, or the dairy no longer furnish the key which unlocks the secret. It is by the study of the Animism, Shamanism, or Magic of the lower tribes that Hinduism can be interpreted. This analysis shows that behind the myths and legends which shroud the forms of the sectarian gods the dim shape of a Mother goddess appears, at once chthonic or malignant because she gives shelter to the dead, and beneficent because she nurtures the sons of men with the kindly fruits of the earth. Beside her, though his embodiment is much less clearly defined, stands a male deity, her consort, and by a process of magic, mimetic, sympathetic, or homœopathic, their union secures the fertility of the animal and vegetable creation.

Much, however, remains to be done before the problems of this complex polytheism can be fully solved. The action of archaic religions, as has been well said, 'takes place in the mysterious twilight of sub-consciousness'; and the foreign observer is trammelled by the elaborate system of tabu with which the Hindu veils the performance of his religious rites. This feeling extends to all classes, and the ceremonial of the jungle shrines is as little open to examination as the *penetralia* of the greater temples. The great army of mendicant friars jealously conceals the secrets of its initiation, rites, and beliefs, and this field of Indian religious life remains practically unworked. Much may be done by the training of a body of native observers who are not subject to the tabu imposed upon the foreigner. Here the difficulty lies in the contempt displayed by the higher educated classes towards the beliefs and usages of the lower tribes. There are some indications that this feeling is passing away, and in recent years much useful ethnological work has been done by native scholars.

The problems of ethnology, so far as they are concerned with the origin of prehistoric races and their relation to the existing population, are more or less academic. Ethnography, which examines the religious, cultural, and industrial conditions of the people, has more practical uses. At the present time it is incumbent upon us to preach, in season and out of season, that the information which it is competent to supply is the true basis of administrative and social reform. If, for example, we were now in possession of the facts which an

anthropometrical survey of our home population would supply, many of our social problems would assume a clearer aspect. Such, for instance, are the questions of degeneration due to slum life and malnutrition, the influence of alcoholism on industrial efficiency, the condition of dangerous and sweated industries, and that of the aliens settled in our midst. It is characteristic of the genius of the English people, that while we are not yet prepared to admit the need of such a survey, the provision of medical inspection and relief for children in elementary schools will soon render it inevitable.

This is more clearly the case in those regions where a large native population is controlled by a small European minority. The Negro question in America teaches us a useful lesson, applicable to native races in most parts of the Empire. In India, whenever the Government has made really serious mistakes, the failure has been due to ignorance or disregard of the beliefs or prejudices of the subject people. A little more than a century ago a mutiny of native troops at Vellore was due to injudicious attempts to change a form of headdress which they believed to be a symbol of their religion or caste; ignorance of the condition of the Santáls allowed them to be driven to frenzy by the extortions of moneylenders which culminated in a serious outbreak; the greased cartridges of the Great Mutiny, and the revolt against measures, adopted in defiance of native feeling to check the plague epidemic, teach a similar lesson.

In India at the present time 'the old order changeth, yielding place to new'; and at no period in the history of our rule was it more necessary to effect a reconciliation between the foreigner and the native. While the tabus of marriage relations and commensality will for an indefinite period prevent the amalgamation of the races, much of the present disquiet is due to ignorance and misunderstanding on both sides. The religious and social movements now in progress deserve the attentive study of the British people. In religion various attempts are being made to free Hinduism from some of its most obvious corruptions, to harmonise Eastern and Western ideals, and to elevate the former so as to enable them to resist the pressure of the latter. Such is Vedantism, a revival of the ancient pantheistic philosophy, which not only claims supremacy in India, but asserts that its mission is to replace the dying faiths of the Western world. The spread of monotheism, as represented by Bhagavata beliefs, is equally noteworthy; and the effect of the revival of the cults of Ganpati, god of luck, and of Sivaji, the Mahratta hero, on the political situation in the Deccan deserve the most careful consideration.

The social movement is the result of that fermentation which is in progress among the subject peoples in many parts of the world. While the educated Indian claims social equality with the foreigner, he is occupied with a serious problem at his own doors. The degraded castes, popularly called the 'untouchables,' are revolting against the obloquy which they have long endured at the hands of the higher races. Many of them have sought relief by joining the Christian or Muhammadan communities, and the process of conversion is so remarkable as to excite the surprise and alarm of the orthodox classes. Measures have been designed to improve their almost intolerable position. It remains to be seen how far any concessions which are likely to satisfy them can be reconciled with the ideals of the caste system.

It is true that the people of India prefer to celebrate many of their religious and social rites free from observation of the foreigner, and that there are forbidden chambers in the Oriental mind which no stranger may enter. But the experience of those best qualified to express an opinion is that a sympathetic interest in the religious and social life of the people, so far from tending to increase the existing tension, is a valuable aid towards the promotion of mutual goodwill and sympathy. Orthodox native States not only show no aversion to ethnographical inquiry, but are themselves actively engaged in such surveys. Even the Rajputs, who ordinarily display little taste for scientific work, are beginning to undertake the collection of the bardic chronicles which embody their tribal folk-lore and traditions.

When the divergencies in the beliefs and institutions of the foreigner and the indigenous races are realised and understood, a compromise must be effected, each side discarding some hereditary prejudices—the Hindu that aversion to the manners and customs of the European which is the chief barrier to the promotion

of intercourse between the races; the European that insularity of thought which makes it difficult for him to understand all that is valuable in novel types of belief and culture, as well as that lack of imagination which inclines him to exaggerate what seems to him intolerable in the economical condition, the social organisation and beliefs of races whose environment differs from his own.

Anthropology has thus a practical as well as a scientific side. The needs of inquirers whose interest mainly lies in the investigation of survivals and in the stages of evolution in culture and belief can, as I have endeavoured to show, be met only by the adoption of improved methods of inquiry and a more rigorous dissection of evidence. Unfortunately the inadequate resources of the societies devoted to the study of man, as contrasted with the extent of the sphere of inquiry and the importance of the savage or semi-savage races as factors in the progress of the Empire, prove that the practical value of anthropology is as yet only imperfectly realised. If its progress is to be continuous we must convince the politician that it has an important part to play in the schemes in which he is interested. Thus it is certain that in the near future the relations between the foreigner and the native races will demand the increasing attention of statesmen at home and abroad. Here anthropology has a wide field of action in the examination of the causes which menace the very existence of the savage; of the condition of the mixed races, like the Creole or the Eurasian; of the relations of native law and custom to the higher jurisprudence; of the decay of primitive industries in the face of industrial competition. One of its chief tasks must be the examination of the physical and moral condition of the depressed classes of our home population, and the effect of modern systems of education on the mind and body of the child. It will thus be in a position to assist the servants of the State to meet the ever-increasing responsibilities imposed upon them; and it will help to dispel the ignorance and misconceptions which prevail even among the intelligent classes in this country in regard to the condition of the native races, who, by a strange decree of destiny, have been entrusted to their charge. By such practical contributions to the welfare of humanity it will not only secure the popular interest which is a condition of efficiency, but engage the ever-increasing attention of those to whom its scientific side is of paramount importance.

British Association for the Advancement of Science.

SHEFFIELD, 1910.

ADDRESS

TO THE

PHYSIOLOGICAL SECTION

BY

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PRESIDENT OF THE SECTION.

THE record of investigation of the phenomena of the life of animal and vegetable cells for the last eighty years constitutes a body of knowledge which is of imposing magnitude and of surpassing interest to all who are concerned in the studies that bear on the organic world. The results won during that period will always constitute, as they do now, a worthy memorial of the intense enthusiasm of the scientific spirit which has been a distinguishing feature of the last six decades of the nineteenth century. We are to-day, in consequence of that activity, at a point of view the attainment of which could not have been predicted half a century ago.

This body of knowledge, this lore which we call cytology, is still with all this achievement in one respect an undeveloped science. It is chiefly—nay, almost wholly—concerned with the structural or morphological side of the cell, while of the functional phenomena our knowledge is only of the most general kind, and the reason is not far to seek. What little we know of the physiological side of the cell—as, for example, of cellular secretion, absorption, and nutrition—has only to a very limited extent been the outcome of observations directed to that end. It is in very great part the result of all the inferences and generalisations drawn from the data of morphological research. This knowledge is not the less valuable or the less certain because it has been so won, but simply because of its source and of the method by which we have gained it, it is of a fragmentary character, and therefore less satisfactory in our estimation.

This state of our knowledge has affected—or, to express it more explicitly, has fashioned—our concept of living matter. When we think of the cell it is idealised as a morphological element only. The functional aspect is not ignored, but we know very little about it, and we veil our ignorance by classing its manifestations as vital phenomena.

It is true that in the last twenty years, and more particularly in the last ten, we have gathered something from biochemical research. We know much concerning ferment or catalytic action, of the physical characters of colloids, of the constitution of proteins, and their synthesis in the laboratory promises to be an achievement of the near future. We are also in a position to understand a little more clearly what happens in proteins when, on decomposition in the cell, they yield the waste products, urea, and other metabolites, with carbon dioxide and water. Further, fats can be formed in the laboratory from glycerine and fatty acids, a large number of which have also been synthesised, and a very large majority of the sugars of the aldohexose type have been built up from simpler compounds. These facts indicate that some of the results of the activity of animal and vegetable cells may be paralleled in the laboratory, but that is as far as the resemblance extends. The methods of the laboratory are not as yet those of nature. In the formation of carbohydrates, for example, the chlorophyll-

holding cell makes use of processes of the most speedy and effective character, but nothing of these is known to us except that they are quite unlike the processes the laboratory employs in the artificial synthesis of carbohydrates. Nature works unerringly, unfalteringly, with an amazing economy of material and energy, while our laboratory syntheses are but roundabout ways to the waste sink.

In consequence, it is customary to regard living matter as unique—*sui generis*, as it were, without an analogue or parallel in the inorganic world—and the secrets involved in its actions and activities as insoluble enigmas. Impelled by this view there are those, also, who postulate as an explanation for all these manifestations the intervention in so-called living matter of a force otherwise and elsewhere unknown, biotic or vital, whose action is directed, according to the character of the structure through which it operates, to the production of the phenomena in question. Living protoplasm is, in this view, but a mask and a medium for action of the unknown force.

This is an old doctrine, but it has again made headway in recent years owing to the reaction from the enthusiasm which came from the belief that the application of the known laws of physics and chemistry in the study of living matter would explain all its mysteries. A quarter of a century ago hopes were high that the solution of these problems would soon be found in a more profound comprehension of the laws of the physical world. Since then there has been an extraordinary increase in our knowledge of the structure and of the products of the activity of living matter without a corresponding increase in knowledge of the processes involved. The obscurity still involving the latter appears all the greater because of the high lights thrown on the former. Despair, in consequence, has taken the place of hope with some, and the action of a mysterious force is invoked to explain a mystery.

It may be admitted that our methods of investigation are very inadequate, and that our knowledge of the laws of matter, seemingly comprehensive, is not at present profound enough to enable us to solve all the problems involved in the vital phenomena. The greatest factor in the difficulty of their solution, however, has been the fact that there has been a great lack of investigators specially trained not only in biology, but also in physics and chemistry, for the very purpose of attacking intelligently such problems. The biologists, for want of such a wide training, have emphasised the morphological aspect and the readily observable phenomena of living matter; while the physicist and chemist, knowing little of the morphology of the cell and of its vital manifestations, have been unable to apply satisfactorily the principles of their sciences to an understanding of its processes. The high degree of specialism which certain departments of biology has in recent years developed has made that difficulty greater than it was.

It must also be said that in some instances in which the physicist and chemist attempted to aid in the solution of biological problems the result on the whole has not been quite satisfactory. In, for example, the phenomena of osmosis the application of Arrhenius' theory of ionisation and van 't Hoff's gas theory of solutions promised at first to explain all the processes and the results of diffusion through animal membranes. These theories were supported by such an array of facts from the side of physics and physical chemistry that there appeared to be no question whatever regarding their universal validity, and their application in the study of biological phenomena was urged with acclaim by physical chemists and eagerly welcomed by physiologists. The result in all cases was not what was expected. Diffusion of solutes, according to the theories, should, if the membrane is permeable to them, always be from the fluid where their concentration is high to that in which it is low. This appears to happen in a number of instances in the case of living membranes—or, at least, we may assume that it occurs—but in one signal instance at least the very reverse normally obtains. In the kidney, membranes formed of cells constituting the lining of the glomeruli and the renal tubules separate the urine, as it is being formed, from the blood plasma and the lymph circulating through the kidney. Though the excreted fluid is derived from the plasma and lymph, it is usually of much greater osmotic concentration than the latter.

It may be urged that this and other discrepancies are explained by the distribution (or partition) coefficient of the solutes responsible for the greater con-

centration of the product of excretion, these solutes being more soluble in the excreted medium than in the blood plasma and distributing or diffusing themselves accordingly. If such a principle is applicable here as an explanation, it may be quite as much so in other physiological cases in which the results are supposedly due only to the forces postulated in the theories of van 't Hoff and Arrhenius. Whether this be so or not, the central fact remains that the enthusiastic hopes with which the theories were applied by physiologists and biologists in the explanation of certain vital phenomena have not been wholly realised.

The result has been a reaction amongst physiologists and biologists which has not been the least contributory of all the causes that have led to the present revival of vitalism.

Another difficulty in accounting for the vital phenomena has been due, until recently, to a lack of knowledge of the physical and chemical properties of colloids and colloidal 'solutions.' The importance of this knowledge consists in the fact that protoplasm, 'the physical basis' of life, consists mainly of colloids and water. Till eleven years ago what was known regarding colloids was derived chiefly from the researches of Graham (1851-62), Ljubavin (1889), Barus and Schneider (1891), and Linder and Picton (1892-97), who were the pioneers in this line. In 1899 were published the observations of Hardy, through whose investigations very great progress in our knowledge of colloids was made. In 1903 came the invention of the ultramicroscope by Siedentopf and Zsigmondy, by which the suspension character of colloid material in its so-called 'solutions' was visually demonstrated. During the last seven years a host of workers have by their investigations greatly extended our knowledge of the physical and chemical properties of colloids, and now the science of Collochemistry bids fair, the more it develops, to play a very important part in all studies bearing on the constitution and properties of living matter.

Then, also, there are the phenomena of surface tension. This force, the nature of which was first indicated by Segner in 1751, and described with more detail by Young in 1804 and La Place in 1806 in the expositions of their theories of capillarity, was first in 1869 only casually suggested as a factor in vital processes by Engelmann. Since the latter date and until 1892, when Bütschli published his observations on protoplasmic movement, no serious effort was made to utilise the principle of this force in the explanation of vital phenomena. Even to-day, when we know more of the laws of surface tension, it is only introduced as an incidental factor in speculations regarding the origin of protoplasmic movement and muscular contraction, and yet it is, as I shall maintain later on in this address, the most powerful, the most important of all the forces concerned in the life of animal and vegetable cells.

It may be gathered from all that I have advanced here that the chief defect in biological research has been, and is, the failure to apply thoroughly the laws of the physical world in the explanation of vital phenomena. Because of this too much emphasis is placed on the division that is made between the biological and the physical sciences. This division is very largely an artificial one, and it will in all probability be maintained eventually only as a convenience in the classification of the sciences. The biologist and physiologist have to deal with problems in which a wide range of knowledge is necessary for their adequate treatment; and, if the individual investigator has not a very extensive training in the physical sciences, it is impossible for him to have at his command all the facts bearing on the subject of his research, unless the problem involved be a very narrow one. The lack of this wide knowledge of the physical sciences tends to specialism, and, as the specialism is ever growing, it will produce a serious situation eventually, for it will develop a condition in the scientific world in which co-ordination of effort and a broad outlook will be much more difficult than is the case now.

This growing defect in the biological sciences can only be lessened by the insistence of those in charge of advanced courses in biological and physiological laboratories that only they whose training is of a very wide character should be allowed to take up research. It is, perhaps, futile to expect that such a rule will ever be enforced, for in the keen competition between universities for young teachers who have made some reputation for original investigation there may not be too close a scrutiny of the qualifications of those who offer themselves for post-graduate courses. There is, further, the difficulty that the heads of scientific departments are not desirous of limiting the output of new knowledge from

their laboratories by insisting on the wider training for the men of science who are in the process of developing as students of research.

It is perhaps true, also, that there still remains a great deal unobserved or unrecorded in the fields of biology, physiology, and biochemistry, in the investigation of all of which a broad training is not specially required to give good service; and that, further, this condition will obtain for one or two decades still. It is quite as certain, however, that the returns from such service will tend to diminish in number and value, and, if the coming generation of workers is not recruited from a systematically and broadly trained class of students, a period of comparative sterility may supervene.

As it is to-day, there are few who devote themselves to the direct study of the chemical and physical properties of the cell, the fundamental unit of living matter. There are, of course, many who are concerned with the morphology of the cell, and who employ in their studies the methods of hardening and staining which have been of very great service in revealing the structural as well as the superficial chemical properties of the cell. On the facts so gained views are based which deal with the chemistry of the cell, and which are more or less widely accepted, but the results and generalisations drawn from them give us but little insight into the chemical constitution of the cell. We recognise in the morphologists' chromatin a substance which has only in a most general way an individuality, while the inclusions in the nucleus and the cytoplasm, on whose distinction by staining great emphasis is laid, can only in a most superficial way be classified chemically.

The results of digestion experiments on the cell structures are also open to objection. The action of pepsin and hydrochloric acid must depend very largely on the accessibility of the material whose character is to be determined. If there are membranes protecting cellular elements, pepsin, which is a colloid, if it diffuses at all, must in some cases at least penetrate them with difficulty. In *Spirogyra*, for example, the external membrane formed of a thick layer of cellulose is impermeable to pepsin, but not to the acid; and, in consequence, the changes which occur in it during peptic digestion are due to the acid alone. Even in the cell whose periphery is not protected by a membrane, the insoluble colloid material at the surface serves as a barrier to the free entrance of the pepsin. It is, however, more particularly in the action on the nucleus and its contents that peptic digestion fails to give results which can be regarded as free from objection. Here is a membrane which during life serves to keep out of the nucleus not only all inorganic salts but also all organic compounds, except chiefly those of the class of nucleo-proteins. That such a membrane may, when the organism is dead, be permeable to pepsin is at least open to question, and in consequence what we see in the nucleus after the cell has been acted on by pepsin and hydrochloric acid cannot be adduced as evidence of its chemical or even of its morphological character.

The results of digestive experiments on cells are, therefore, misleading. What may from them appear as nucleo-protein may be anything but that, while, if the pepsin penetrates as readily as the acid, there should be left not nucleo-protein, but pure nucleic acid, which should not stain at all.

The objections which I now urge against the conclusions drawn from the results of digestion experiments have developed out of my own observations on yeast cells, diatoms, *Spirogyra*, and especially the Blue-green Algae. The latter are, as is *Spirogyra*, encased in a membrane which is an effective barrier to all colloids. When, therefore, threads of *Oscillaria* are subjected to the action of artificial gastric juice, a certain diminution in volume is observed owing to the dissolving power of the hydrochloric acid, and an alteration of the staining power of certain structures is found to obtain, but the pepsin has nothing to do with these, as may be determined by examination of control preparations treated with a solution of hydrochloric acid alone.

It is thus seen how slender is our knowledge of the chemistry of cells derived from staining methods and from digestion experiments. That, however, has not been the worst result of our confidence in our methods. It has led cytologists to rely on these methods alone, to leave undeveloped others which might have thrown great light on the chemical constitution of the cell, and which might have enabled us to understand a little more clearly the causation of some of the vital phenomena.

PRESIDENTIAL ADDRESS.

It was the futility of some of the old methods that led me, twenty years ago, to attack the chemistry of the cell from what appeared to me a correctly chemical standpoint. It seemed to me then, and it appears as true now, that a diligent search for decisive chemical reactions would yield results of the very greatest importance. In the interval I have been able to accomplish only a small fraction of what I hoped to do, but I think the results have justified the view that, if there had been many investigators in this line instead of only a very few, the science of Cytochemistry would play a larger part in the solution of the problems of cell physiology than it now does.

The methods and the results are, as I have said, meagre, but they show distinctly indeed that the inorganic salts are not diffused uniformly throughout the cell; that in vegetable cells they are rigidly localised, while in animal cells, except those devoted to absorption and excretion, they are confined to specified areas in the cell. Their localisation, except in the case of inorganic salts of iron, is not due to the formation of precipitates, but rather to a condition which is the result of the action of surface tension. This seems to me to be the only explanation for the remarkable distribution, for example, of potash salts in vegetable cells. We know that, except in the chloroplatinate of potassium and in the hexanitrite of potassium, sodium and cobalt, potassium salts form no precipitates; and yet, in the cytoplasm of vegetable cells, the potassium is so localised at a few points as to appear at first as if it were in the form of a precipitate. In normal active cells of *Spirogyra* it is massed along the edge of the chromatophor, while in the mesophyll cells of leaves it is condensed in masses of the cytoplasm, which are by no means conspicuous in ordinary preparations of these cells.

This effect of surface tension in localising the distribution of inorganic salts at points in the cytoplasm would explain the distribution of potassium in motor structures. In striated muscle the element is abundant in amount, and is confined to the dim bands in the normal conditions. In *Vorticella*, apart from a minute quantity present at a point in the cytoplasm, it is found in very noticeable amounts in the contractile stalk; while in the Holotrichate Infusoria (*Paramecium*) it is in very intimate association with the basal elements of the cilia in the ectosarc. This, indeed, would seem to indicate that the distribution of the potassium is closely associated with contraction, and, therefore, with the production of energy in contractile tissues. The condensation of potassium at a point may, of course, be a result of a combination with portions of the cytoplasm, but we have no knowledge of the occurrence of such compounds; and, further, the presence of such does not explain anything, or account for the liberation of energy in motor contraction. On the other hand, the action of surface tension would explain not only the localisation of the potassium but also the liberation of the energy.

In vessels holding fluids the latter, in relation to surface tension, have two surfaces—one free, in contact with the air, and known as the air-water surface; the other, that in contact with the wall of the containing vessel (glass). In the latter the tension is lower than in the former. When an inorganic compound—a salt, for example—is dissolved in the fluid it increases the tension at the air-water surface, but its dilution is much greater here than in any other part of the fluid; while at the other surface its concentration is greatest. In the latter case the condition is of the nature of adsorption. The condensation on that portion of the surface where the tension is least is responsible for what we find when a solution of a coloured salt, as, e.g., potassium permanganate, is driven through a layer of dry sand. If the latter is of some considerable thickness the fluid as it passes out is colourless. The air-solution surface tension is higher than the tension of each of the solution-sand surfaces on which, therefore, the permanganate condenses or is adsorbed. The same phenomenon is observed when a long strip of filter paper is allowed to hang with its lower end in contact with a moderately dilute solution of a copper salt. The solution is imbibed by the filter paper, and it ascends a certain distance in a couple of minutes, when it may be found that the uppermost portion of the moist area is free from even a trace of copper salt.

If, on the other hand, an organic compound—as, for instance, one of the bile salts—instead of an inorganic compound is dissolved in the fluid, the surface tension of the air-water surface is reduced, and in consequence the bile salt is

concentrated at that surface; while in the remainder of the fluid, and particularly in that portion of it in contact with the wall of the vessel, the concentration is reduced.

The distribution of a salt in such a fluid, whether it lowers surface tension or increases it, is due to the action of a law which may be expressed in words to the effect that the concentration in a system is so adjusted as to reduce the energy at any point to a minimum.

Our knowledge of this action of inorganic and organic substances on the surface tension in a fluid and of the differences in their concentrations throughout the latter was contained in the results of the observations on gas mixtures by J. Willard Gibbs published in 1878. The principle as applied to solutions was independently discovered by J. J. Thomson in 1887. It is known as the Gibbs' principle, although the current enunciations of it contain the more extended observations of Thomson. As formulated usually it is more briefly given, and its essential points may be rendered in the statement *that when a substance on solution in a fluid lowers the surface tension of the latter the concentration of the solute is greater in the surface layer than elsewhere in the solution; but when the substance dissolved raises the surface tension of the fluid, the concentration of the solute is least in the surface layers of the solution.*

It is thus seen how in a system like that of a drop of water with different contact surfaces the surface tension is affected and how this alters the distribution of solutes. It is further to be noted that for most organic solutes the action in this respect is the very reverse of that of inorganic salts. Consequently, in a living cell which contains both inorganic and organic solutes, and in which there are portions of different composition and density, the equilibrium may be subject to disturbance constantly through an alteration of the surface tension at any point. Such a disturbance may be found in a drop of an emulsion of olive oil and potassium carbonate in the well-known experiments of Bütschli. When the emulsion is appropriately prepared, a minute drop of it, after it is surrounded with water, will creep under the cover glass in an amoeboid fashion for hours, and the movement will be more marked and rapid when the temperature is raised to 40 to 50° C. All the phenomena manifested are due to a lowering of the surface tension at a point on the surface, as a result of which there is protrusion there of the contents of the drop, accompanied, Bütschli holds, by streaming cyclic currents in the remainder of the mass.

Surface tension also, according to J. Traube, is all-important in osmosis, and he holds that it is the solution pressure (*Haftdruck*) of a substance which determines the velocity of the osmotic movement and the direction and force of the osmotic pressure. The solution pressure of a substance is measured by the effect that substance exercises when dissolved on the surface tension of its solution, or, to put it in Traube's own way, the more a substance lowers or raises the surface tension of a solvent (water) the less or greater is the solution pressure (*Haftdruck*) of that substance. This solution pressure, Traube further holds, is the only force controlling osmosis through a membrane, and he rejects completely the bombardment effect on the septum postulated in the van 't Hoff theory of osmosis.

The question as to the nature of the factors concerned in osmosis must remain undecided until the facts have been more fully studied from the physiological standpoint, but enough is now known to indicate that surface tension plays at least a part in it, and the omission of all consideration of it as a factor is not by any means a negligible defect in the van 't Hoff theory of osmosis.

The occurrence of variations in surface tension in the individual cells of an organ or tissue is difficult to demonstrate directly. We have no methods for that purpose, and, in consequence, one must depend on indirect ways to reveal whether such variations exist. The most effective of these is to determine the distribution of organic solutes and of inorganic salts in the cell. The demonstration of the former is at present difficult or even in some cases impossible. The occurrence of soaps which are amongst the most effective agents in lowering surface tension may be revealed without difficulty microchemically, as may also neutral fats, but we have as yet no delicate microchemical tests for sugars, urea, and other nitrogenous metabolites, and in consequence the part they play, if any, in altering the surface tension in different kinds of cells, is unknown. Further research may, however, result in discovering methods of revealing their occurrence micro-

chemically in the cell. We are in a like difficulty with regard to sodium, whose distribution we can determine microchemically in its chief compounds, the chloride and phosphate, only after the exclusion of potassium, calcium, and magnesium. We have, on the other hand, very sensitive reactions for potassium, iron, calcium, haloid chlorine, and phosphoric acid, and with methods based on these reactions it is possible to localise the majority of the inorganic elements which occur in the living cell.

By the use of these methods we can indirectly determine the occurrence of differences in surface tension in a cell. This determination is based on the deduction from the Gibbs-Thomson principle that, where in a cell an inorganic element or compound is concentrated, the surface tension at the point is lower than it is elsewhere in the cell. If, for example, it is concentrated on one wall of a cell the surface tension there is less than on the remaining surfaces or walls of the cell. The thickness of this layer must vary with the osmotic concentration in the cell, with the specific composition of the colloid material of the cytoplasm and with the activity of the cell, but it should not exceed a few hundredths of a millimetre (0.02–0.04 mm.), while it might be very much less in an animal cell whose greatest diameter does not exceed $20\ \mu$.

Numerous examples of such localisation may be observed in the confervoid Protophyta. In *Ulothrix*, ordinarily, there is usually a remarkable condensation of the potassium at the ends of the cell on each transverse wall. The surface tension, on the basis of the deduction from the Gibbs-Thomson principle, should be, in all these cases, high on the lateral walls and low on those surfaces adjoining the transverse septa.

The use of this deduction may be extended. There are in cells various inclusions whose composition gives them a different surface tension from that prevailing in the external limiting area of the cell. Further, the limiting portion of the cytoplasm in contact with these inclusions must have surface tension also. When, therefore, we find by microchemical means that a condensation of an inorganic element or compound obtains immediately within or without an inclusion, we may conclude that there, as compared with the external surface of the cell, the surface tension is low. It may be urged that the condensation is due to adsorption only, but this objection cannot hold, for in the Gibbs-Thomson phenomena the localisation of the solute at a part of the surface as the result of high tension elsewhere of the solution is, in all probability, due to adsorption, and is indeed so regarded.¹

It is in this way that we can explain the remarkable localisation of potassium in the cytoplasm at the margins of the chromatophor in *Spirogyra* and also the extraordinary quantities of potassium held in or on the inclusions in the mesophyll cells of leaves. In Infusoria (*orticella*, *Paramoecium*) the potassium present apart from that in the stalk or ectosarc is confined to one or more small granules or masses in the cytoplasm.

How important a factor this is in clearing the active portion of the cytoplasm of compounds which might hamper its action, a little consideration will show. In plants very large quantities of salts are carried to the leaves by the sap from the roots, and among these salts those of potassium are the most abundant as a rule. Reaching the leaves these salts do not return, and in consequence during the functional life of the leaves they accumulate in the mesophyll cells in very large quantities, which, if they were not localised as described in the cell, would affect the whole cytoplasm and alter its action.

Enough has been advanced here to indicate that surface tension is not a minor feature in cell life. I would go even farther than this and venture to say that the energy evolved in muscular contraction, that also involved in secretion and excretion, the force concerned in the phenomena of nuclear and cell division, and that force also engaged by the nerve cell in the production of a nerve impulse are but manifestations of surface tension. On this view the living cell is but a machine, an engine, for transforming potential into kinetic and other forms of energy, through or by changes in its surface energy.

To present an ample defence of all the parts of the thesis just advanced is more than I propose to do in this address. That would take more time than is customarily allowed on such an occasion, and I have, in consequence, decided to confine my observations to outlines of the points as specified.

It is not a new view that surface tension is the source of the muscular contraction. As already stated, the first to apply the explanation of this force as a factor in cellular movement was Engelmann in 1869, who advanced the view that those changes in shape of cells which are classed as contractile are all due to that force which is concerned in the rounding of a drop of fluid. The same view was expressed by Rindfleisch in 1880, and by Berthold in 1886, who explained the protoplasmic streaming in cells as arising in local changes of surface tension between the fluid plasma and the cell sap, but he held that the movement and streaming of *Amœbe* and *Plasmodie* are not to be referred to the same causes as operate in the protoplasmic streaming in plant cells. Quincke in 1888 applied the principle of surface tension in explaining all protoplasmic movement. In his view the force operates, as in the distribution of a drop of oil on water, in spreading protoplasm, which contains oils and soaps, over surfaces in which the tension is greater, and as soap is constantly being formed, the layer containing it, having a low tension on the surface in contact with water, will as constantly keep moving, and as a result pull the protoplasm with it. The movement of the latter thus generated will be continuous and constitute protoplasmic streaming. In a similar way Bütschli explains the movement of a drop of soap emulsion, the layer of soap at a point on the surface of the spherule dissolving in the water and causing there a low tension and a streaming of the water from that point over the surface of the drop. This produces a corresponding movement in the drop at its periphery and a return central or axial stream directed to the point on the surface where the solution of the soap occurred and where now a protrusion of the mass takes place resembling a pseudopodium. In this manner, Bütschli holds, the contractile movements of *Amœbe* are brought about. In these the chylema or fluid of the foam-like structure in the protoplasm is alkaline, it contains fatty acids and, in consequence, soaps are present which, through rupture of the superficial vesicles of the foam-like structure at a point, are discharged on the free surface and produce there the diminution of surface tension that calls forth currents, internal and external, like those which occur in the case of the drop of oil emulsion.

The first to suggest that surface tension is a factor in muscular contraction was D'Arsonval, but it was Imbert who, in 1897, directly applied the principle in explanation of the contractility of smooth and striated muscle fibre. In his view the primary conditions are different in the former from what obtain in the latter. In smooth muscle fibre the extension is determined, not by any force inside it, but by external force such as may distend the organ (intestine, bladder, and arteries) in whose wall it is found. The 'stimulus' which causes the contraction increases the surface tension between the surface of the fibre and the surrounding fluid, and this of itself has the effect of making the fibre tend to become more spherical or shorter and thicker, which change in shape does occur during contraction. He did not, however, explain how the excitation altered the surface tension, except to say that its effect on surface tension is like that of electricity, with which the nerve impulse presents some analogy. In striated fibre, on the other hand, the discs constituting the light and dim bands have each a longitudinal diameter which is an effect of its surface tension, and this causes extension of the fibre during rest. When a nerve impulse reaches the fibre the surface tension of the discs is altered and there results a deformation of each involving a shortening of its longitudinal axis and thus a shortening of the whole fibre.

According to Bernstein, in both smooth and striated muscle fibre there is, in addition to surface tension, an elastic force residing in the material composing the fibre which, according to the conditions, sometimes opposes and sometimes assists the surface tension. The result is that in the muscle fibre at rest the surface must exceed somewhat that of the fibre in contraction. In both conditions the sum of the two forces, surface tension and elasticity, must be zero. In contraction the surface tension increases and with it the elasticity also. Taken as a whole this would not explain the large force generated in contraction, for the energy liberated would be the product of the surface tension and the amount representing the diminution of the surface due to the contraction. As the latter is very small the product is much below the amount of energy in the form of work done actually manifested. To get over this difficulty Bernstein postulates that in muscle fibres, whether smooth or striated, there are fibrils surrounded by sarco-plasma, and that each fibril is formed of a number of cylinders or biaxial

ellipsoids singly disposed in the course of the fibril, but separated from each other by elastic material and surrounded by sarcoplasma. Between the ellipsoids and the sarcoplasma there is considerable surface tension which prevents mixture of the substances constituting both. The excitation through the nerve impulse causes an increase of surface tension in these ellipsoids, and they become more spherical. In consequence the decrease in surface of all the ellipsoids constituting a fibril is much greater than if the fibril were to be affected as an individual unit only by an increase of surface tension, and thus the surface energy developed would be correspondingly greater. The ellipsoids, Bernstein explains, are not to be confused with the discs, singly and doubly refractive in striated fibre; for these, he holds, are not concerned in the generation of the contraction but with the processes that make for rapidity of contraction. The extension of a muscle after contraction is due to the elastic reaction of the substance between the ellipsoids in the fibrils. Bernstein further holds that fibrils of this character occur in the protoplasm of *Amœba*, in the stalk of *Forficella*, and in the ectoplasma of *Stentor*, and this explains their contractility.

It may be said in criticism of Bernstein's view that his ellipsoids are from their very nature non-demonstrable structures and, therefore, must always remain as postulated elements only. Further, it may be pointed out that he attributes too small a part to surface tension in the lengthening of the fibre after contraction, and that the elasticity which muscle appears to possess is, in the last analysis, but a result of its surface tension.

As regards Quincke's explanation of protoplasmic movement and streaming, as well as of muscular contraction, Bütschli has shown that it is based on a mistaken view of the structure of the cell in *Chara* and other plant forms in which protoplasmic streaming occurs. Bütschli's own hypothesis, however, is defective in that it postulates a current in the fluid medium just outside the *Amœba* and backward over its surface, the existence of which Berthold denies, and Bütschli himself has been unable to demonstrate, even with the aid of fine carmine powder in the fluid. He did, indeed, observe a streaming in the water about a creeping *Pelomyxa*, but the current was in the opposite direction to that demanded by his hypothesis. Further, his failure to demonstrate the occurrence of the postulated backflow in the water about the contracting or moving mass of an *Amœba* or a *Pelomyxa*, makes it difficult to accept the hypothesis he advanced to explain that backflow, namely, that rupture of peripheral vesicles (*Waben*) of the protoplasm occurs with a consequent discharge of their contents (proteins, oils, and soaps) into the surrounding fluid. Surface tension, further, on this hypothesis would be an uncertain and wasteful factor in the life of the cell. On *a priori* grounds also it would seem improbable that this force should be generated outside instead of inside the cell.

One common defect of all these views is that they made only a limited application of the principle of surface tension. This was because some of its phenomena were unknown and especially those illustrating the Gibbs-Thomson principle. With its aid and with the knowledge of the distribution of inorganic constituents in animal and vegetable cells that microchemistry gives us we can make a more extended application of surface tension as a factor in cellular life than was possible ten years ago.

In regard to muscle fibre this is particularly true, and microchemistry has been of considerable service here. From the analyses of the inorganic constituents of striated muscle in vertebrates made by J. Katz and others we know that potassium is extraordinarily abundant therein, ranging from three and a half in the dog to more than fourteen times in the pike the amount of sodium present. How the potassium salt is distributed in the fibre was unknown before 1904, in which year, by the use of a method, which I had discovered, of demonstrating the potassium microchemically, the element was found localised in the dim bands. Later and more extended observations suggested that in the dim band itself, when the muscle fibre is at rest, the potassium is not uniformly distributed, and it was found to be the case in the wing muscles of certain of the insects—as, for example, the scavenger beetles—in which the bands are broad and conspicuous enough to permit ready observation on this score. In these the potassium salt was found to be localised in the zones of each dim band adjacent to each light band. Subsequently Miss M. L. Menten, working in my laboratory and using the same microchemical method, found the potassium similarly

limited in its distribution in the muscle fibres of a number of other insects. She determined, also, that the chlorides and phosphates have a like distribution in these structures, and it is consequently probable that sodium, calcium, and magnesium have the same localisation.

Macdonald has also made investigations on the distribution of potassium in the muscle fibre of the frog, crab, and lobster, using for this purpose the hexanitrite reagent. He holds, as a result of his observations, that the element in the uncontracted fibril is limited to the sarcoplasm in the immediate neighbourhood of the singly refractive substance, while it is abundantly present in the central portion of each sarcomere of the contracted fibril—that is, in the doubly refractive material. I am not inclined to question the former point, as I have not investigated the microchemistry of the muscle in the crab and lobster, and my only criticism would be directed against placing too great reliance on the results obtained in the case of frog's muscle. The latter is only very slowly penetrated by the hexanitrite reagent, and, apparently because of this, alterations in the distribution of the salts occur and, as I have observed, the potassium may be limited to the dim bands of one part of the contracted fibre and may be found in the light bands of another part of the same. In the wing muscles of insects in the uncontracted condition such disconcerting results are not so readily obtained, owing, it would seem, to the readiness with which the fibrils may be isolated and the almost immediate penetration of them by the reagent. Here there is no doubt about the occurrence of the element in the zones of the dim band immediately adjacent to the light bands.

Whether the potassium in the resting fibre is in the sarcoplasm or in the sarcostyle I would hesitate to say. It may be as Macdonald claims, but I find it difficult to apply in microchemical studies of muscle fibre the concepts of its more minute structure gained from merely stained preparations. Because of this difficulty I have refrained from using here, as localising designations, other expressions than 'light bands' and 'dim bands.' The latter undoubtedly include some sarcoplasm, but in the case of the resting fibre I am certain only of the presence of potassium, as described, in the dim band regarded as an individual part, and not as a composite structure.

Now, on applying the Gibbs-Thomson principle enunciated above, this distribution would seem to indicate that in the dim band of a fibril the surface tension is greatest on its lateral walls, in consequence of which the potassium salts are concentrated in the vicinity of the remaining surfaces, *i.e.*, those limiting the light bands. This explanation would seem to be confirmed by the observations I made on the contracted fibrils of the wing muscles of a scavenger beetle. In these the potassium was found uniformly distributed throughout each dim band, which, instead of being cylindrical in shape as in the resting element, is provided with a convexly curved lateral wall, and therefore with a smaller surface than the mass of the dim band has when at rest. This contour suggests that the surface tension on the lateral wall is lessened to an amount below that of either terminal surface, followed by a redistribution of the potassium salt to restore the equilibrium thus disturbed. The consequent shortening of the dim bands of the fibrils would account for the contraction of the muscle.

How the surface tension of the lateral wall of the dim band is lessened in contraction is a question which can only be answered after much more is known of the nature of the nerve impulse as it reaches the muscle fibril, and of the part played by the energy set free in the combustion process in the dim bands. It may be that electrical polarisation, as a result of the arrival of the nerve impulse, develops on the surface of the lateral wall, and as a consequence of which its surface tension is diminished. The energy so lost appears as work, and it is replaced by energy, one may suppose, derived from the combustion of the material in the dim band. In this case the disturbance of surface tension would be primary, while the combustion process would be secondary, in the order of time. In support of this explanation may be cited the fact that the current of action in muscle precedes in time the contraction itself—that is, the electrical response of the stimulus occurs in the latent period and immediately before the contraction begins.

It may, however, be postulated, on the other hand, that the chemical changes occur in those parts of the dim band immediately adjacent to the light bands,

and as a result the tension of the terminal surfaces may be increased, this resulting in the shortening of the longitudinal axis of the dim band and the displacement laterally of the contents. This would imply that the energy of muscle contraction comes primarily from that set free in the combustion process, and not indirectly as involved in the former explanation.

Whatever may be the cause of the alteration in surface tension, there would seem to be no question of the latter. The very alteration in shape of the dim band in contraction makes it imperative to believe that surface tension is concerned. The redistribution of the potassium which takes place as described in the contracting fibrils of the wing muscles of the scavenger beetle can be explained in no other way than through the alteration of surface tension.

In the smooth muscle fibre potassium is also present and in close association throughout with the membrane. When a fresh preparation of smooth muscle is treated so as to demonstrate the presence of potassium, the latter is shown in the form of a granular precipitate of hexanitrite of sodium, potassium, and cobalt in the cement substance between the membranes of the fibres. In the smooth muscle fibres in the walls of the arteries in the frog the precipitate in the cement material is abundant, and its disposition suggests that it plays some part in the rôle of contraction. Inside of the membrane potassium occurs, but in very minute quantities, which, with the cobalt sulphide method, gives a just perceptible dark shade to the cytoplasm as a whole. Microchemical tests for the chlorides and phosphates indicate that the cytoplasm is almost wholly free from them, and consequently there is very little inorganic material inside of the fibre. Chlorides and phosphates, but more particularly the former, are abundant in the cement material, and their localisation here would seem to indicate that the potassium of the same distribution is combined chiefly as chloride.

In smooth muscle fibre, then, the potassium is distributed very differently from what it is in striated fibre, and on first thought it seemed difficult to postulate that the contraction could be due to alterations of surface tension. This, however, would appear to be the most feasible explanation, for the potassium salts in the cement substance might be supposed to shift their position under the influence of electrical force so as to reach the interior of the membranes of the fibres, in which case the surface tension of the latter would be immediately increased and the fibre itself would in consequence at once begin to contract. The slowness with which this shifting into, or absorption by, the membrane of the potassium salts would take place would also account for the long latent period of contraction in smooth muscle.

It is of interest here to note that the potassium ions have the highest ionic mobility (transport number) of all the elements of the kationic class, except hydrogen, which are found to occur in connection with living matter. Its value in this respect is half again as great as that of sodium, one-eighth greater than that of calcium, and one-seventh greater than that of magnesium. This high migration velocity of potassium ions would make the element of special service in rapid changes of surface tension.

Loew has pointed out that potassium in the condensation processes of the synthesis of organic compounds has a catalytic value different from that of sodium. For example, ethyl aldehyde is condensed with potassium salts to aldol, with sodium salts to crotonic aldehyde (Kopf and Michael). Potassium is, but sodium is not, effective in the condensation of carbon monoxide. When phenol is fused with potassium salts condensation products like diphenol are produced, but when sodium salts are used the products are dioxybenzol and phloroglucin (Barth). It is, therefore, not improbable that potassium, along with those properties which come from its ionic mobility, has a special value in the metabolism of the dim bands of striated muscle fibre and in the condensation synthesis which characterise the chromatophors of *Protohyta* (*Spirogyra*, *Zygnema*).

With the use of this method of determining differences in surface tension in cells it is possible, in some cases at least, to ascertain whether this force plays a part in both secretion and excretion, and evidence in favour of this view can be found in the pancreatic cells of the rabbit, guinea pig, and in the renal cells of the frog. In the pancreatic cells there is an extraordinary condensation of potassium salts in the cytoplasm of each cell adjacent to the lumen of the tubule, and

during all the phases of activity—except, it would appear, that of the so-called “resting stage”—potassium salts occur in, and are wholly confined to, this part of each cell. It is difficult to say whether they pass into the lumen with the secretion and their place taken by more from the blood-stream and lymph, but the important point is that the condensation of potassium salts immediately adjacent to the lumen seems to indicate a lessened surface tension on the lumen surface of the cell.

According to Stoklasa¹ the pancreas of the pig is much richer in potassium than in sodium, the dried material containing 2.09 per cent. of potassium and 0.28 per cent. of sodium, while the values for the dried material of ox muscle are, as he determined them, 1.82 and 0.26 per cent. respectively. It is significant that in the pancreas this large amount of potassium should be localised as described.

In the renal cells of vertebrates there is usually a considerable amount of potassium salts distributed throughout the cytoplasm. These cells are always active in the elimination of the element from the blood, and it is in consequence not possible to determine whether there are differences in surface tension in them. Under certain conditions, however, these can be demonstrated. In the frogs which have been kept in the laboratory tanks throughout the winter, and in the blood of which the inorganic salts have been, because of the long period of inanition, reduced to almost hypotonic proportions, the renal cells are very largely free from potassium. When it is present it is usually diffused throughout the cytoplasm. If now a few cubic centimetres of a decinormal solution of potassium chloride be injected into the dorsal lymph sacs of one of these frogs, and after twenty minutes the animal is killed, appropriate treatment, with the cobalt reagent, of a thin section of the fresh kidney made by the carbon dioxide freezing method, reveals in the cells of certain of the tubules a condensation of potassium salts in the cytoplasm immediately adjacent to the wall of the lumen. There is also a very slight diffuse reaction throughout the remainder of the cytoplasm, except in that part immediately adjacent to the external boundary of the tubule. In these cells the potassium injected into the lymph circulation is being excreted, and the condensation of the element at or near the surface of the lumen is evidence that there the tension is less than at the other extremity of the cell.

These facts are in their significance in line with some observations that I have made on the absorption of soluble salts by the intestinal mucosa in the guinea-pig. When the ‘peptonate’ of iron was administered in the food of the animal it was not unusual to find that in the epithelial cells of the villi the iron salt was distributed through the cytoplasm, but its concentration, as a rule, was greatest in the cytoplasm adjacent to the inner surface of the cell, from which it diffused into the underlying tissue. Here also, inferentially, surface tension is lower than elsewhere in the cell.

It would perhaps be unwise to form final conclusions at this stage in the progress of the investigation of the subject, but the results so far gained tempt one to adopt as a working hypothesis *that in the secreting or the excreting cell lower surface tension exists at its secreting or excreting surface than at any other point on the cell surface*. How this low surface tension is caused or maintained it is impossible to say, but, whatever the solution of the question may be, it is important to note that we must postulate the participation of this force in renal excretion in order to explain the formation of urines of high concentration. These have a high osmotic pressure, as measured by the depression of the freezing point, while the osmotic pressure of the blood plasma determined in the same way is low. On the principle of osmosis alone, as it is currently understood, this result is inexplicable, for the kinetic energy, as required in the gas theory of solutions, should not be greater, though it might be less, in the urine than in the blood. It is manifest that in the formation of concentrated urines energy is expended. We know also from the investigations of Barcroft and Brodie that the kidney during diuresis absorbs much more oxygen per gram weight than the body generally, and that, assuming it is used in the combustion of a proteid, a very large amount of energy is set free, very much more indeed than is necessary. It has also been observed that a portion of the energy set free is found in a higher temperature in

¹ Stoklasa gave the values in K_2O and Na_2O .

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the excretion than obtains in the blood itself circulating through the kidney. This large expenditure of energy is, probably, a result of the physiological adaptation of the principle of the 'factor of safety,' which, as Meltzer has pointed out, occurs in other organs of the body.

In cell and nuclear division surface tension operates as a force, the action of which cannot be completely understood till we know more of the part played by the centrosomes and centrosphere. That this force takes part in cell reproduction has already been suggested by Brailsford Robertson. He has devised an ingenious experiment to illustrate its action. If a thread moistened with a solution of a base is laid across a drop of oil in which is dissolved some free fatty acid the drop divides along the line of the thread. When the latter is moistened with soap the drop divides in the same way and in the same plane. The soap formed in one case and present in the other, it is explained, lowers the surface tension in the equatorial plane of the drop, and this diminution results in streaming movement away from that plane which bring about the division. He suggests that in cell division there is a liberation of soaps in the plane of division which set up streaming movements from that plane towards the poles and terminating in the division of the cytoplasm of the cell.

I have observed in the cells of *Zygnema* about to divide a remarkable condensation of potassium in the plane of division. In the 'resting' cell of this *Alga* the potassium is, as a rule, more abundant in the cytoplasm near the transverse walls of the thread, and only traces of the element are to be found along the line of future division of the cell. But immediately after division has taken place the potassium is concentrated in the plane of division. This would seem to indicate that surface tension in the plane of division is, as postulated by the deduction from the Gibbs-Thomson principle, lower than it is on the longitudinal surface, and lower, especially, than it is on the previously formed transverse septa of the thread.

One must not, however, draw from this the conclusion that in all dividing cells surface tension is lower in the plane of division than it is elsewhere on the surface of the dividing structure. All that it means is that in the dividing cell of *Zygnema* the condition already exists along the plane of division, which subsequently makes for low surface tension in the cell membrane immediately adjacent to each transverse septum in the coniferoid thread. If the evidence of low surface tension vanished immediately after division was complete, then it might be held that it determined the division. As it is, the low surface tension in this case is the result and not the cause of the division.

This conclusion is corroborated by the results of observations on the cells of the ovules of *Lilium* and *Tulipa*. The potassium salts in these are found condensed in minute masses throughout the cytoplasm. When division is about to begin the salts are shifted to the peripheral zone of the cytoplasm, and when the nuclear membrane disappears not a trace of potassium is now found in the neighbourhood of the free chromosomes, a condition which continues till after nuclear division is complete. The absence of potassium, the most abundant basic element in the cytoplasm, would indicate that soaps are not present, and appropriate treatment of such cells, hardened in formaline only, with Scarlet Red demonstrates that fats, including lecithins, are absent also. This would seem to show that high instead of low surface tension prevails about the nucleus during division. During the 'resting' condition of the nucleus this high tension is maintained, for, except in very rare cases, and these of doubtful character, there is no condensation of inorganic salts in the neighbourhood or on the surface of the nuclear membrane. It is also to be noted that the nucleus, with exceptions, the majority of which are found in the Protozoa, is of spherical shape, which also postulates that high surface tension obtains either in the cytoplasmic layer about the nucleus or in the nuclear membrane itself. It may also be suggested that high surface tension, and not the physical impermeability of the nuclear membrane, is the reason why the nucleus is, as I have often stated, wholly free from inorganic constituents.

It does not follow from all this that surface tension has nothing to do with cell division. If, as Brailsford Robertson holds, surface tension is lowered in the plane of division, then the internal streaming movement of the cytoplasm of each half of the cell should be towards that plane, and, in consequence, not separation but fusion of the two halves would result. The lipoids and soaps would indeed spread superficially on the two parts from the equatorial plane towards the two

poles, and, according to the Gibbs-Thomson principle, they would not distribute themselves through the cytoplasm in the plane of division, except as a result of the formation of a septum in that plane. In other words, the septum has first to exist in order to allow the soaps and lipoids to distribute themselves in a streaming movement over its two faces. In Brailsford Robertson's experiment this septum is provided in the thread. If, on the other hand, surface tension is higher about the nucleus in and immediately adjacent to the future plane of division, then constriction of the nucleus in that plane will take place accompanied or preceded by an internal streaming movement in each half towards its pole and a consequent traction effect on the chromosomes which are thus removed from the equatorial plane. When nuclear division is complete then a higher surface tension on the cell itself limited to the plane of division would bring about there a separation of the two halves, a consequent condensation on each side of that plane of the substances producing the low tension elsewhere, and thereby also the formation of the two membranes in that plane.

In support of this explanation of the action of surface tension as a factor in division I have endeavoured to ascertain if, as a result of the Gibbs-Thomson principle, there is a condensation of potassium salts in the cytoplasm at the poles of a dividing cell, that is, where surface tension, according to my view, is low. The difficulty one meets here is that, in the higher plant forms, cells preparing to divide appear to be much less rich in potassium than those in the 'resting' stage, and under this condition it is not easy to get unambiguous results, while in animal cells potassium may even in the resting cell be very minute in quantity, as, for example, in *Porticella*, in which, apart from the contractile stalk, it is limited to one or two minute flecks in the cytoplasm. Instances of potassium-holding cells undergoing division are, however, found in the spermatogonia of higher vertebrates (rabbit, guinea-pig), and in these the potassium is gathered in the form of a minute, and thin caplike layers at each pole of the dividing cell.

This of itself would appear to show that surface tension is less in the neighbourhood of the poles than at the equator of the dividing cell, but I am not inclined to regard the fact as conclusive, and a very large number of observations to that end must be made before certainty can be attained. I am, nevertheless, convinced that it is only in this way that we can finally determine whether differences of surface tension in dividing cells account, as I believe they do, for all the phenomena of cell division. The difficulties to be encountered in such an investigation are, as experience has shown me, much greater than are to be overcome in efforts to study surface tension in cells under other conditions, but I am in hopes that what I am now advancing will influence a number of workers to take up research in microchemistry along this line.

I must now discuss surface tension in nerve cells and nerve fibres. I have stated earlier in this address that I hold that the force concerned in the production of the nerve impulse by the nerve cell is surface tension. The very fact that in the repair of a divided nerve fibre the renewal of the peripheral portion of the axon occurs through a movement—a flowing outward, as it were—of the soft colloid material from the central portion of the divided fibre is, in itself, a strong indication that surface tension is low here and high on the cell body itself. This fact does not stand alone. I pointed out six years ago that potassium salt is abundant along the course of the axon and apparently on its exterior surface, while it is present but in traces in the nerve cell itself. In the latter chlorides also are present only in traces, and therefore sodium, if present, is there in more minute quantities, while haloid chlorine is abundant in the axon. Macdonald has also made observations as to the occurrence of potassium along the course of the axon, and has in the main confirmed mine. We differ only as to mode of the distribution of the element in the axon, and the manner in which it is held in the substance of the latter; but, whichever of the two views may be correct, it does not affect what I am now advancing. Extensive condensation or adsorption of potassium salts in or along the course of the axon, while the nerve cell itself is very largely free from them, can have but one explanation on the basis of the Gibbs-Thomson principle, and that explanation is that surface tension on the nerve cell itself must be high while it is low on or in its axon.

The conclusions that follow from this are not far to seek. We know that an electrical displacement or disturbance of ever so slight a character occurring at

a point on the surface of a drop lowers correspondingly the surface tension at that point. What a nerve impulse fundamentally involves we are not certain, but we do know that it is always accompanied by, if not constituted of, a change of electrical potential, which is as rapidly transmitted as is the impulse. When this change of potential is transmitted along an axon through its synaptic terminals to another nerve cell, the surface tension of the latter must be lowered to a degree corresponding to the magnitude of the electrical disturbance produced, and, in consequence, a slight displacement of the potassium ions would occur at each point in succession along the course of its axon. This displacement of the ions as it proceeded would produce a change of electrical potential, and thus account for the current of action. The displacement of the ions in the axon would last as long as the alteration of surface tension which gave rise to it, and this would comprehend not more than a very minute fraction of a second. Consequently, many such variations in the surface tension of the body of the nerve cell would occur in a second; and, as the physical change concerned would involve only the very surface layer of the cell, a minimum of fatigue would result in the cell, while little or none would develop in the axon.

It may be pointed out that in medullated nerve fibres the lipid-holding sheath, in close contact as it is with the axon, must of necessity maintain on the course of the latter a surface tension low as compared with that on the nerve cell itself, which, as the synaptic relations of other nerve cells with it postulate, is not closely invested with an enveloping membrane. In non-medullated nerve fibres the simple enveloping sheath may function in the same manner, and probably, if it is not rich in lipid material, in a less marked degree.

What further is involved in all this, what other conclusions follow from these observations, I must leave unexplained. It suffices that I have indicated the main points of the subject, the philosophical significance of which will appear to those who will pursue it beyond the point where I leave it.

In bringing this address to a close I am well aware of the fact that my treatment of the subjects discussed has not been as adequate as their character would warrant. The position which I occupy imposes limits, and there enters also the personal factor to account in part for the failure to achieve the result at which I aimed. But there is, besides, the idea that in applying the laws of surface tension in the explanation of vital phenomena I am proceeding along a path into the unknown which has been as yet only in a most general way marked out by pioneer investigators, and in consequence, to avoid mistakes, I have been constrained to exercise caution, and to repress the desire to make larger ventures from the imperfectly beaten main road. Perhaps, after all, I may have fallen into error, and I must therefore be prepared to recall or to revise some of the views which I have advanced here, should they ultimately be found wanting. That, however, as I reassure myself, is the true attitude to take. It is a far cry to certainty. As Duclaux has aptly put it, the reason why Science advances is that it is never sure of anything. Thus I justify my effort of to-day.

Notwithstanding this inadequate treatment of the subject of surface tension in relation to cellular processes, I hope I have made it in some measure clear that the same force which shapes the raindrop or the molten mass of a planet is an all-important factor in the causation of vital phenomena. Some of the latter may not thereby be explained. We do not as yet know all that is concerned in the physical state of solutions. The fact, ascertained by Rona and Michaelis, that certain sugars, which neither lower nor appreciably raise surface tension in their solutions, condense or are adsorbed on the surface of a solution system, is an indication that there are at least some problems with a bearing on vital phenomena yet to solve. Nevertheless, what we have gained from our knowledge of the laws of surface tension constitutes a distinct step in advance, and a more extended application of the Gibbs-Thomson principle may throw light on the causation of other vital phenomena. To that end a greatly developed science of microchemistry is necessary. This should supply the stimulus to enthusiasm in the search for reactions that will enable us to locate with great precision in the living cell the constituents, inorganic and organic, which affect its physical state and thereby influence its activity.

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ADDRESS TO THE BOTANICAL SECTION

BY

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PRESIDENT OF THE SECTION.

THE honour conferred in the election to be President for the year of the Botanical Section of the British Association imposes the duty of preparing an address. I trust that my selection of a subject will not be attributed by anyone to a want of appreciation of the worth and importance of certain sides of botanical research to which I shall have less occasion to refer. These have been eloquently supported by former Presidents, and I take this opportunity to express the thanks I owe for the benefit received from their contributions to the advancement of the science of botany. They have told us of the advance in departments of which they could speak as leaders in research, and I do not venture to follow in their steps. My subject is from a field in which I have often experienced the hindrances of which I shall have to speak, both in personal work and still more as a teacher of students, familiar with the many difficulties that impede the path of those who would gladly give of their best, but find the difficulties for a time almost insurmountable, and who are too frequently unable to spare the time or labour to allow of their undertaking scientific investigations that they might well accomplish, and in which they would find keen pleasure under other conditions. Those whose tastes lie in the direction of studying plants in the field rather than in the laboratory are apt to find themselves hampered seriously if they seek to become acquainted with the plants of their own vicinity; and, if they wish to undertake investigations in the hope of doing what they can to advance botanical science, they may find it scarcely possible to ascertain what has been already done and recorded by others.

For a time the knowledge of plants was too much confined to the ability to name them according to the system in vogue and to a knowledge of their uses, real or imagined. The undue importance attached to this side of the study, even by so great a leader as Linnaeus, naturally led to a reaction as the value of other aspects of botany came to be realised, and as improvements in the instruments and methods of research opened up new fields of study. The science has gained much by the reaction; but there is danger of swinging to the other extreme and of failing to recognise the need to become well acquainted with plants in their natural surroundings. The opportunities for study in the laboratory are so great and so much more under control, and the materials are so abundant and of so much interest, that there is for many botanists a temptation to limit themselves to such work, or at least to regard work in the field as subordinate to it and of

little value. It is scarcely necessary to point out that each side is insufficient alone. Yet some find more pleasure in the one side, and do well to make it their chief study; while they should recognise the value of the other also, and learn from it.

It is especially on behalf of the work in the field that I now wish to plead. There are few paths more likely to prove attractive to most students. The study of the plants in their natural environments will lead to an understanding of their nature as living beings, of their relations to one another and to other environments, of the stimuli to which they respond, and of the struggle for existence that results in the survival of certain forms and the disappearance of others. In this way also will be gained a conception of the true meaning and place of classification as an indispensable instrument for accurate determination and record, and not as an end in itself. To one that has once gained a true insight into the pleasure and worth of such studies, collections made for the sake of mere possession or lists of species discovered in a locality will not suffice. Many questions will arise which will prove a constant source of new interest. From such studies a deep and growing love for botany has in not a few cases arisen.

The British flora has interested me for upwards of forty years, and has occupied much of my attention during that time— not only as desirous to aid by my own efforts to extend our knowledge of it, but also, as a teacher, seeking to assist my students to become able to do their parts also, and making use of the materials within reach to enable me to help them. Thus our present knowledge of the plants of our own country has become known to me, and the difficulties of acquiring that knowledge have also become known through both my own experience and those of my students. The nature of the hindrances and difficulties that at present bar the way has also become familiar, as well as the steps to be taken to clear some of them away and to make the path less difficult to those who come after us; and I have also gained a fairly good acquaintance with the means at the command of students of the floras of other countries, so as to have a standard for comparison in the estimate to be formed of the condition of matters in our own country.

In how far is the present provision for the study of the flora of the British Islands sufficient and satisfactory?

I venture to hope that the subject will be regarded as among those for the consideration of which the British Association was formed, and that a favourable view will be taken of the conclusions which I take this opportunity to lay before you. What, then, is the present provision for the study of our plants? Since the days of Morrison and Ray there have been many workers, especially during the past century; and an extensive literature has grown up, in the form both of books and of papers, the latter more or less comprehensive, in the scientific journals and in the transactions of societies. These papers contain much that is of great value; but, owing to the absence of any classified index, most of the information in it is beyond the reach of anyone, except at the expenditure of much time and labour. The constantly increasing accumulation of new publications makes the need for a classified index always more urgent; for the mass of literature is at present one of the greatest obstacles to the undertaking of new investigations because of the uncertainty whether they may not have been already undertaken and overlooked through want of time or opportunity to search the mass exhaustively.

While the early writers of descriptive floras sought to include every species of plant known to occur in Britain, this has not been attempted during the past seventy or eighty years, and instead of one great work we now have monographs of the greater groups, such as Babington's 'Manual' and Hooker's 'Student's Flora' of the vascular plants, Braithwaite's 'Mossflora,' &c. Local floras still, in a good many cases, aim at including all plants known to grow apparently wild in the districts to which they refer; but they are often little more than lists of species and varieties and of localities in which these have been found. In some, however, there are descriptions of new forms and notes of general value, which are apt to be overlooked because of the place in which they appear.

The early works were necessarily not critical in their treatment of closely allied species and varieties, but they are valuable as giving evidence of what plants were supposed to be native in England when they were published. Even the works that were issued after Linnæus had established the binominal nomenclature

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for a time related almost wholly to England. Sibbald in '*Scotia Illustrata*' (1684) enumerated the plants believed by him to be native in Scotland, and of those then cultivated. Between his book and Lightfoot's '*Flora Scotia*,' published in 1777, very little relating to the flora of Scotland appeared. Irish plants were still later in being carefully studied.

The floras of Hudson, Withering, Lightfoot, and Smith, all of which include all species of known British plants, follow the Linnæan classification and nomenclature in so far as the authors were able to identify the Linnæan species in the British flora. '*English Botany*,' begun in 1795, with plates by Sowerby and text by Smith, was a work of the first rank in its aim of figuring all British plants and in the excellence of the plates; but it shared the defect of certain other great floras in the plates being prepared and issued as the plants could be procured, and thus being without order. Its cost also necessarily put it beyond the reach of most botanists, except those that had the advantage of access to it in some large library. A second edition, issued at a lower price, and with the plants arranged on the Linnæan system, was inferior to the first, in the plates being only partially coloured and in having the text much curtailed. The so-called third edition of the '*English Botany*,' issued 1868-86, is a new work as far as the text is concerned, that being the work of Dr. Boswell Syme, who made it worthily representative of its subject; but the plates, with few exceptions, are reissues of those of the first edition, less perfect as impressions and far less carefully coloured; and this applies with still greater force to a reissue of the third edition a few years ago. This edition, moreover, included only the vascular plants and Characeæ. As this is the only large and fully illustrated British flora that has been attempted, it is almost needless to add that in this respect provision for the study of the flora of our islands is far behind that of certain other countries, and very notably behind that made in the '*Flora danica*.'

Turning next to the provision of less costly aids to the study of British plants, we have manuals of most of the larger groups. The vascular plants are treated of in numerous works, including a considerable number of illustrated books in recent years, inexpensive but insufficient for any but the most elementary students. Fitch's outline illustrations to Bentham's '*Handbook to the British Flora*,' supplemented by W. G. Smith, were issued in a separate volume in 1887, which is still the best for use in the inexpensive works of this kind. Babington's '*Manual*,' on its first appearance in 1843, was gladly welcomed as embodying the result of careful and continued researches by its author into the relations of British plants to their nearest relatives on the Continent of Europe; and each successive issue up to the eighth in 1881 received the careful revision of the author, and contained additions and modifications. In 1904 a ninth edition was edited, after the author's death, by H. and J. Groves; but, though the editors included notes left by Professor Babington prepared for a new edition, they were 'unable to make alterations in the treatment of some of the critical genera which might perhaps have been desirable.' The '*Student's Flora of the British Islands*,' by Sir J. D. Hooker, issued in 1870, took the place of the well-known '*British Flora*' (1830, and in subsequent editions until the eighth in 1860, the last three being issued in collaboration by Sir W. J. Hooker and Professor Walker Arnott). The third edition of the '*Student's Flora*' appeared in 1884, and there has been none since. Mr. F. N. Williams' '*Prodromus Floræ Britannicæ*,' begun in 1901, of which less than one half has yet appeared, though a work of much value and authority, is scarcely calculated for the assistance of the ordinary student; and Mr. Druce's new edition of Hayward's '*Botanist's Pocket Book*' 'is intended merely to enable the botanist in the field to name his specimens approximately, and to refresh the memory of the more advanced worker.' In all the books that are intended for the use of British botanists, apart from one or two recently issued local floras, the classification is still that in use in the middle of last century, even to the extent in the most of them of retaining Conifera as a division of Dicotyledones. Apart from this, the critical study of British plants has led to the detection of numerous previously unobserved and unnamed forms, which find no place in the '*Student's Flora*,' and are only in part noticed in the recent edition of the '*Manual*.'

The '*Lists*' of vascular plants of the British flora that have recently been issued by Messrs. Rendle and Britten, by Mr. Druce, and as the tenth edition of the '*London Catalogue of British Plants*' are all important documents for

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the study of the British flora; but they illustrate very forcibly certain of the difficulties that beset the path of the student eager to gain a knowledge of the plants of his native land. In these lists he finds it scarcely possible to gain a clear idea of how far the species and varieties of the one correspond with those of the other, owing to the diversities of the names employed. It would be a great boon to others as well as to students were a full synonymic list prepared to show clearly the equivalence of the names where those for the same species or variety differ in the different lists and manuals. Probably in time an agreement will be generally arrived at regarding the names to be accepted, but that desirable consummation seems hardly yet in sight. Meantime the most useful step seems to be to show in how far there is agreement in fact under the different names.

Among the Cryptogams certain groups have fared better than the higher plants as regards both their later treatment and their more adequate illustration by modern methods and standards. Several works of great value have dealt with the mosses, the latest being Braithwaite's 'British Moss-flora,' completed in 1899. The Sphagna were also treated by Braithwaite in 1880, and are to be the subject of a monograph in the Ray Society's series. The liverworts have been the subject also of several monographs, of which Pearson's is the fullest.

Among the Thallophyta certain groups have been more satisfactorily treated than others—*e.g.* the Discomycetes, the Uredineæ and Ustilagineæ, the Myxomycetes, and certain others among the fungi, and the Desmidiaceæ among the algae; but the Thallophyta as a whole are much in need of thorough revision to place them on a footing either satisfactory or comparable to their treatment in other countries.

Of the Thallophyta many more of the smaller species will probably be discovered within our islands when close search is made, if we may judge by the much more numerous forms already recorded in certain groups abroad, and which almost certainly exist here also; but among the higher plants it is not likely that many additional species will be discovered as native, yet even among these some will probably be found. It is, however, rather in the direction of fuller investigation of the distribution and tendencies to variation within our islands that results of interest are likely to be obtained.

The labours of H. C. Watson gave a very great stimulus to the study of the distribution of the flora in England and Scotland, and the work he set on foot has been taken up and much extended by numerous botanists in all parts of the British Islands. It is largely owing to such work and to the critical study of the flora necessary for its prosecution that so many additions have been made to the forms previously known as British. Many local works have been issued in recent years, often on a very high standard of excellence. Besides these larger works scientific periodicals and transactions of field clubs and other societies teem with records, some of them very brief, while others are of such size and compass that they might have been issued as separate books. A few of both the books and papers are little more than mere lists of names of species and varieties observed in a locality during a brief visit; but usually there is an attempt at least to distinguish the native or well-established aliens from the mere casuals, if these are mentioned at all. In respect of aliens or plants that owe their presence in a district to man's aid, intentional or involuntary, their treatment is on no settled basis. Every flora admits without question species that are certainly of alien origin, even such weeds of cultivated ground as disappear when cultivation is given up, as may be verified in too many localities in some parts of our country. Yet other species are not admitted, though they may be met with here and there well established, and at least as likely to perpetuate their species in the new home as are some native species.

Comparatively few writers seek to analyse the floras of the districts treated of with a view to determine whence each species came and how, its relation to man, whether assisted by him in its arrival directly or indirectly, whether favoured or harmfully affected by him, its relations to its environment—especially to other species of plants and to animals, and other questions that suggest themselves when such inquiries are entered on. It is very desirable that a careful and exhaustive revision of the British flora should be made on these and similar lines. In such a revision it is not less desirable that each species should be represented by a good series of specimens, and that these should be compared with similar series from other localities within our islands, and from those countries from which it is

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believed that the species originally was sprung. Such careful comparison would probably supply important evidence of forms being evolved in the new environments, differing to a recognisable degree from the ancestral types, and tending to become more marked in the more distant and longer isolated localities. An excellent example of this is afforded by the productive results of the very careful investigation of the Shetland flora by the late Mr. W. H. Beeby.

Within recent years excellent work has been done in the study of plant associations, but the reports on these studies are dispersed in various journals (often not botanical), and are apt to be overlooked by, or to remain unknown to, many to whom they would be helpful. The same is true in large measure of the very valuable reports of work done on plant-remains from peat-mosses, from lake deposits, and from other recent geological formations, researches that have cast such light on the past history of many species as British plants, and have proved their long abode in this country. Mr. Clement Reid's 'Origin of the British Flora,' though published in 1899, has already (by the work of himself and others) been largely added to, and the rate of progress is likely to become still more rapid. Among the fruits and seeds recorded from interglacial and even from preglacial deposits are some whose presence could scarcely have been anticipated, e.g. *Hypocum procumbens*, in Suffolk. Some of the colonists, or aliens now almost confined to ground under cultivation, have been recorded from deposits that suggest an early immigration into the British Islands. While much remains to be discovered, it is desirable that what is already established should find a place in the manuals of British botany.

Apart from the descriptive and topographical works and papers on our flora, there is a serious lack of information gained from the study of our British plants. Although a few types have received fuller study, we have little to compare with the work done in other countries on the structure and histology of our plants, on the effects of environment, on their relations to other species and to animals, and on other aspects of the science to which attention should be directed. On these matters, as on a good many others, we gain most of what information can be had not from British sources, but from the literature of other countries, though it is not wise to assume that what is true elsewhere is equally true here. It is as well, perhaps, that for the present such subjects should find scanty reference in the manuals in ordinary use; but, when trustworthy information has been gained within the British Islands, under the conditions prevailing here, these topics should certainly not be passed over in silence. Students of the British flora have as yet no such works of reference as Raunkjaer's book on the Monocotyledons of Denmark or the admirable 'Lebensgeschichte der Blütenpflanzen Mitteleuropas,' at present being issued by Drs. Kirchner, Loew, and Schröter.

In a complete survey of the British botany there must be included the successive floras of the earlier geological formations, though they cannot as yet be brought into correlation with the recent or existing floras. In the brilliant progress made recently in this field of study our country and the British Association are worthily represented.

The present provision for the study of the British flora and the means that should be made use of for its extension appear to be these:—

Much excellent work has already been accomplished and put on record towards the investigation of the flora, but much of that store of information is in danger of being overlooked and forgotten or lost, owing to the absence of means to direct attention to where it may be found. A careful revision of what has been done and a systematic subject index to its stores are urgently required.

The systematic works treating of the flora are in great part not fully representative of the knowledge already possessed, and require to be brought up to date or to be replaced by others.

Great difficulty is caused by the absence of an authoritative synonymic list that would show as far as possible the equivalence of the names employed in the various manuals and lists. There is much reason to wish that uniformity in the use of names of species and varieties should be arrived at, and a representative committee might assist to that end; but, in the meantime, a good synonymic list would be a most helpful step towards relieving a very pressing obstacle to progress.

There is need for a careful analysis of the flora with a view to determining those species that owe their presence here to man's aid, intentional or uncon-

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scious; and the inquiry should be directed to ascertain the periods and methods of introduction, any tendencies to become modified in their new homes, their subsequent relations with man, and their influence on the native flora, whether direct or by modifying habitats, as shown by *Lupinus nootkatensis* in the valleys of rivers in Scotland.

Those species that there is reason to regard as not having been introduced by man should be investigated as regards their probable origins and the periods and methods of immigration, evidence from fossil deposits of the period during which they have existed in this country, their constancy or liability to show change during this period, their resemblance to or differences from the types in the countries from which they are believed to have been derived, or the likelihood of their having originated by mutation or by slow change within the British Islands, and their relation to man's influence on them (usually harmful, but occasionally helpful) as affecting their distribution and permanence.

The topographical distribution, though so much has been done in this field during the past sixty or seventy years, still requires careful investigation, to determine not merely that species have been observed in certain districts, but their relative frequency, their relations to man (natives of one part of our country are often aliens in other parts), whether increasing or diminishing, altitudes, habitats, &c. From such a careful topographical survey much should be learned of the conditions that favour or hinder the success of species, of the evolution of new forms and their relation to parent types in distribution, especially in the more isolated districts and islands, and of other biological problems of great interest. A most useful aid towards the preparation of topographical records would be afforded by the issue at a small price of outline maps so as to allow of a separate map being employed for recording the distribution of each form.

A careful study of the flora is also required from the standpoint of structure and development, with comparison of the results obtained here with those of workers in other countries where the same or closely allied species and varieties occur. It is also needed in respect of the relations between the plants and animals of our islands, both as observed here and in comparison with the already extensive records of a similar kind in other countries. On such topics as pollination, distribution of seeds, and injuries inflicted by animals and galls produced by animals or plants we have still to make use very largely of the information gained abroad; and the same holds good with regard to the diseases of plants.

While 'English Botany' in its first edition was deservedly regarded as a work of the first rank among floras, it has long been defective as representing our present knowledge of British plants, and it has not been succeeded by any work of nearly equal rank, while other countries now have their great floras of a type in advance of it. There is need for a great work worthy of our country, amply illustrated so as to show not only the habit of the species and varieties, but also the distinctive characters and the more important biological features of each. Such a flora would probably require to be in the form of monographs by specialists, issued as each could be prepared, but as part of a well-planned whole. It should give for each plant far more than is contained in even the best of our existing British floras. Means of identification must be provided in the description, with emphasised diagnostic characters; but there should also be the necessary synonymy, a summary of topographical distribution, notes on man's influence upon distribution, abundance, &c., on any biological or other point of interest in structure or relations to habitat, environment, associated animals or plants, diseases, &c. Local names, uses, and folklore should also be included; and for this the need is all the greater, because much of such old lore is rapidly being forgotten and tends to be lost. In a national flora there should be included an account of the successive floras of former periods, and, as far as possible, the changes that can be traced in the existing flora from its earliest records to the time of issue should be recorded.

A flora of this kind would not only afford the fullest possible information with regard to the plant world of the British Islands at the date of issue, but would form a standard with which it could be compared at later periods, so as to permit of changes in it being recognised and measured. In the meanwhile the production of such a flora can be regarded only as an aim towards which to press on, but which cannot be attained until much has been done. But while the fulfilment must be left to others, we can do something to help it on by trying to remove

difficulties from the way, and to bring together materials that may be used in its construction.

I have sought to call attention to the difficulties that I have experienced and to directions in which progress could be made at once, and to provision which should be made for the advancement of the study of the British flora with as little delay as possible. There is, I feel assured, the means of making far more rapid and satisfactory progress towards the goal than has yet been accomplished. Many persons are interested in the subject, and would gladly give their aid if they knew in what way to employ it to the best purpose. As a nation we are apt to trust to individual rather than to combined efforts, and to waste much time and labour in consequence, with discouragement of many who would gladly share the labour in a scheme in which definite parts of the work could be undertaken by them.

I believe that a well-organised botanical survey of the British Islands would give results of great scientific value, and that there is need for it. I believe, also, that means exist to permit of its being carried through. There is no ground to expect that it will be undertaken on the same terms as the Geological Survey. A biological survey must be accomplished by voluntary effort, with possibly some help towards meeting necessary expenses of equipment from funds which are available for assistance in scientific research. Is such a survey not an object fully in accord with the objects for which the British Association exists? In the belief that it is so, I ask you to consider whether such a survey should not be undertaken; and, if you approve the proposal, I further ask that a committee be appointed to report on what steps should be taken towards organising such a survey, and preparing materials for a national flora of the British Islands.

British Association for the Advancement of Science.

SHEFFIELD, 1910.

ADDRESS TO THE EDUCATIONAL SCIENCE SECTION

BY

PRINCIPAL H. A. MIERS, M.A., D.Sc., F.R.S.,

PRESIDENT OF THE SECTION.

To preside over this Section is to incur a responsibility which I confess somewhat alarms me; for the President may, by virtue of his temporary office, be regarded as speaking with authority on the subjects with which he deals. Now, it is my desire to speak about University education, and for this purpose I must say something of school education; but I would have it understood that I really know little about the actual conduct of modern school teaching. One may read books which describe how it should be conducted, but this is a very different thing from seeing and hearing the teacher in his class; and I fear that personal recollections of what teaching in preparatory and public schools was like from thirty to forty years ago do not qualify one to pose as an intelligent critic of the methods which now prevail.

Human nature, however, has not changed much in the last forty years, and if, in considering the relations between University and school education, I can confine myself to general principles, based upon the difference between boys and men, I trust that I may not go far wrong.

I propose first to consider some general relations between teachers and their pupils, and then explain what, in my opinion, should be the change in the method of teaching, or at any rate in the attitude of teacher to pupil, which should take place when the scene changes from school to University.

First as to general relations between teachers and their pupils.

Educational systems necessarily prescribe the same methods for different teachers, and, being made for the mass, ignore the individual. But happily, in spite of the attempts to formulate methods of instruction and to make precise systems, there are many, and those perhaps some of the most successful, in the army of earnest school teachers who are elaborating their own methods.

Now among all the changes and varieties of system and curriculum there is one factor which remains permanent and which is universally confessed to be of paramount importance—the individuality of the teacher and his personal influence upon the pupil. It is therefore a healthy sign when school teachers who have been trained on one system begin to develop their own methods, for in this they are asserting their individuality and strengthening that personal influence which is the real animating of all successful education.

Personal influence is, of course, not only a matter of intellectual attainments; it appears to me, however, that at the present time so much is made of the duty

of schools to aim at the formation of character that there is an unfortunate tendency to regard this duty as something distinct from the other functions of a master, and as independent of intellectual qualifications. Among the first qualities now demanded of a master in a public school for boys are manliness, athletic skill, and a hearty and healthy personality, and these are often regarded as compensating for some lack of intellectual equipment. I suspect that there is a similar tendency in schools for girls. And yet I think it will be found that the only permanent personal influence is really wielded by teachers who exercise it through intellectual channels, and that those who acquire intellectual authority will generally succeed in training the characters as well as the minds of their pupils.

On the other hand, the master who is not up to the proper intellectual standard will soon be found out by his cleverer pupils, and will lose influence, whatever may be the charm of his character.

The formation of character, so far as it can be distinguished from intellectual training, is largely worked out by the boys themselves in any public school in which healthy tradition and a sound moral atmosphere are maintained, although it is true that these traditions depend upon the character and personality of the teachers.

The educational value of the personal and intimate association with one and the same teacher throughout the school or University career is officially recognised in the tutorial system at Eton, Oxford, and Cambridge. It has generally led to excellent results, provided that the tutor possesses the right qualities and that pupil and tutor do not happen to be two incompatible personalities; but the results may be well-nigh disastrous where there happens to be antagonism between the two, or where the tutor does not realise his opportunities and responsibilities. I have known some tutors who only excited a distaste for learning in their pupils, and others who entirely neglected or abused the high trust which had been committed to them; but far more, I am glad to say, who have not only exercised the most profound influence for good on their better and cleverer pupils, but also inspired intellectual interest in the most unpromising of them. Although such a tutorial system does not enter fully into the scheme of other schools and Universities, and therefore a student does not usually remain long under any one teacher, it must be within the experience of most persons to have come for a time at least under the influence of a teacher who has inspired real enthusiasm for learning and from whose lips the instruction, that might from others have been a trial, has become an intellectual treat.

It is given to comparatively few to exert this powerful and subtle influence in a high degree, for it is a gift confined to a few rare natures. All the more important is it, therefore, to ensure that an effective personal influence may play its part in the intercourse between ordinary teachers and ordinary pupils in the customary routine of school and University life.

How, then, is the proper personal and sympathetic relation to be established between teacher and pupil, so that the individuality of the one may call out the character and the effort of the other? Those who enquire of their earliest school reminiscences will probably recollect that the teachers who obtained a real hold upon them did so by virtue of the power which they possessed of arousing their intellectual interest. I would ask you for a moment to analyse the character of this interest.

In the young child I believe that it will be found to be mainly that of novelty: with him 'this way and that dividing the swift mind,' sustained thought, or even sustained attention, has not yet become possible; the inquisitive and acquisitive faculties are strong; and every new impression awakens the interest by its novelty quite apart from its purpose. You have only to watch and see how impossible it is for a young child to keep its attention fixed even upon a game such as cricket or football to realise how still more difficult it is to keep his attention fixed upon an intellectual purpose.

To quite young children, except to those who are unfortunately precocious, even an impending examination is not a permanent object of anxiety.

Now contrast the aimless interest which can be aroused in any young child's mind by the pleasure of a new impression, a new activity, or a new idea, with that which appeals, or should appeal, to the more mature intellect of an older student.

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With him it is not enough that the impression or the idea should be new; if it is to arouse interest it must also direct his mind to a purpose. This is to him the effective interest of his games or sport; in the game the desire to succeed or to win is the animating purpose, just as the expectation of catching a fish is the interest which keeps the angler's attention fixed for hours upon his line. In both the desire is fostered by the imagination, which maintains a definite purpose before the mind.

It is sometimes forgotten that as he grows the pupil is no longer 'an infant crying for the light,' but has become a man with 'splendid purpose in his eyes.'

While, therefore, it should be the aim of a teacher of young children to set before them the subjects of their lessons in an attractive manner, so that the novelty is never lost, and not to weary their active and restless minds with too sustained an effort, it should at a later stage be the teacher's aim to keep the object and purpose of the new fact or idea as constantly as possible in view, and not to distract the ardent mind with purposeless and disconnected scraps of learning.

I ask you to bear this distinction in mind, for it is a principle which may guide us in differentiating University methods from school methods of education.

The distinction need not involve us in a discussion of the 'Ziel-Angabe' in elementary education, for that is rather a question of keeping the interest alive during each lesson than of maintaining a permanent purpose in view throughout a course.

The much discussed Heuristic method as applied to very young children does, no doubt, fulfil this object so far as it provides the inquisitive mind with novelty instead of a set task, but so far as it makes the purpose more prominent than the process it may become a method more suited to the adolescent or the adult mind than to that of the young child.

I can fully realise that a most difficult and anxious time for the teacher must be that of the maturing intellect, in the interval between childhood and the close of the school career, when the method and spirit of the teaching must to some extent gradually change with the changing mental characteristics of the pupil. But, whatever may be the right methods of teaching children of ten and young men and women of twenty, many of our failures are due to one or both of two prevalent mistakes: the first, the mistake of teaching children by methods that are too advanced; the second, that of teaching University students by methods that are better adapted for school children. It is with the latter that I wish to deal in this address; but we may in passing remind ourselves that when young men and young women are sent straight from the University to teach children with nothing but their University experience to guide them, it is not surprising that they often proceed at first on wrong lines and as though they were dealing with University students.

The difficulty of divesting oneself of the mental attitude and the form of expression familiar in University circles, if one is to become intelligible even to the higher classes in a school, is betrayed by the unsatisfactory nature of many of the papers set by University examiners to school children. The teachers complain, and rightly complain, that there is often an academic style and form about them which just make them entirely unsuitable for the child.

It is, of course, hopeful that a diploma in pedagogy or some evidence that they have received instruction in method is now generally required of those who are to become teachers in schools. It seems to me, however, somewhat curious that, while efforts are now being made to give instruction in educational method to such persons, no similar effort is made to give instruction in more advanced methods to those who are called upon at the close of their undergraduate career to become University teachers, and that in consequence many of them have no method at all.

This may be a matter of comparatively small importance to those who possess not only the necessary knowledge, but also the natural gift of personal influence and the power of inspiring those whom they teach. But for those who are not blessed with these powers it may be almost as difficult to fall into the ways of successful University instruction after the sudden transformation from student into teacher as it is for those who become teachers in schools.

Granting, then, that there should be a radical difference between the ways of school and University teaching, and that there is at present an unfortunate overlapping between the two, let me next consider how the distinction between the intellectual interest of a child and the intellectual interest of a man may guide us in adjusting our methods of teaching when students pass from school to the University.

A tenable, perhaps even a prevalent, view concerning a liberal school education is that its chief purpose is not so much to impart knowledge as to train the mind; indeed, some teachers, influenced, perhaps, in the first instance by the views of Plato, go so far as to think that no subject which is clearly of direct practical use should be taught as such at school. This view they would carry to the extent of excluding many obviously appropriate subjects from the school curriculum, whereas almost any subject may be made an intellectual training; this being a question not of subject, but of the manner in which it is taught. In any event, if the scheme of intellectual training be adequately fulfilled, the period of mental discipline should come to an end with the close of school life, and the mind should then be able to enter upon new studies and to assimilate fresh knowledge without a prolonged continuation of preparatory courses. Indeed, the professed object of entrance examinations to the University is to exclude those whose minds are not prepared to benefit by a course of University study, and to admit only those who are sufficiently equipped by previous training to do so. An entrance examination then should not be merely a test of whether a boy or girl has learnt sufficient of certain subjects to continue those subjects in particular at the University; and yet it has unfortunately come to be regarded more and more as performing this function instead of being regarded as a test whether the student is generally fit to enter upon *any* University course. The result is that an entrance examination tends to become a test of knowledge rather than a test of general intelligence; merely one in an organised series of examinations which endeavour to ascertain the advancing proficiency in a limited number of subjects, and therefore tend really to encourage specialisation. Specialisation is not to be prevented by insisting on a considerable number of subjects, but rather by teaching even one subject in a wide spirit. Another result is that the entrance examination belongs properly neither to the school course nor to the University course; if it is taken at the age of sixteen the remainder of the school career tends to be devoted to University work, which should not really be done at school; if it is taken after leaving school this means that work is being done at, or in connection with, the University which ought to be done at school. It is certainly true that for various reasons a vast deal of education is now being carried on at the Universities which should belong to school life, and moreover is being carried on by methods which are identical with those pursued at school. It is equally true that, owing to the early age at which matriculation examinations or their equivalents may be taken, many schools are now asking that at the age of eighteen or nineteen a school examination may be held which shall be an equivalent not for matriculation, but for the first degree examination at the University. This would really imply that schools should be recognised as doing University work for two years of their pupils' careers--surely a most illogical procedure and one which supports my contention that there is now very serious overlapping, for it assumes that the work for the first degree examination can be carried on either at the school or the University, and therefore that there is no difference in the methods of the two.

An increasing number of candidates actually present themselves from secondary schools for the external intermediate examination of the University of London; in 1904 there were about 150, in 1909 there were nearly 500 such candidates.

There will always be exceptional boys and girls who reach a University standard, both of attainments and intelligence, long before they arrive at the ordinary school-leaving age. Let them either leave school and begin their University career early, or let them, if they remain at school, widen their knowledge by including subjects which are not supplied by the more rigid school curriculum designed for the average pupils; but let them not cease to be taught as school pupils. It is equally certain that there will also be boys and girls whose development is so slow that they barely reach the University standard when they leave school; yet some among them are the best possible material and achieve the greatest success in the end. For such persons an entrance examination will be

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required at the age of eighteen or nineteen; but I think it is unfortunate that this should be the same as that which quicker pupils can pass at the age of sixteen or seventeen, for an examination designed for the one age can scarcely be quite satisfactory for the other.

I confess that the whole matter is inextricably involved with the question of University entrance examinations. But to enter upon this here would carry us beyond the limits that I have laid down for myself, and it will be more profitable to decide what should be done at school and the University respectively before discussing how the examinations are to be adapted to our purpose. It will be sufficient for me to say that I have been led to the conclusion that matriculation examinations should be designed to suit the capacity of average pupils not less than seventeen years of age, if they are to test the intelligence of those who are ready to enter upon a University course.

Starting, then, with the principle that the period of mental discipline is closed at the end of the school career, and that those who pass to the University come with fair mental training and sufficient intelligence, let me inquire what should be the relation of University teaching to that which the student has received at school.

Under present conditions the schools which aim at sending students to the Universities endeavour to give a general education which will fit their pupils to enter either upon a University course or upon whatever profession or occupation they may select on leaving school. They do not confine the teaching of any pupil to preparation for a special profession or occupation, and they do not generally encourage special preparation for the University.

Now contrast what happens to the pupils leaving such a school to enter a profession or business with what happens to those who proceed to the University. The former pass into an entirely different atmosphere; they are no longer occupied with exercises and preparatory courses which serve a disciplinary purpose; they are brought face to face with the realities of their business or profession, and, though they have to gain their experience by beginning at the lower or more elementary stages, they do actually and at once take part in it.

The University student, on the other hand, too often continues what he did at school: he may attend lectures instead of the school class, but neither the method nor the material need differ much from what he has already done. Should not the break with school be as complete for him as for his school-fellow who goes into business? Should he not be brought face to face with the actualities of learning? After his years of preparation and mental drill at school should he not, under the direction of his University teachers, appreciate the purpose of his work and share the responsibility of it?

Let me take, as an illustration, the subject of History. A public school boy who comes to the University and takes up the study of history should learn at once how to use the original sources. It will, of course, be easier for him if he has learnt the rudiments of history and become interested in the subject at school; but, if he is really keen upon his University work, it should not be absolutely necessary for him to have learnt any history whatever. In any case, if he has received a good general education and has reached the standard of intelligence required for University work, he ought to be able to enter at once upon the intelligent study of history at first-hand; his teachers will make it their duty to show him how to do this; their lectures and seminars will illustrate the method of independent study, and will make the need of them clear to him. If, as is probable, some acquaintance with one or more foreign languages be necessary, he will take instruction in them as an essential part of his history course, in order that he may acquire the needful working knowledge; and to learn something of them with a definite purpose will be to him far more interesting and profitable than to study them only for linguistic training, as he would have been compelled to do at school. After all this is what would be done by his school-fellow who goes into business and finds it necessary, and probably also interesting, to acquire some knowledge of the particular foreign language required in the correspondence of his firm. It will, of course, be all the better for a University student of history to have acquired some training at school in the rudiments of history both ancient and modern, together with the knowledge of classics which is necessary for the former, and of modern languages which is necessary

for the latter. But there is not space in the school curriculum for all the subjects that may be required either for the University or for the business of life; the best that can be done is to give a good all-round training and to foster a marked taste or ability where it exists by allowing the boy or girl to include the subjects which are most congenial to them in the studies of their last two years of school life, as I have already suggested, provided that mere specialisation is not encouraged at school even towards the end of the school career.

The University course might then become a more complete specialisation, but of a broad character—the study of a special subject in its wider aspects, and with the help of all the other knowledge which may be necessary to that purpose.

The University teacher will also differ from the school teacher in his methods, for it will be his business not so much to teach history as to teach his pupil so to learn and study history as though it were his purpose to become an historian; in so doing he will have opportunities to explain his own views and to contrast them with those of other authorities, and so to express his individuality as a University teacher should.

One might choose any other subject as an illustration. In science there should be all the difference between the school exercises, on the one hand, which teach the pupil the methods of experiment, illustrate the principles laid down in his text-books, and exercise his mind in scientific reasoning, and, on the other hand, the University training, which sets him on a course involving the methods of the classical researches of great investigators and a study of the original papers in which they are contained, illuminated by the views of his own teacher. He also should awaken to the necessity of modern languages. A boy who, on leaving school, passes not to the scientific laboratories of a University, but to a scientific assistantship in a business or Government department, will very soon find it necessary to go to the original sources and acquire a working knowledge of foreign languages. It is regrettable that under existing conditions a scientific student sometimes passes through his University without acquiring even this necessary equipment. I believe this to be largely due to the fact that he is compelled to spend so much of his time in preparatory work of a school character during the early stages of his University career.

In the literary subjects, and especially in classics, there is, of course, not the same scope for the spirit of investigation which it is so easy to encourage in experimental science. Here the only new advances and discoveries which can appeal to the imagination in quite the same way are those which are being made every year in the field of archaeology, and it is therefore not surprising that this subject attracts many of the most ardent students: the methods of the archaeologist are more akin to those of the scientific investigator, and his work is accompanied by the same enthralling excitement of possible discovery. For the more able pupils and those who had a natural taste for language and literature no subjects have been more thoroughly and systematically taught for very many years at school, as well as at the University, than the classics; but for the less intellectual children or those who had no natural taste for such studies no methods could well be more unsuitable than those which used to prevail at schools. The grammatical rules and exceptions, the unintelligent and uncouth translation, the dry comparison of parallel passages, the mechanical construction of Greek and Latin verse, produced in many minds nothing but distaste for the finest literature that exists.

With the improved methods now in use Greek and Latin may be, and are, presented to the ordinary boy and girl as living literature and history, and school training in them may be made as interesting as anything else in the curriculum. Upon such a foundation the University should surely be able to build a course devoted to literary, philosophical, historical, or philological learning even for the average student, provided that the University teacher undertakes the task of helping his pupils to learn for themselves, and to pursue their studies with a purpose, not merely as a preparation.

The spirit of inquiry which drives the literary student to find for himself the meaning of an author by study and by comparison of the views of others is really the same spirit of inquiry which drives the scientific student to interpret an experiment, or the mathematical student to solve a problem. Only by kindling the spirit of inquiry can teaching of a real University character be carried on.

PRESIDENTIAL ADDRESS.

Give it what name you will, and exercise it in whatever manner you desire. there is no subject of study to which it cannot be applied, and there are no intelligent minds in which it cannot be excited.

The first question which a University teacher should ask himself is, 'Am I rousing a spirit of inquiry in my pupils?' And if this cannot be answered in the affirmative it is a confession that the University ideal is not being realised.

Some assert that this principle should also guide school education, and that it should be the first aim of the school teacher to stimulate the spirit of inquiry. My own view is that with young children this should be less necessary; they all possess it, and are by nature inquisitive. It should rather be the object of the teacher not to spoil the spirit of inquiry by allowing it to run riot, nor to stifle it by making the work uninteresting; if the lesson interests them, their inquisitive minds will be quick enough to assimilate the teaching. We are, in fact, brought back to what I have already emphasised—that the real difference between the inquisitive mind of the child and the inquiring mind of the adult is that the former is yearning for information quite regardless of what it may lead to, whereas the latter must learn or investigate with an object if the interest is to be excited and maintained.

I have often thought it an interesting parallel that among original investigators and researchers there are two quite distinct types of mind, which have achieved equally valuable results. There is the researcher who pursues an investigation with a constant purpose and to whom the purpose is the inspiration. But there is also the investigator who has preserved his youthful enthusiasm for novelty and has in some respects the mind of a child; passionately inquisitive, he will always seek to do something new, and very often, like a child, he will tire of a line of research in which he has made a discovery, and take up with equal enthusiasm a totally different problem in the hope of achieving new conquests. I think that a man well known in Sheffield, the late Henry Clifton Sorby, must have been a man of this character. The latter is, perhaps, the most fertile type of original investigator, but it is not the type that produces the best teacher, except for very exceptional and original-minded students; and such teachers do not often found a school of learning and research endowed with much stability. For ordinary students the investigator who pursues his researches as far as possible to their conclusion is the safer guide.

It seems to me suggestive that there are to be found, even amongst the famous researchers, these two types of mind, that somewhat correspond to the mental attitude of the school pupil and the University student. It is as though these great men have preserved a juvenile spirit, some from the days of their childhood, others from early manhood.

It will now be clear that the principle which I am advocating is a very simple one, namely, that the business of direct mental training should be finished at school, and that at the University the trained mind should be given material upon which to do responsible work in the spirit of inquiry. Preparatory exercises belong to school life and should be abandoned at the University.

All this seems so obvious that it might appear to be hardly worth saying were it not that the methods which actually prevail are so far removed from this ideal.

When, for example, a boy who has not learnt Greek or chemistry at school comes to the University and proposes to take up one of these subjects he is generally put through a course of exercises which differ in no essential respect from those which are set before a boy of twelve. In other words, our University method for the trained mind does not really differ from our school method, which is supposed to be adapted to the mind in course of training. Again, boys who have been learning certain subjects for years at school, but are weak in them, have their education continued at the University in the same subjects by the same school methods until they can be brought up to the requirements of a first University examination, which in its character does not differ much from the examination as held at school. Where in this process is to be found the introduction of that spirit of inquiry and investigation which ought to characterise the University course?

It may be a good idea, but in what manner is this change to be introduced, and how is it possible under present conditions, where so many students are all pursuing ordinary

degree courses and have no time or opportunity for special work, to provide teachers who can educate them in this spirit, if it is also their duty to get pass students through their examinations? The answer, I think, is that in a University the professors and higher teachers should be, without exception, men who, whatever may be their teaching duties, are also actively engaged in investigation. Their assistants should be teachers who, even if the whole or part of their time is occupied in routine teaching, have yet had some experience in, and possess real sympathy with, modern advanced work under such professors. This is only to be secured by insisting that teachers in a University should all have had some experience of original work, and, just as one of the necessary qualifications for an elementary teacher is some education in method, so a necessary qualification for a University teacher should be some education in research. Anyone desirous of qualifying for University teaching should be compelled to devote a certain portion of his student career to research, and the funds of a University cannot be better applied than to the retention of the better students at the University for the distinct purpose of enabling them to pursue investigation under the professor for a period of one year after they have completed their degree course, if they have not been able to do so during their undergraduate period. It is not, however, too much to hope that the majority of those who are endeavouring to qualify for the higher educational posts will be assisted to obtain this special experience during their degree course. Under the present system at most Universities, unless the student has been fortunate enough to come in contact with a teacher imbued with the spirit of research who is carrying on his own investigations, it rarely happens that he has the time or the means which would enable him to obtain any insight into the meaning of investigation before he leaves to take up teaching work. The need of post-graduate scholarships for this purpose is very widely felt, and is now frequently expressed. To insist upon such qualifications for all University students is, of course, under present conditions, impossible; but there should be no insuperable difficulty in insisting upon them for those who are to be allowed to enter a University as teachers.

Researchers are born, not made, and it is not by any means desirable that all University students should be cast adrift to make new researches and seek discoveries even under the direction of experienced teachers and investigators. This must depend to some extent upon the character of the pupil as well as of the teacher.

The mere publication of papers may mean nothing, and much that is dignified with the name of research is of no account. To turn a lad on to research, unless it be in the right spirit, may be only to set him a new exercise instead of an old one; to leave him to prosecute an investigation for himself may be to condemn him to disappointment and failure. On the other hand, to carry on any piece of work, whether it be new or old, in the zealous spirit of inquiry, with faith in a purpose, is to insure the intellectual interest of the student; and I cannot see why this spirit should not animate all University education, whether it be accompanied by original research or not. The essential condition is that the chief University teachers should themselves create an atmosphere of investigation.

So deep-seated is the belief that nothing must be undertaken without a preparatory course of training that even the best and most brilliant students are frequently discouraged from undertaking a new study until they have been subjected to the mental discipline of an elementary course in it.

I cannot refrain from quoting an example which came within my own experience, although I have already alluded to it in another address delivered last year.

When I was at Oxford a young Frenchman of exceptional ability, whose training had been almost exclusively literary and philosophical, and who was at the time engaged on a theological inquiry, expressed to me his regret that he had never learnt to understand by practical experience the meaning of scientific work. And when I assured him that nothing was easier than to acquire practical experience by taking up a piece of actual investigation under the direction of a scientific worker, he explained to me that when he had applied for admission to scientific laboratories he had been told that it was useless to do so until by preparatory courses he had acquired an adequate knowledge of mathematics, physics, and chemistry. I offered to make the trial with him, and began

with a problem that happened to interest me and that required a new method of simple experimental research. I soon found that a well-trained mind, able to grasp the meaning of the problem and eager to investigate it, could begin without delay upon the experiments, and in the desire to interpret them could find a pleasure and a purpose in seeking the necessary chemical and physical knowledge; whereas to have begun by acquiring this in a preparatory course, with no definite object in view, would have been to set back a mature mind to school methods of training and, very possibly to have stifled instead of kindling any real scientific interest.

This is, again, an illustration of my contention that the most special study, if carried on in the true University spirit, is very far removed from ordinary specialisation, and involves very wide extension of interest and learning; whereas, if carried on in a preparatory spirit, it is necessarily limited.

In a very short time this student had published three original papers which seem to me of considerable importance, though perhaps on a somewhat obscure subject, and I see that they are now quoted as marking a substantial advance in knowledge.

Of course this is the exceptional case of the exceptionally able student; but I think it illustrates two things—firstly, the prevalence of the conventional attitude that preparation on school lines is necessary even for the post-graduate student; secondly, the fact that what is really necessary to the University student is the purpose, and that with this before his eyes he may safely be introduced to new fields of work.

One result of the conventional attitude is that those who have distinguished themselves at school in some subject are often assumed to have a special aptitude in it, and to be destined by Nature to pursue the same subject at the University, whereas their school success may only prove that they are abler than their fellows, and that this ability will show itself in whatever subject they may take up. Such students would sometimes on coming to the University be all the better for a complete change of subject, without which the continuance of the school studies too often means a perpetuation of the school methods.

Another result is that when teachers are always playing a somewhat mechanical part in a systematised course, receiving duly prepared pupils and preparing them again for the next stage, such an atmosphere of preparation is produced that many persons continue to spend the greater part of their lives in preparation without any reasonable prospect of performance.

I am well aware that, on the other hand, there always have been and are now many earnest and accomplished University teachers who are pursuing the methods that I advocate, whose teaching is always inspired with a purpose, whose pupils are stimulated to learn in the spirit of inquiry, and who consequently exercise a personal influence that is profound and enduring. I am deeply conscious how much I owe to some such teachers with whom I have studied and to others whom I have known. But still it does remain true that this is not yet the atmosphere of ordinary University education, that it does not yet invigorate the ordinary University student, and that to him the passage from school to the University does not necessarily mean a transition from mental discipline and preparation to mental activity and performance.

The distinction that I have in my mind between University and school teaching may be expressed in this way. At school no subject should be taught to a class as though it were intended to be their life work; to take an example, it too often happens at present, owing really to excessive zeal on the part of school teachers, that mathematics is taught as though each member of the class were destined to become a mathematician; consequently only the few scholars with a real aptitude for mathematics become interested, and the remainder are left behind. On the other hand, at the University each subject should be studied as though it really were the life work both of teacher and student. Thus, to take the same subject as an illustration, the mathematical student will attend the full courses of his professors and will follow them with the interest of a mathematician; whereas for the scientific student it will only be in those branches of mathematics which concern him that the interest of his special science will put him on terms of equality with the mathematical student. If I may choose an illustration which is familiar to myself, any student of mineralogy can easily be

interested in and benefit by a course in spherical trigonometry, because it is one of the tools of his trade, but to send him to lectures on differential equations would be only to discourage him. On the other hand, the student of chemistry would rather be interested in the latter. To each of them certain branches of mathematics as taught by an ardent teacher afford a real intellectual training, but neither would gain much if compelled to follow a general University course of mathematics designed for mathematicians.

It will be observed that I have endeavoured to confine myself to the subject of University education and not to say much, except by way of contrast, concerning school teaching.

I must, however, return to it for a moment, if only to emphasise the danger of that specialisation, which, since it takes place at school and not at the University, is bound to be narrow, and which is often encouraged in pupils of special aptitude preparing for University scholarships.

That a boy or girl should for a year or even two years before leaving school be practically confined to one subject, and should before entering the University be examined in that alone, appears to me to be contrary to all the best traditions of school teaching, and to the often expressed desire of the Universities to insure a good general education in those whom they admit. There should, I think, be no scholarship examination which does not include several of the subjects of a normal school curriculum, however much additional weight may be given to any of them. Although it may be necessary that University entrance scholarships in one subject should be given either to encourage its study or to discover those who have a special aptitude, yet, so far as scholarships are intended to be rewards for intellectual pre-eminence, they should, I think, be directed to general capacity, and not be used as an encouragement to limited study. From what I have already said it will be clear that I do not attach much importance to special preparation at school for those who intend to proceed to the University. If a boy has a very special taste or aptitude, it should have abundant opportunity for displaying and exercising itself at the University, provided only that it has not been stifled, but has been given some encouragement in the school curriculum. I understand, for example, that those who teach such a subject as physiology at the University would prefer that their pupils should come to them from school with a general knowledge of chemistry and physics rather than that they should have received training in physiology. With the present modern differentiation into a classical and modern side, or their equivalents, the ordinary school subjects should be sufficient preparation for any University course if they are not mutually strangled in the pressure of an overcrowded curriculum.

To be fair, however, I must state another view. A very experienced college tutor who has had previous valuable experience as a master in a public school tells me that in his opinion the real problem of the public schools is the 'arrest of intellectual development that overtakes so many boys at about the age of sixteen.' 'There are few public schools,' he says, 'whose fifth forms are not full of boys of seventeen or eighteen, many of them perfectly orderly, well-mannered, reasonable, in some sense the salt of the place, exercising great influence in the school and exercising it well, with a high standard of public spirit, kindly, and straight-living, in whom, nevertheless, it is difficult to recognise the bright, intelligent, if not very industrious, child of two or three years before.'

He thinks that there is a real danger of degeneration at this age, owing, for one thing, to the manner in which the boys are educated *en bloc*: up to a certain age boys can be herded together and taught on the same lines without great harm being done, but after a certain time differentiation begins to set in. The school curriculum, however, does not admit of being adjusted to suit the dawning interests of a couple of hundred boys; and he sees no cure for this difficulty except a considerable increase in the staff and a corresponding reduction in the size of the forms. But he thinks that much may be done by an alteration in the system of matriculation examination, which sets the standard at the public schools. He would make this consist of two parts: an examination coming at about the age of sixteen and well within the reach of a boy of ordinary intelligence and industry, and comprising the ordinary subjects of school curriculum at this age; he would then let the boy leave the subjects from which he is not likely to get much further profit and begin to specialise for the remaining two

or three years, say, in two subjects, which would then be the material of the second examination. In this way they would make a wholly fresh start at a critical age, and he thinks that the bulk of the boys would probably find this a great advantage.

I quote this opinion because it shows that an experienced schoolmaster regards it as highly desirable that at a certain period in a schoolboy's career a real change should be made in his curriculum, and I have expressly stated that I find it difficult to express an opinion upon this particular educational period.

What should be the exact nature of the teaching before and after the age of sixteen or seventeen for the mass of ordinary boys I would prefer to leave to the decision of those who are best able to judge. I think it highly probable that there should be a considerable alteration of curriculum at the critical age. But, if a break and change of subject are required at this age, I believe that a yet more complete change is required at the later stage when the boy goes to the University, and that school methods should then be entirely replaced by University methods—not because there is then a natural change in the mental powers of the student, but because it is the obvious stage at which to make the change if we are to abandon preparatory training at all. Should it be proposed that the change ought to be made at sixteen, and that after that age something of the nature of University methods should be gradually introduced, my fear is that this would only lead to the perpetuation of school methods at the University.

An interesting question which deserves to be very seriously considered is the question, What sort of school education affords the best preparatory training for the University? I have often heard it asserted that, if a boy is capable of taking up at the University a course which is entirely different from his school course, he will generally be found to have come from the classical side and not from the modern side. An ordinary modern-side boy is rarely able to pursue profitably a literary career at the University, whereas it often happens that ordinary classical-side boys make excellent scientific students after they have left school. I am bound to say that this is, on the whole, my own experience. It suggests that a literary education at school is at present a better intellectual training for general University work than a scientific education. If this be so what is the reason?

There are no doubt many causes which may contribute. In some schools the brighter boys are still retained on the classical side while those who are more slow are left to find their way to other subjects; and some whose real tastes have been suppressed by the uniformity of the school curriculum turn with relief to new studies at the University and pursue them with zeal. But the facts do also, I think, point to some defect in the present teaching of school science whereby a certain narrowness and rigidity of mind are rendered possible. This may be partly due to the lack of human interest in the teaching of elementary science; the story of discovery has a personal side which is too much neglected, though it is more attractive to the beginner and might with advantage be used to give some insight into the working of the human mind and character. Moreover, it would form an introduction to the philosophy of science which is at present so strangely ignored by most teachers.

But another noteworthy defect is the absence of that mental exercise which is provided by the thoughtful use and analysis of language.

I believe that the practice of expressing thoughts in carefully chosen words, which forms so large a part of a good literary education, constitutes a mental training which can scarcely be surpassed, and it is unfortunately true that in the non-literary subjects too little attention is paid to this practice. In school work and examinations a pupil who appears to understand a problem is often allowed full credit, although his spoken or written answer may be far from clear. This is a great mistake. A statement which is not intelligibly expressed indicates some confusion of thought; and, if scientific teaching is to maintain its proper position as a mental training, far more attention must be paid to the cultivation of a lucid style in writing and speaking.

The various Universities seem fairly agreed upon the subjects which they regard as essential to an entrance examination—subjects which may be taken to imply the groundwork of a liberal education. Among these is English: and yet of all the subjects which children are taught at school there is none in

which such poor results are achieved. It may be taught by earnest and zealous teachers; the examination papers are searching, and seem to require a considerable knowledge of English literature and considerable skill in the manipulation of the language, and yet the fact remains that the power of simple intelligible expression is not one that is possessed by the average schoolboy and schoolgirl. It is the most necessary part of what should be an adequate equipment for the affairs of life whether the pupil passes to the University or not, and yet it is on the whole that which is least acquired.

Although it is true that the intelligent reading and study of literature mustery should assist in the acquisition of a good style, it is equally true that, if they come to be regarded as a school task, they are not viewed with affection, especially in these days of crowded curricula, when there is little leisure for the enjoyment of a book that requires delicate reading. If our modern strenuous curriculum of work and games has abolished the loiterer it has also abolished leisure, and has therefore removed one of the opportunities thus needed to exist for the cultivation of literary and artistic tastes and pursuits by those to whom they are congenial. The art of expressing one's ideas in simple, straightforward language is to be acquired not so much by study as by practice. There is no essential reason why children should write worse than they speak; they do so because they have constant practice in the one and little practice in the other. Our grandparents felt less difficulty in expressing themselves clearly than we do ourselves: of this their letters are evidence. It may have been partly due to the fact that they had more time and encouragement for leisurely reading, though they had not so much to read; but I believe that the letter, which they wrote as children were their real education in the art of writing English. Much would be gained if boys and girls were constantly required to express their own meaning in writing. The set essay and the *poems* play a small part, but do not do all that is needed. Translation does not give quite the necessary exercise. What is required is constant, with certain periods of conscious practice, and that is only to be obtained by making every piece of school work in which the English language is used an exercise in lucid expression. Very few paragraphs in anything written by the ordinary schoolboy, or, for the matter of that, by the ordinary educated Englishman are wholly intelligible, and teachers cannot devote too much pains to criticising all written work from this point of view. If we first learnt by practice to express our meaning clearly we should be more likely to acquire the graces of an elegant style later. I must add that I believe the training in the manipulation of words would be improved if all children were required to practise the writing of English verse, not in efforts to write poetry, but narrative verse used to express simple ideas in plain language, and I believe that this would enable them the better to appreciate poetry, the love of which is possibly now to some extent stifled by the pedantic study of beautiful poems treated as school tasks.

In such a subject as English composition, in which reform is so badly needed, something, perhaps, would be gained by an entire break with existing traditions: a break of the sort which would be required if it became suddenly necessary to provide for an entirely new type of student.

Now, there is one new and interesting development in which, for the first time, an opportunity offers itself of dealing with a body of students who, although possessed of more than average intelligence and enthusiasm, have not received the conventional training which leads to a University course. The tutorial classes for working people which have now been undertaken by several Universities, and which already number about 1,200 students, are attended by persons carefully selected for the purpose and anxious to pursue a continuous course of study of an advanced standard. In these classes the Universities will be compelled to begin new subjects for students of matured minds who have not received the usual preparation, and will therefore necessarily deal with them in a new way. Here, if anywhere, the difference between school methods of teaching and University methods ought to be apparent; and I feel sure that, if University teachers attempt conventional methods with these students, they will be condemned to failure. It is certain that these classes will increase enormously and rapidly, and I have great hope that they will for this reason influence the methods of University teaching in a very healthy manner. In the tutorial classes

the teachers will be confronted with the entirely new problem of students who have thought much, and of whom many are experienced speakers, well able to express their thoughts by the spoken word, but who, nevertheless, have received little training, and have had still less experience, in expressing their ideas in writing. Many of the students whom I have met have told me that this difficulty of writing is their real obstacle, and the matter in which they feel the want of experience most acutely. It will be a very valuable exercise for those who conduct these classes to instruct their students in the art of writing simple and intelligible English, and I hope that the necessity of giving this instruction will have a good effect upon the conventional methods of teaching English in schools as well as in Universities.

I am conscious that this address is lamentably incomplete in that it is concerned only with the manner of University teaching, and scarcely at all with its matter, and that, to carry any conviction, I should address myself to the task of working out in detail the suggestions that I have made. But this would lead me far beyond the limits of an address, and I am content to do little more than touch the fringe of the problem. Reduced to its simplest terms, this, like so many educational problems, involves an attempt to reconcile two more or less incompatible aims.

The acquisition of knowledge and the training of the mind are two inseparable aims of education, and yet it often appears difficult to provide adequately for the one without neglecting the other. If childhood is the time when systematic training is most desirable it is also the time when knowledge is most easily acquired; if early manhood is the time when special knowledge must be sought it is also the time when training for the special business of life is necessary. To withdraw from the child the opportunities of absorbing knowledge may be as harmful as it is unnatural; to turn a young man or young woman loose into a profession without proper preparation is cruel, and may be disastrous.

And so we get the battle of syllabus, time-table, scholarships, examinations, professional training, technical instruction, under all of which lies the disturbing distinction between training and knowledge.

But, if we inquire further into these matters, I think we shall find that the fundamental question is to a large extent one of responsibility. Left to himself, a boy or a man will acquire a knowledge of the things which interest him, even though they be only the arts of a pickpocket, and will obtain a training from experience such as no school or college can give. If education is to achieve the great purpose of interesting and instructing him while young in the right objects, and also of training him for the proper business of his life before it is too late, is it not mainly a question of deciding when and how far to take for him, or to leave to him, the responsibility of what he is to learn and how he is to learn it? If the teacher bears the responsibility during the period of school training, should not the student have a large share of responsibility in the quest of knowledge at the University?

Now, it is of the essence of responsibility that there should be something sudden and unexpected about it. If, before putting a young man into a position of trust, you lead him through a kindergarten preparation for it, in which he plays with the semblance before being admitted to the reality, if you teach him first all the rules and regulations which should prevent him from making a mistake, you will effectually smother his independence and stifle his initiative. But plunge him into a new experience and make him feel the responsibility of his position, and you will give him the impulse to learn his new duties and the opportunity to show his real powers. It is because I feel that this sudden entrance into an environment of new responsibility is so necessary that I would regard with suspicion any attempt to provide a gradual transition between school and University methods.

In matters of discipline and self-control it is possible and advisable to place responsibility upon school children; in intellectual matters it is not advisable, except for the few who are matured beyond their years. It is, therefore, all the more necessary that this should be done at the moment when they enter the University.

This should be the moment of which Emerson says: 'There is a time in every man's education when he arrives at the conviction that he must take him-

self for better or worse as his portion; that, though the wide universe is full of good, no kernel of nourishing corn can come to him but through his toil bestowed on that plot of ground which is given him to till. The power which resides in him is new in Nature, and none but he knows what that is which he can do, nor does he know until he has tried.'

The spirit of independent inquiry, which should dominate all University teaching and learning, is not to be measured, as I have already said, by the number of memoirs published, but it is to be tested by the extent to which University students are engaged upon work for which they feel a responsibility. Visit the Universities at the present moment, and, in spite of all the admirable investigation which is being carried on, you will find the majority of students engaged in exercises in which they feel no responsibility whatever. In my opinion this indicates that for them the spirit of true University education has never been awakened. It is, after all, very largely a question of attitude of mind. Any subject of study, whether it be a scientific experiment or an historical event, or the significance of a text, is a matter of interpretation, and to approach it in the University spirit is to approach it with the question, 'Is this the right interpretation?' Upon that question can be hung a whole philosophy of the subject, and from it can proceed a whole series of investigations: it embodies the true spirit of research and it opens the door to true learning.

In discussing University education I have not, of course, forgotten that many persons have taught themselves up to a University standard entirely without the aid of professors; indeed, the University of London long ago provided an avenue to a University degree which has been successfully followed by many such persons with the best possible results. But I have endeavoured to remind you that at the University as at school for most students the personal influence of the teacher is the important thing; that at the University as at school success in teaching depends mainly on the extent to which the interest of the student is aroused; and that at the University this is only to be done by providing him with a purpose and a responsibility in his work in order that he may understand to what conclusions it is leading him. Until this is done we shall still have University students complaining that they do not see the object of what they are learning or understand what it all means. This complaint, which I have often heard from past and present students of different Universities, suggested to me that I should on the present occasion deal with this defect in our customary methods.

In the hope that the attention of University teachers may be turned more fully to this aspect of their work I have ventured to make it the subject of my address.